Shifting power relations and the ethics of journal peer review

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ABSTRACT

**Background:** Peer-review of manuscripts has recently become a subject of academic research and ethical debate. Critics of the review process argue that it is a means by which powerful members of the scientific community maintain their power, and achieve their personal and communal aspirations, often at others’ expense. This qualitative study aimed to generate a rich, empirically-grounded understanding of the process of manuscript review, with a view to informing strategies to improve the review process.

**Method:** Open-ended interviews were carried out with 35 journal editors and peer reviewers in the UK, USA and Australia.

**Results:** It is clear from this research that relations of power and epistemic authority in manuscript review are complex and dynamic, may have positive and negative features and that even where power is experienced as controlling, restrictive and illegitimate, it can also be resisted.

**Conclusions:** The manuscript review process is best thought of not in terms of simple dominance of reviewers and editors over authors, but rather as a shifting ‘net’ of power relations. These complex power relations need to be understood if reviewers are to be encouraged to participate in the process and to do so in the the most ethical and effective manner.

BACKGROUND AND RATIONALE

The ‘trouble with journal peer review’

Peer review of biomedical research is generally seen to serve two important functions: 1) ensuring that manuscripts are improved and that only high quality research is published and 2)
ensuring that important findings are disseminated to other researchers and practitioners and, perhaps, to the general public and political bodies. While there is broad acceptance that some form of pre-publication review is essential, there is also broad agreement that manuscript review is often unsuccessful in achieving its goals. Richard Smith, ex-editor of the British Medical Journal, recently summed up his view of the situation as follows:

We have little evidence on the effectiveness of peer review, but we have considerable evidence on its defects. In addition to being poor at detecting gross defects and almost useless for detecting fraud it is slow, expensive, profligate of academic time, highly subjective, something of a lottery, prone to bias, and easily abused (Smith 2006, 89).

As is evident in Smith’s comments, reviewers are often criticised for their perceived incapacity to detect instances of scientific misconduct such as fraud, plagiarism, repetitive publication and author conflict of interest (Rennie 2003; Smith 2006). On the other hand, manuscript review is also widely criticised for being anti-innovative and non-constructive and for causing unacceptable delays in publishing (Horrobin 1990; Rennie 2003). As Atkinson argues:

It is clear that editorial regulation leads to conservatism. For the innovators—one whom progress is dependent—time and effort are wasted in repeated attempts to communicate. For their fellows, work continues on possibly superseded lines (Atkinson 2001, 198).

Manuscript review is, therefore, seen to fail both in its capacity to ensure the quality of published work, and in its capacity to facilitate the dissemination of ideas to other researchers and to clinicians.

**Manuscript review as an abuse of power**

Those who are critical of the manuscript review process frequently argue that the review process is a means by which powerful members of the scientific community (i.e. reviewers and editors) maintain their power, and achieve their personal and communal aspirations, often at others’ (i.e. authors’) expense (Atkinson 1994; Judson 1994; Godlee 2000; Atkinson 2001; Rennie 2003; Wager and Herxheimer 2003). In explaining the cause of excessive conservatism, for example, some emphasise that those with power have a vested interest in preserving the scientific status quo and that those with unconventional ideas face such hostility that they are likely to conceal these ideas in both their research and their writing in order to be able to compete for resources (Atkinson 2001; Spier 2002; Hojat, Gonnella and Caelleigh 2003; Rennie 2003).

