

1: Editorial peer review: its development and rationale

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Editorial peer review has developed in a slow and haphazard way, and has only become truly institutionalised since the 1940s, mainly in response to the evolving complexity of the subject matter and concerns over quality of published material. Despite it being seen as a blunt, inefficient, and expensive tool, most of the scientists involved in peer review want to keep it, believing that other alternatives, such as audit, are worse. The popularity of peer review is growing and there have been four increasingly popular world congresses on the topic since the first in 1989. Most peer review systems and alternatives remain poorly studied. The popularity of peer review may follow from the way it democratises the process of scientific publication. Electronic publication is allowing journals to experiment with posting manuscripts on the web for open review, and affording rapid and easily accessible posting of criticisms to increase postpublication peer review. As a result, we can look forward to increasing transparency, and therefore increased utility in the peer review process, and, with transparency, an improvement in the ethical level of peer review.

The system whereby scientific manuscripts are reviewed by outside experts – editorial peer review – is generally regarded as an essential step prior to biomedical publication. Prominent journals owe much of their prestige to the fact that readers are aware that the editors take trouble to ensure such critical review, and in the nearly 300 years since journals began to use peer review¹ most biomedical journals have come to adopt the system. Editorial peer review, however, is arduous, expensive, often slow and subject to all sorts of bias, so why is there this almost universal acceptance of its necessity?

The evolution of editorial peer review

Early history of peer review

Kronick, in his essay on peer review in eighteenth-century scientific journalism,¹ pointed out that “peer review can be said to have existed ever since people began to identify and communicate what they

thought was new knowledge ... because peer review (whether it occurs before or after publication) is an essential and integral part of consensus building and is inherent and necessary to the growth of scientific knowledge". In the narrower sense of prepublication review, peer review seems to have begun in the early eighteenth century. Kronick cites as an example the first volume of the Royal Society of Edinburgh's *Medical Essays and Observations*, published in 1731. The preface describes the (anonymous) process: "Memoirs sent by correspondence are distributed according to the subject matter to those members who are most versed in these matters. The report of their identity is not known to the author." The Royal Society of London, when, in 1752, it took over responsibility for the *Philosophical Transactions*, set up a "Committee on Papers" to review articles, and the committee was empowered to call on "any other members of the Society who are knowing and well skilled in that particular branch of Science that shall happen to be the subject matter ..."¹

Denis de Sallo, the first editor of the first scientific journal, in the first issue of the *Journal des Scavans* in 1665, wrote: "we aim to report the ideas of others without guaranteeing them". In the same vein, the Literary and Philosophical Society of Manchester, in 1785, noted that "a majority of votes, delivered by ballot, is not an infallible test in literary or philosophical productions".¹ The Edinburgh Society's statement concludes:

The sanction which the Society gives to the work now published under its auspices, extends only to the novelty, ingenuity or importance of the several memoirs which it contains. Responsibility concerning the truth of facts, the soundness of reasoning, in the accuracy of calculations is wholly disclaimed: and must rest alone, on the knowledge, judgement, or ability of the authors who have respectfully furnished such communications.¹

It is clear, then, that systems of peer review, internal and external to journals, were put in place by editors during the eighteenth century in order to assist editors in the selection of manuscripts for publication. It was appreciated from the start that the peer review process could not authenticate or endorse because the editors and reviewers could not be at the scene of any crime.²

Commendably, the journals from the beginning threw the ultimate responsibility for the integrity of the article squarely upon the author. Peer review makes an assumption of honesty, and, though it can assist in establishing scientific validity, it cannot guarantee it.

Later history of peer review

Burnham, in a seminal paper,³ attempted to enumerate the influences on the evolution of peer review in the nineteenth and first

half of the twentieth centuries. He concluded on the basis of admittedly fragmentary evidence that the two main types of peer review – that is, of articles and of grant applications – developed independently of each other, and that editorial peer review developed in a most disorderly fashion. In particular, he noted that “crusading and colourful editors” like Thomas Wakley, who founded *The Lancet*, were like editors of newspapers, with little appreciation of peer review, and no incentive to use it, especially when they wrote much of their journal’s content themselves. Medical editors who followed the newspaper model had their scientific equivalent in the German professors who directed specialised academic institutes and edited journals that existed to publish the research done there.