Many critics of peer review therefore challenge the view that peer reviewers act as an impartial jury whose specific role is to overcome the unconscious or arbitrary abuse of power (Atkinson 1994; Eisenhart 2002). Rather, the focus is on the capacity of those with the judge-like ‘power of print’ (Crigger 1998, 453) to impose their opinions on others; assign status to selected information; govern what counts as truth and orthodoxy; and even bring particular researchers, research communities and academic disciplines into existence or non-existence by legitimating or challenging their identities (Crane 1972; Judson 1994; Cain 1999; Chubin 2002; Larochelle
and Désautels 2002; Osborne and Brady 2002; Roth 2002; Tobin 2002; Tobin and Roth 2002; Theilheimer 2003; Kumashiro 2005; Luke and Luke 2005; Scheurich 2005). Indeed, professional recognition in the form of peer reviewed publications has been seen by some as the principal instrument of social control within the scientific community (Godlee 2000; Callaham, Wears and Weber 2002; Parrish and Bruns 2002; Fletcher and Fletcher 2003; Ginsparg 2003; Davidoff 2004; Cheek, Garnham and Quan 2006). At the professional level, peer review processes are seen as a form of visible self-regulation that allows scientific and medical groups to demonstrate that their professional power and their shielding from political, administrative and public interference is warranted (Freidson 1986; Judson 1994; Crigger 1998; Roth 2002; Emanuel and Greenland 2005). Perceived abuses of this power have, in turn, been attributed to a wide variety of personal vices on the parts of journal editors and peer reviewers. Greed, jealousy, egoism, favouritism, malice, caprice, lying, unscrupulousness, abuse of privilege, strategic manoeuvering, self-promotion, disingenuity, predatory behaviour and corruption are just some of the accusations that have been levelled at editors and reviewers (Osmond 1983; Judson 1994; Horrobin 1996; Smith 1997; Godlee 2000; Horton 2000; Eisenhart 2002; McCrory 2002; Kumashiro 2005; Lagendijk 2005; Farthing 2006).

This is not to say that reviewers’ and editors’ power is invariably viewed in negative terms. A recognition of the potential for benign use of power is evident, for example, in the suggestion that reviewers should be able to identify authors (as is typical in some disciplines) in order to allow reviewers to give special consideration, and relatively more assistance, to junior academics or those writing in a second language (Fletcher and Fletcher 2003) or to allow reviewers to voluntarily withdraw from the process if they find themselves unable to review a particular author’s work in an impartial manner (Morrison 2006). The critical discourse on power in peer review is also qualified by the recognition that reviewers’ and editors’ power is not absolute. First, it is frequently noted, particularly in the context of debates about reviewer anonymity, that (junior) reviewers may at times hold less power than (senior) authors, who may subsequently retaliate (Godlee 2002; Fletcher and Fletcher 2003). It is also recognised that even a relatively powerful reviewer or editor is him/herself subject to academic competition and demands for publication (Ingelfinger 1974; Horrobin 1990; Judson 1994; Cain 1999; Rennie 2003; Emanuel and Greenland 2005; Kumashiro 2005). Finally, the critical discourse is occasionally qualified by the recognition that power may shift over time, with once marginalised academic fields becoming increasingly powerful through, for example, the long term dynamics of cultural enactment and production (Tobin and Roth 2002; Luke and Luke 2005).

For the most part, however, analysis of power in manuscript review tends to equate it with reviewer’ and editors’ dominance over authors and views power in negative terms. This has led to the elucidation of a number of strategies for ‘dealing with’ power imbalances in manuscript review, including: insisting that reviewers and editors declare their conflicts of interest, both financial and non-financial (epistemological, methodological, ideological, pedagogical) (Godlee 2000; Hojat, Gonnella et al. 2003; Rennie 2003; Kumashiro 2005); allowing authors to exclude potentially biased and hostile reviewers (Mruck and Mey 2002); having reviewers sign their reviews and publishing reviews for broader scrutiny (Bingham and van der Weyden 1998). The idea here is that rather than promoting integrity, allowing reviewers to act ‘under the cloak of
anonymity' (Godlee 2002, 2762) insulates reviewers from accountability, leading to laziness, irresponsibility, failure to suppress whims and self-interests, and even to outright abuses of power (Altman 1996; Rennie 1998; Atkinson 2001; Gannon 2001; Baez 2002; Rennie 2003). There is also the broader socio-political argument that, all else being equal from a quality point of view, reviewers should be identified and should be made to declare biases and conflicts of interest because justice needs to be seen to be done and because power without responsibility should not be tolerated (Godlee 2000; Godlee 2002; Hojat, Gonnella et al. 2003; Smith 2003).