Burnham showed that peer reviewing developed in response to increasing specialisation, but that in the absence of any systematic movement, it developed haphazardly. Those journals that used it at all produced their own versions, often dependent on the editor in charge at the time.³ Use of an editorial board did not constitute an intermediate step, and there were considerable differences between journals. In 1893, Ernest Hart, editor of the *British Medical Journal* from 1868 to 1898, described to the American Medical Editors’ Association in Milwaukee a very modern-sounding system of outside expert review, then in operation at his journal. When Hart told them how the *BMJ* referred every article to a recognised expert, he was clear about the work and expense involved:

It is a laborious and difficult method, involving heavy daily correspondence and constant vigilance to guard against personal eccentricity or prejudice or – that bugbear of journalism – unjustifiable censure. But that method may ... be recommended as one that gives authoritative accuracy, reality and trustworthiness to journalism.³

But Burnham could find no evidence whatsoever that the American editors had heard Hart’s message. Indeed, 50 years later, Morris Fishbein claimed that outside experts were consulted only very rarely at another large general medical journal, *JAMA*.³

Why the haphazard adoption of what seems to us to be as obviously good a step as it was to those who ran journals in the seventeenth and eighteenth centuries? And why the differences between journals? Burnham suggests several reasons. For a long time, the urgent need to fill their pages discouraged editors from adopting any rigorous (and expensive and time consuming) process of weeding out poor material. Moreover, in the United States, those with medical degrees were loth to admit they were not competent to handle anything in medicine. In addition, editors were under pressure to publish material sent in by members of the societies that owned the journals. Finally, their educational function, such as printing well written lectures, made

peer review a less important priority for editors. To these factors, I would add that it has taken a long time for medicine to become a science, and the necessity for review has become greater as manuscripts have moved from case history to randomised trial, and as journals have had to compete on the apparent quality of their content. The editors of journals that have very low acceptance rates, because authors compete for their pages,⁴ are grateful to rely on the discrimination of experts, and these are the journals that set the standards.

Institutionalisation of peer review

Peer review became institutionalised after the second world war, but again gradually and in no organised fashion.³ Robin Fox recalled that when Ian Munro took over as editor of *The Lancet* in 1976, “the buccaneering approach had to end. In the USA ... doctors were becoming reluctant even to cast an eye on research papers that did not bear the ‘pass’ sticker of peer review”.⁵ In other words, if *The Lancet* was to survive in the United States, it had to appear to have a serious approach to quality assurance. Despite the fact that it has come into common use, however, 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.

The modern history of peer review

Over the past dozen years, we have reached the stage of rational enquiry into the workings of editorial peer review. Though an important start had been made by the Coles with their examination of the peer review system of grants at the National Science Foundation (NSF),⁶ two independent events have changed the field. The first was the publication in 1985 of Stephen Lock’s ground breaking book on peer review, *A Difficult Balance*.⁷ The second was the decision by *JAMA*, in response to a commentary by Bailar and Patterson,⁸ to hold a conference to present research, as opposed to opinion, on editorial peer review. The conference was announced in 1986,⁹ and held in Chicago in 1989; 50 abstracts were received. Since then, three other peer review congresses have been held: in 1993, in Chicago, when 110 abstracts were submitted; in 1997, in Prague, to which 160 abstracts were submitted; and, in 2001, in Barcelona, when 180 abstracts were received.¹⁰⁻¹⁵ With the increase in numbers has come an increase in quality and sophistication.¹⁰ The four issues of *JAMA* devoted to these congresses have added materially to what we know about peer review, and a fifth congress is planned for 2005. These congresses have accomplished several goals. First, they have

stimulated research into what had previously been a black box, spoken of approvingly by editors (and scathingly by authors) – the vast majority, without facts to back up their assertions.⁹ At a growing number of journals, for example the *Annals of Internal Medicine*, the *Annals of Emergency Medicine*, the *British Medical Journal*, *JAMA*, *The Lancet*, and *Obstetrics and Gynecology*, research into editorial processes, ranging from simple audit to randomised trials of interventions, has become an accepted and expected editorial function and an important step in improving journal quality.⁴ The demonstration that journals can cooperate in multi-journal trials^{16,17} is particularly encouraging. We have gradually come to know more about the process by the steady accumulation of data and their careful analysis.¹⁸ As a result, it is becoming harder for editors to make *ex cathedra* pronouncements and easier for them to make statements based on evidence.¹⁹ Peer review, then, has itself come under searching scrutiny, and this in itself marks a considerable advance, even though the results may not have been encouraging.^{9,15,20}