The desire to contain reviewers’ and editors’ power is also one of the main reasons for the increasing emphasis on external oversight of the review process, in the form of regulatory bodies and journal ‘ombudsmen’ (Horton 1998; Cain 1999; Godlee 2000; Roth 2002). Without external oversight, it is argued, we are left with a dangerous situation in which the editor, working in secrecy, is ‘investigator, judge and jury in his or her own journal’s case’ (Rennie 2003, 127) and in which editors and reviewers become ‘the sole guardians of standards within research publication’ (Brice and Bligh 2005, 88). For some, this situation is untenable and demands a radical rethinking of the relationship between journals, journal editors, academics, political organisations and the commercial sector (Farthing 2006). It is argued, for example, that in the name of democratisation and a challenge to established authority, the ultimate interpretation and evaluation of science should be in the hands of readers rather than reviewers, freeing publication from the preferences of dominant individuals and making it answerable to the peer community as a whole (Horrobin 1996; Eisenhart 2002; McCrory 2002; Rennie 2003). Even if pre-publication review is not to be replaced entirely, it is argued that editors should at least reconceptualise the place of external review in the overall publishing process, seeing reviewers as consultants who can provide second opinions and a rich array of insights, rather than as often highly conflicted and very conservative referees who, on their own, decide what should and should not be published (Horrobin 1990; Atkinson 2001; Fletcher and Fletcher 2003; Rennie 2003).

To date none of these ‘power-management’ strategies has been adopted consistently and there remains a strong sense that manuscript review is still a process in which certain voices are excluded (Kumashiro 2005), that assessment of scientific papers is ‘little more advanced than trial by battle’ (Atkinson 1994, 148) and that the major impact of the review process is to censor debate and innovation, perpetuate the status quo and reward the powerful (Horrobin 1990; Atkinson 2001; Horrobin 2001; Eisenhart 2002; Rennie 2003).

**Justification for a qualitative empirical study of manuscript review**

Some argue that these abuses of power and other failings of manuscript review (e.g. lack of reviewer expertise) are so serious that pre-publication review should be done away with completely (Smith 2003). Most people within the biomedical community appear, however, to want to retain pre-publication review but work to improve it through systematic study. This has resulted in a large body of empirical research into the process of peer review, the limitations of peer review, and ways in which the process might be improved (e.g. anonymisation, signing reviewers, training, feedback, rewards) (Jefferson, Rudin, Brodney Folse et al. 2007). Despite
these efforts, a recent Cochrane review of studies of manuscript review concluded that there remains a need for an extensive multicentre programme of empirical research, cautioning:

Until such research is undertaken, peer review should be regarded as a long-standing, potentially expensive, untested process with uncertain outcomes. (Jefferson, Rudin et al. 2007, 13).

One possible explanation for the limited impact of existing studies is that they do not adequately account for the social and subjective dimensions of the process, thus limiting the critical discourse surrounding the process, and the range of possible political and institutional responses (Kassirer and Campion 1994). In this regard, it is worth noting that there are a small number of studies which have focused on the nature of manuscript review as a discursive process (Stamps 1997; Kourilova 1998; Oliver 2001; Gosden 2002; Gosden 2003; Hewings 2004) and have been justified on the basis that:

Analysis of the language of peer review can reveal insights into the relationship between reviewers as gatekeepers of standards and conventions within a particular discipline, and writers who seek to convince that they are able to produce writing whose content and presentation conform to the communication conventions established by the discipline. (Hewings 2004, 249).

These linguistic studies of manuscript review are embedded in a larger body of literature examining scientific research writing (e.g. Bazerman 1988; Dear 1991; Halliday and Martin 1993; Atkinson 1999) which have highlighted the importance of the symbolic means used by scientists to express themselves scientifically, and to maintain the norms and boundaries of their communities (Atkinson 1999; Canagarajah 2002; Casanave and Vandrick 2003). All of these studies are ‘critical’ in the sense that they see manuscript review as a highly contextualised and social process, rather than as a depersonalised, decontextualised extension of science, carried out using scientific methods in order to achieve scientific goals. But, in biomedicine at least, such studies are rare.

Moreover, almost all published research into biomedical manuscript review has been quantitative and reductionist—reducing the process of review to its component parts and studying the effects of various technical interventions on each of these parts, thus limiting our ability to understand, and manage, review as a complex whole (Atkinson 2001; Callaham and Tercier 2007). Insofar as qualitative research has been conducted into biomedical manuscript review, this has focused largely on the criteria used by reviewers in assessing manuscripts (Dickersin, Ssemmanda, Mansell et al. 2007) and on specific stakeholder preferences (e.g. Kearney and Freda 2005), rather than on the psycho-social underpinnings of the process.

This study therefore aimed to use qualitative methods to generate a detailed understanding of the biomedical manuscript review process as it occurs and is described by participants. In particular, the goal was to inductively characterise the most salient social and subjective dimensions of the biomedical manuscript review process, and to examine biomedical editors’ and peer reviewers’ understanding and experience of these dimensions of the process. We emphasise that this was not set up as a study of power relations or any other pre-defined topic,
and we made an effort (to the extent that this is possible) to set aside any preconceptions as to what editors and reviewers would say.

METHOD

Purposive sampling was used because the aim of this project was to enable conceptual exploration and theory generation rather than population representativeness. Thirty-five open-ended interviews were carried out. Twenty-three of the interviewees were current or past editors at major general medical journals in the UK, USA and Australia (21 of whom were full-time editors, including 3 editors-in-chief (1 retired) and 2 of whom part-time editors who also do clinical work). Nine were current or past part-time (usually unpaid) editors of specialty journals based in Australia, the UK and the USA. Attempts were made to vary the sample as much as possible and interviews were carried out with editors of different ages, from a variety of professional backgrounds (e.g. clinical, research, other publishing) and from a variety of biomedical disciplines/journals including basic science, clinical/translational science, public health and health-related philosophical and social research. Of these editors, the majority had been, or were currently, engaged in research, academic writing and/or peer reviewing (as distinct, for example, from moving into editorial work from journal production/copy editing, purely clinical roles or junior research roles without reviewing responsibilities). Although the focus of this study was on editors of subscription-based peer-reviewed journals publishing primary research and commentary, we also interviewed three current or past editors of ‘review’ journals (i.e. journals publishing review articles, systematic reviews and/or meta-analyses), two past editors of non-peer reviewed medical journals, one editor of an open-access journal and two people who had been reviewers but not editors. It is important to note that our purpose in varying the sample was not to examine sub-groups in depth or to identify subtle differences between groups, but rather to ensure that we were not missing any major issues that might be obscured by the experience of editing ‘mainstream’ biomedical journals.

The interviews lasted approximately one hour and were unstructured, which allowed participants to define and discuss manuscript review as they wished. In each case, participants were simply asked to describe their career paths and their experiences of acting as editors and/or peer reviewers. While we had prepared an interview schedule, our research participants were extremely articulate and it was seldom necessary to make use of these prompts. Instead, the interviews took the form of a conversation in which emergent themes were clarified and followed up. Interviews were carried out by one researcher (WL) and were recorded and transcribed verbatim.

The method of data analysis drew on both Morse’s outline of the cognitive basis of qualitative research (Morse 1994) and Charmaz’s outline of data analysis in Grounded Theory (Charmaz 2006), and involved: initial coding using Charmaz’s method of line-by-line analysis and ‘gerunding’ (encoding action or process); synthesis of codes into categories; focused coding using these categories; and abstraction into concepts (Charmazs’ ‘analytic categories’). A coding tree was generated using the qualitative research software NVivo 7. Throughout the data analysis process, a process of constant comparison was employed. Existing codes, categories and concepts were constantly refined, enriched and reorganised as new codes, categories and
concepts were developed, or as similarities and differences among existing codes, categories and concepts were recognised. Enough material was analysed to ensure that categories were saturated, that is, that all codes appeared to fit under one or more existing categories, and all analytic categories were fully described and well-understood. We emphasise that we did not initially “read for” any particular themes. The initial inductive phase of data analysis enabled the material to be organised under five broad headings:

1. Manuscript review as a “scientific” (objective, methodical, reproducible process)
2. Manuscript review as a moral enterprise
3. Power and vulnerability in manuscript review
4. Authority and deference in manuscript review
5. Intuition and prejudice in manuscript review.