Electronic review: Prepublication and postpublication peer review

Another, more recent, change in publication processes should benefit peer review. The electronic revolution is allowing authors increasing control over their publications, as they become their own typists and now their own typesetters and publishers.²¹ But, if scholarly publication is not to degenerate into some vast and chaotic chat page, formal review by peers will form an indispensable part of whatever systems of electronic publication of science prevail.^{21–23} Odlyzko favours a formal editorial and refereeing structure, with the reviewers' comments made public, grafted on top of an uncontrolled system in which preprints are submitted, and all comments, responses, and changes become part of the permanent public record.²¹ Harnad already practises a system with his journal, *Behavioral & Brain Sciences*, whereby, after formal review, articles are circulated to up to 100 "potential commentators", and the article is "copublished with the 20 to 30 (accepted) peer commentaries it elicits, plus the author's response ..."²² Bingham *et al.* have described a system for open review on the world wide web of articles for the *Medical Journal of Australia*.²⁴ Many other experiments are being undertaken,²³ most of them designed to increase the speed of review, and incorporate the unsolicited comments of readers: "postpublication peer review".^{23,25}

While the solicitation of reviewers' opinions by editors guarantees some comment and criticism for a manuscript, it will be unlikely that the editor will always have sent the manuscript to those particular researchers and others (including patients) most knowledgeable and

most capable of providing useful criticism. It is for this reason that criticism after publication, when every interested person will have an opportunity to provide it, is so important. The only mechanism until now has been the publication of letters to the editor. But the fact that these constitute an essential part of the peer review system has not been grasped or acknowledged. Most journals, if they publish letters at all, publish a fraction of those they receive and often many months after the original publication – a perfect way to discourage debate, incorporation of feedback and correction of the record – and attempts by databases to link the letters with the publication have been relatively recent. The internet, and the change in culture and readers' habits associated with it, are changing that. The *BMJ*, for example, is taking advantage of this change, and leading the way by putting almost all letters up on the website within 24 hours of receipt.^{26,27} This is an important initiative which I am sure other journals should and will imitate.

Electronic publication is affecting peer review in another important way. In 1994, the journal *Cardiovascular Research* published an article by Fabiato²⁸ on open peer review, that is, where the names of the reviewers are known to the authors. The article was published with responses from a number of independent critics. I was one of those who, with Stephen Lock and Richard Smith, former and current editors of the *BMJ* respectively, argued strenuously that there was no ethical justification for the closed, anonymous system of peer review.^{25,29} After all, the knowledge that their names will be disclosed to the authors and the public cannot fail to make reviewers more responsible in their comments.

Richard Smith, the pioneering editor of the *BMJ*, and his colleagues, began to open up prepublication peer review at their journal in 1999.³⁰ What is particularly refreshing about this initiative is that it was an editorial initiative actually based on evidence, much of it gathered for the peer review congresses. Furthermore, from the start, the *BMJ* was explicit that the new process would be subjected to research. They started by identifying reviewers to the authors. Having validated an instrument for measuring review quality³¹ they then showed that this, and letting coreviewers know each other's identities, did not affect the quality of reviews.^{32,33} They then demonstrated that revealing the reviewer's identity to the author did not affect the quality of the reviewer's opinion.³⁴ This resulted in the introduction of signed referees' opinions, which has not affected the quality of the opinions (Tony Delamothe, personal communication, 23 July 2002). These changes in such an important clinical journal, and the fact that they are based on evidence and have been studied carefully, represent the beginning of a revolution in peer review.

It is ironic that, at any rate in the United States, case law is supporting the confidentiality of the journal peer review system just

when a culture of criticism of peer review has been established, and cultural forces as well as electronic capabilities are moving us towards greater transparency of this system.^{25,35-38}

The rationale of peer review

In the most general sense, journal peer review is the formal expression of the principle that science works best in an environment of unrestrained criticism. The one thing that peer review does guarantee is the provision of that criticism and I regard this institutionalised criticism as one of the glories of science. It was a great disappointment, though scarcely a surprise, that no abstracts for any of the peer review congresses were received from the former Soviet bloc countries of eastern Europe, even though the 1997 peer review congress was held in Prague with the express purpose of stimulating peer review in countries where free criticism had previously been discouraged.