Once these broad categories had been identified we focused on each category in turn: First, we searched the background literature on peer review to find discussions and/or other empirical research relating to each category (the material we found on power and authority is summarized in the introduction to this article). We then returned to the data and “read for” each category in order to elucidate the findings in more detail and consider them in relation to the background literature. In this way, our developing understanding of manuscript review was continually and iteratively informed by existing scholarly work. This is consistent with Peirce’s process of abduction (Peirce 1958), recognised as part of the development process in qualitative analysis (Reichertz 2007).

The findings relating to power and authority are presented here.

This study was approved by the [NAME OF INSTITUTION REMOVED] Human Research Ethics Committee and by the editors-in-chief of all of the involved journals. All names used here are pseudonyms (only gender has been retained).

RESULTS

Part 1: Relations of power and vulnerability

Editors’ and reviewers’ power over authors

Many interviewees saw the overall power relation in manuscript review as involving an editor and/or reviewer’s exercise of power over a more vulnerable author. Several participants, reflecting upon their own experience of being an author, noted the acute pain associated with rejection and the sense of helplessness that the review process can induce:

As a researcher I always kind of felt like I was sort of flopping around at the end of this line, sometimes at the whim of reviewers in some cases. A lot of it, it seemed mystical. Sometimes it worked out well, and sometimes ultimately things were rejected, and to
me it was never predictable what was going to happen. It kind of felt like there was an arbitrary process going on. [Glen, previously an author and reviewer, now a full-time editor]

In addition to causing a sense of helplessness, the experience of rejection was described as profoundly demoralising and threatening to an author’s very identity as a scientist and member of an academic community. Rejection of a manuscript was often likened to rejection of an artwork or even one’s child:

When one of your major papers is finally rejected, it’s extraordinarily identity challenging, and diminishing. It’s very like the artist putting his work out on public show and the public turning their backs and the critics saying what crap this is. This is what somebody is all about. [Louis, author, reviewer and part-time specialty journal editor]

The manuscripts are the intellectual children of the authors. And for somebody to tell you, ‘I’m sorry, but your child is just not good enough’, that is a very painful experience. [Gavin, full-time editor]

Authors’ resistance to editorial/reviewer power

Not all participants, however, saw authors as completely disempowered, and several participants described strategies that they, as authors, had used in order to make themselves less vulnerable, including: writing in a manner that conforms to the dominant scientific paradigm (so as to make rejection less likely); preparing themselves emotionally for rejections (in particular trying to take rejections less personally); recognising that academic writing is simply a ‘performance’ which doesn’t require actual compromise of one’s integrity as a scientist; learning to appreciate the guidance provided by even negative reviews, and even removing themselves from mainstream science:

You have to be fairly streetwise to survive (outside the system), but people do. I think it depends a bit on what motives people have for doing science. I never really set out to be an academic. I have never seen the end product of science as being manuscripts, publications and successful grant applications. I couldn’t see why one should be bothered to send stuff to people who weren’t even interested in reading it. That’s a slightly masochistic approach to things, because people say: “You’ve GOT to publish in the big journals because that’s how you get forward. [Eric, scientist, reviewer and specialty journal editor].

Authors’ power over editors and reviewers

Power relations were seen to be complicated not only by authors’ capacity to modulate their own dependence and vulnerability, but also by the fact that there are situations in which an author may be truly more powerful than a reviewer or editor. Most obviously, there was the recognition that negative reviews might prompt disgruntled authors to later ‘return the favour’. This sense of vulnerability seemed particularly acute for junior reviewers who were aware that
they may be dependent upon more senior scientists in the future for references, employment and funding opportunities—something that was used to justify anonymous reviewing:

It’s very difficult to criticise someone who is going to be looking at your grant application. For a young researcher that’s pretty much impossible. [Brian: full-time editor]

Because it was such a small community, it was probably important to protect reviewers by de-identifying both ends of the process, so as to enable a reviewer if they were relatively junior to be able to say “this paper is crap and for these reasons”. You might want to use fairly junior people, junior people are often very good reviewers, (but) they might not like to be seen picking off the high flyers. [Cherie, full-time editor]

But it was not only junior reviewers who felt disempowered in this way. Senior specialty journal editors (who were also members of the scientific community) described similar concerns:

You don’t want to be nasty to people. And especially if it’s a specialty journal, inevitably everybody knows everybody else. It’s not like you’re reviewing something and you’ll never meet this other person. So it all gets really unpleasant. [Sarah, specialty journal editor]

In addition to having this potential power over external reviewers, authors were also seen to have power over journal editors. This power was seen to stem primarily from the fact that all journals need submissions, and authors—though relatively powerless once a manuscript has been submitted—have the power to withhold submission from a particular journal. This was described as being particularly problematic for editors of less prestigious journals who find themselves in the vicious circle of needing to increase submissions in order to improve their impact factors and thus attract more authors:

When you are looking at a journal that’s struggled to get an impact factor of 1.2 (laughs), you know we’re not talking about terribly high quality at all! The first day that I get a randomised controlled trial sent to my journal I think I’m going to break open a bottle of champagne because I haven’t even been sent one, because nobody would—I wouldn’t send myself an RCT, I’d be trying to send it to a better journal (laughs). That’s the problem. That’s why the impact factor is the killer. I’m trying to drag this thing up, because I know that it’s like this vicious circle. [Sarah, specialty journal editor]

To attract authors, editors of less prestigious journals spoke of the need to work harder to improve manuscripts since they did not have the liberty of using rejection of manuscripts as a quality control mechanism, and needed to be more careful not to disenfranchise potential future authors.

While editors of elite journals described themselves as relatively less disempowered (having at least the capacity to engage proactively with potentially cooperative authors and the resources treat their authors well), even these editors spoke about the need to actively solicit manuscripts as even their journals existed in a ‘competitive environment’ in which each ‘top tier’ journal needed to win over authors who may choose instead to publish in one of the equally high-ranking competitors:
We are a bit elitist because we can afford to be elitist. You need [elite journal name] publication more than we need your paper, so do what you want, or publish elsewhere. It’s not an issue to us. But when it comes to there being a good paper, we really want it. We don’t want it to go to the competitor. I don’t think people realise how much we still do the “Would you like to submit your paper to us because it’s really good”, and we’re not afraid of asking for the really good stuff. [Belinda, full-time editor]

Indeed, editors of even the most prestigious journals saw their role as being akin to any other ‘service industry’ in which authors (like any other kind of customer) have to be comfortable and in which ‘relationship management’ is crucial:

(Relationships with authors are) the absolute key to our job, actually. We’re very much a service industry, I guess. And if (authors) have a bad experience with us, then if I was them, I wouldn’t submit again. The very best authors can choose where they take their papers, so it’s relationship management. [Brian, full-time editor]

This perceived need to provide a service to authors was believed to set up a ‘difficult balance’ for editors who spoke of feeling obliged at times to send sub-standard manuscripts out for review, and even to publish these manuscripts, in order to keep an author happy:

(When) we were too hungry for that manuscript, what we found was during subsequent revisions we would lower our standards more than they would raise theirs. And then we would publish something and then immediately feel horrible that we had done this. [Leslie, full-time editor-in-chief]