It is unlikely, however, that editorial peer review would have become so widespread merely to buttress a scientific principle. Peer review was set up as a quality assurance system.^{38,39} External peer review, which greatly broadens the expertise available to the editor, exists to help the editor (and therefore the authors, and science) to detect flaws⁴⁰ in order to select and improve the best manuscripts for publication. The broadening of expertise is required by editors dealing with increasing numbers of multi-authored manuscripts spanning several disciplines. There is now evidence that peer review improves manuscript quality⁴¹ and readability.⁴² But, as Jefferson and colleagues have shown in a systematic review, the effects of peer review are still uncertain.⁴³ Given the disparity between the apparent enthusiasm for peer review on the part of editors, and the data, it may be that researchers are studying the wrong factors.¹⁵

Advantages for the different players

There would seem to be advantages to all who are involved in any well run, smoothly functioning peer review system. Editors feel more comfortable in their decisions when they are informed by expert opinion, even if those decisions, given the frequent disagreement between reviewers, must inevitably offend some reviewers. Editors are aware that their journal's prestige rides to a great extent on the thoroughness and expertise of the reviewers. Editors are confirmed in their commitment to peer review when they see what happens when peer review is bypassed with, for example, the announcement of the apparent results of the early experiments on cold fusion at a press

conference, rather than in a scientific publication.⁴⁴ Reviewers appreciate being recognised as experts and drawn into the academic body. As reviewers discharge this academic duty, they learn about their subject and they learn about scientific criticism. Authors realise that the only hurdles worth jumping are those they respect: the hurdles erected by experts. Authors frequently acknowledge the assistance given to them by constructive reviewers. Readers have their task eased by the efforts of reviewers,⁴⁵ and are reassured by the seal they suppose peer review confers on published articles. Science in general benefits from careful, formal examination of the product. Given this idyllic picture, how could one ever question the rationale or existence of peer review?

What's wrong with this picture?

Several allegations are levelled against our present system of peer review. Peer review is a human activity: reviewers, like editors, may be partial, biased, jealous, ignorant, incompetent, malicious, corrupt, or incapacitated by conflicts of interest. And even if both editors and reviewers are competent, honest, and well intentioned, editorial considerations, such as lack of space, may serve to negate the effect of the best review.

The allegations take the following general forms.

- Peer review is unreliable, unfair, and fails to validate or authenticate. It is surprising, given its early history (above), that anyone should be so naive as to imagine that peer review could validate or authenticate scientific work, or guarantee its integrity.^{1,2} Unfortunately, many people confuse an effort to improve quality with the granting of a stamp of perfection, and sometimes journals are accused of taking advantage of this confusion,⁴⁶ in the face of ample evidence for the deficiencies of peer review.^{32,47–48} If reliability is equated with reviewer agreement, then numerous studies have shown the reliability of peer review to be poor.^{39,40} What this means, however, is unclear. Why should one necessarily expect agreement between a statistician and a cardiologist about the merits of a manuscript? The editor consults them because of, not in spite of, their different expertise, and expects differing opinions. Moreover, agreement does not guarantee validity, a far more important goal.^{39,40}

However, it is a reasonable aim that peer review should be fair, if fairness means an effort to overcome partiality and bias, and the editor behaves as more than a vote counter. Though Bailar⁴⁹ has numbered fairness among his inappropriate goals, I would merely say that we should do all we can to identify sources of bias and

remove them.⁵⁰ Unfortunately, a simple way to remove several biases, by masking reviewers to the identification of the authors, has recently been shown to vary widely between journals, the success rate being low^{16,17} and the effect being negligible.³² I am, however, encouraged by the finding, at one journal, *JAMA*, of no bias in favour of manuscripts with positive results.^{51,52}