This is not to say that all journals are equally ‘hungry’, and editors at more powerful journals described themselves as themselves in the position of being able to ‘play a bit of hardball’ with authors by rejecting manuscripts with the knowledge that previously uncooperative authors would be likely to take an interest (whereas previously they might have handed over responsibility to their juniors), appeal the rejection and then revise adequately. A kind of power play was described:

We rejected because I didn’t feel the authors were revising their paper adequately, and then once you’ve rejected it, they appeal, and you say: “Well, fine, but only if you revise adequately and then you get what you want and you have the paper you wanted. It can work two ways: in the author’s favour and in the editor’s favour. You have to play a bit of hardball occasionally (laughs). [Belinda, full-time editor]

External reviewers’ power over editors

This study also found evidence of complex and shifting power relations between journal editors and their reviewers. For the most part, editors saw themselves as being in a vulnerable position relative to external reviewers because they depend entirely upon the goodwill of these reviewers for the functioning of their journal. Indeed, this need was sometimes perceived as a type of ‘desperation’ on the part of journal editors. An editor’s dependence on maintaining his/her pool of reviewers was also seen as having the potential to change editorial practice by, for example, preventing editors from asking reviewers to sign their reviews (even if this would
otherwise be the editor’s preference) and by making it difficult for a journal editor to reject a manuscript written by someone who has previously reviewed for the journal. At the very least, it was believed that more effort would need to go into explanations of rejections:

Sometimes (a previous editor-reviewer relationship) makes it difficult to reject (a manuscript). If they send a paper that isn’t really that great, but they think it’s brilliant (laughs), because you know them, it’s harder. You have to spend a bit more time on the reject letter rather than if they were anonymous. Of course, once you have those relationships you have to maintain them and therefore give them a bit of a reason.  

[Brian, full-time editor]

Given their dependence on their reviewers, editors made it clear that they were aware of the need to show that they appreciated reviewers’ efforts, and to ‘tread carefully’ when they overrode reviewers’ recommendations, making it clear to reviewers that their concerns have been taken seriously and that they were ‘acknowledged’ and ‘respected’.

It was made clear, however, that the power balance between editors and reviewers can shift. Speaking as reviewers, participants described feeling vulnerable to editors if, for example, they knew that their progress was being tracked electronically, and they described a sense of pressure to carry out reviews within a certain amount of time. Unsurprisingly, the extent to which reviewers felt pressured to perform for editors appeared to depend to some extent upon the reputation of the journal:

The journal you are actually working for has a big effect. Like if you get invited by the New England Journal to review something, you think “Oh my god fooof, fooof” you know, down tools and everything else. So there is a sort of hierarchy of concern, but if it’s, let’s say who, say it’s the [local journal name], you sort of think, “I’ll do that quicker, that’s fine”, and do a good job but you’re not feeling sort of quite as pressured I guess to go into the ins and outs and read all of the references and look things up, and all that stuff. So there’s that sort of interesting effect.  

[David, scientist, reviewer and speciality journal editor]

**Editors’ dependence upon broader social and political systems**

In addition to being dependent upon the cooperation of their authors and their reviewers, journal editors saw themselves and their journals as being embedded within broader social and political systems. A number of editors noted that even editors-in-chief of elite journals have limited power in determining the epistemic priorities and practices of their journals. They described social pressures that force them to take on particular editorial practices, such as making manuscripts electronically available:

I suppose it is like a collaborative of scientists are saying, “Well, let’s just have the journals we want and we’ll have them electronic and we’ll peer review them just as rigorously and maybe more rigorously, and fast, and you don’t have to pay for them”. The journal industry has sort of been pushed into making everything available.  