- Peer review is unstandardised, and in the absence of clear standards and structure, is idiosyncratic, and open to every sort of bias.⁵³ We know that reviewers apply differing criteria, not least because standardised criteria have yet to be developed and tested.³⁹ Armstrong has summarised evidence to show that reviewers give excessive weight to “false cues”, being inappropriately impressed by statistical significance, large sample sizes, complex procedures, and obscure writing.⁴⁰
- Peer review secrecy leads to irresponsibility, insulates reviewers from unaccountability,²² and invites malice.^{2,25,29,46,54,55}
- Peer review stifles innovation, perpetuates the status quo and rewards the prominent.^{56,57} Peer review tends to block work that is either innovative or contrary to the reviewer’s perspective.^{2,57} Controversial work is more harshly reviewed³⁸ and Horrobin⁵⁶ has cited 18 cases where he believes innovation has been blocked by the peer review system.
- Peer review lends a spurious authority to reviewers. Reviewers’ anonymous opinions are set against the documented work of the author, and are given exaggerated weight by the editor who appointed the reviewers.²
- Peer review must fail because only reviewers close to the subject are knowledgeable enough to review, but these, being competitors, are disqualified by their conflict of interest.
- Peer review causes unnecessary delay in publication.
- Peer review is very expensive.⁴⁶
- Science is scarcely benefited because authors usually ignore reviewers’ comments if their manuscript has been rejected.⁴⁰
- Peer review is insufficiently tested. At the four congresses on peer review in biomedical publication^{12–14} evidence was advanced concerning usual forms of peer review, but not about other systems. No comparison has been made with any other intervention, which is odd, because peer review is indeed an intervention, and editors come down hard on authors who make assertions in the absence of comparative trials.

New burdens on the peer review system

Now that the editorial problem has turned from a search to fill empty pages to the selection of the best from an avalanche of

incoming manuscripts, editors have felt the need to refine their techniques of selection and rejection without good evidence as to the predictive powers of peer review to distinguish exactly between the “best” 10% and the next best 5%, a matter of great concern to authors submitting to journals with a 90% rejection rate.⁴ Yet despite its unknown accuracy, the test – editorial peer review – is being asked to bear further burdens. The career consequences to authors of publication in prestigious journals can be considerable, since academic advancement is closely linked to an individual’s publication record. This linkage has raised the stakes for researchers, and the pressure they bring to bear on editors, particularly on editors of large circulation, highly visible journals, has correspondingly increased. In addition, peer review is one of the entry criteria used by indexing services,⁴ and the provision of “rigorous peer review” is a prominent part of the advertising of most journals. Altman makes the point that newsworthy journals justify their news embargoes and rules about prior publication on the basis of peer review, though it is known to be a poor validating device.⁴⁶ Finally, Noah⁵⁸ has drawn attention to the fact that dependence on anonymous journal peer reviewers, in the United States at least, is threatening to pre-empt careful review by governmental agencies of the evidence needed before new legislation is proposed.

So many problems, but more and more popular

Given all these problems, how can peer review be growing in popularity? My guess is that editorial peer review is seen by investigators and research institutions as a convenient quality control mechanism, for which they usually do not have to pay. Despite the growing evidence that peer review is a blunt, inefficient, and expensive tool, most of those involved want to keep it, believing that other alternatives, such as audit, are worse. Reviewers frequently do spot poor design, methodological flaws, internal inconsistencies, unbalanced citation, distorted analyses, and so on. And even if studies have shown that they often do not, peer review seems to be better than nothing.

Democratisation of the process

Peer review is spreading, but not merely because it is a marketing tool for journals trying to pretend that their quality control is tight. Why then? Many editors, trying to justify the complex system that keeps them in business, make the analogy with democracy. They cite Winston Churchill: “it has been said that democracy is the worst form of government except all those other forms that have been tried from time to time” (Winston Churchill. House of Commons, 11 November 1947).⁵⁹

So why do editors make the analogy of peer review with democracy? What editors are really agreeing to is that peer review is democracy, because editors like the comfort of having experts, and unnamed ones at that, shoulder the blame for the unpleasant editorial tasks of actually having to make decisions, and actually having to take responsibility for those decisions. At the same time, authors like the assurance that at any rate some outside experts were called in to moderate the arbitrary decisions of a few power-crazed ignoramuses like myself: authors like decisions by jury rather than by judge. And peer reviewers like the compliment being paid to them in being asked to be included as editors by proxy; they like being included in the body academic; they like being privy to the latest work of their competitors; they enjoy the illusion of power and they like having a vote. Finally, readers, who are the majority voters in this democracy, are reassured to find choices made for them as they wade through the information avalanche. This is, I think, a portion of what Kronick meant when he wrote that peer review was “an essential and integral part of consensus building and inherent and necessary to the growth of scientific knowledge”.¹ It is therefore no surprise at all that as the evidence of its flaws and inefficiencies accumulates,^{1,2,4,10-14,39,40} peer review, far from foundering as it hits iceberg after iceberg, shrugs them off and sails proudly on.

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