[David, author, reviewer and speciality journal editor]
In addition to ‘pushing’ editors into particular practices, social pressures were also seen to prevent editors from making any changes that threaten the academic status quo, in which publishing and funding are tightly coupled and in which impact factor is a key measurement of journal/publishing success:

I think the biggest problem’s going to be resistance from a community that’s driven by research assessment exercises, high impact factor, funding from grant giving bodies that tend to focus more on technologies than on public health issues or social issues. [Hugh, full-time editor-in-chief]

Editors of both well-resourced and less wealthy journals noted their vulnerability to resource limitations, as well as the need to meet the financial demands of their funding organisations:

We essentially have to say to ourselves: “You know, is this ten page article worth this many tens of thousands of dollars to put in our journal? Is it worth making an exception, and the value of that exception is this many tens of thousands of dollars?” [Leslie, full-time editor-in-chief]

Interestingly, very few editors made reference to guidelines set by bodies such as the Committee on Publication Ethics (COPE) or the International Committee of Medical Journal Editors (ICMJE), which has produced a well-known set of guidelines entitled ‘Uniform Requirements for Manuscripts Submitted to Biomedical Journals’. (This is a particularly interesting finding given that almost all participants were members of these organisations). Moreover, even those who made such references appeared willing to override policy:

We take very seriously core values and all these principles I have shared with you, and try not to make any silly draconian rules. Often the people who are making policy at the journals are not recently experienced investigators. I think they’ve talked amongst themselves so much that they’ve created a new truth that is not based in reality. [Leslie, full-time editor-in-chief]

Part 2: Relations of epistemic authority

While there was clear evidence of shifting relationships of power and vulnerability among reviewers, editors and authors, this study also revealed how reviewers and editors had differing senses of their own ‘epistemic authority’—that is, their authority to make judgments about the quality and dissemination of scientific research.

External reviewers’ attitudes towards epistemic authority

When reflecting upon their experience as external peer reviewers, participants both emphasised and de-privileged their own authority. On the one hand, some participants noted that, in being asked to review, their expertise is being acknowledged by their peers and argued that this should be a source of pride:
Of course one was pleased that one had been acknowledged to be an expert. [Simon, past reviewer and now full-time editor]

Other participants emphasised the need for reviewers to recognise that there are limits to their epistemic authority and that they needed to defer (back) to editors, insisting that editors have responsibility for decisions about publication:

Quite often it’s more suggestions rather than prescription, I don’t tend to be very prescriptive. The editors have to do that. (It’s) “had you thought of this?”, “what about that?”.[David, author, reviewer and part-time speciality journal editor]

A number of participants also spoke of deferring to other reviewers, noting that they might, under certain circumstances, suggest to an editor that somebody else (sometimes named) may be a more appropriate reviewer:

I think it’s quite important to say when you don’t know enough. So when I’m writing a review I’ll say: “I’m not qualified to comment on this aspect of it, so you should get an “x” person to talk about it.”[Sarah, author, reviewer and part-time speciality journal editor]

Finally, participants described the practice of deferring informally to their colleagues. This kind of collaboration was seen to be facilitated (somewhat unintentionally) by electronic submission and review systems:

I had one today where a reviewer must have submitted his review and he had recommended reject and given very clear problems with the paper, and two other reviewers had given the same problems but said “this is very original, it’s a first in Australia so they should revise it”, and then he must have gone in and had a look at their comments after he submitted his and asked if I would change his recommendation to revise. I have had people add other comments after they’ve read the other reviewers’ comments. [Anna, full-time editor]

**Editors’ attitudes towards epistemic authority**

Like external reviewers, editors had complex attitudes towards their own epistemic authority. On the one hand, no journal editor interviewed described him- or herself as having a purely administrative role in the publishing process, and all editors spoke of the need be ‘hands on’, to assume at least some degree of authority and responsibility and to act as peer reviewers of a kind:

We look at the paper in detail and take it every step of the way and we have a say in every single word that ends up in that paper. [Belinda, full-time editor]

I guess as an editor you are a reviewer to some extent anyway, although you might have slightly different criteria to a content expert peer reviewer. [Cherie, full-time editor]

At the same time, editors also spoke at length about the ways in which they defer to others—particularly to their external reviewers. While all editors interviewed appeared to take the
external review process seriously, there did appear to be a spectrum in terms of the ways in which these external reviews are used in the editorial decision-making process, ranging from very strong deference at one extreme to very weak deference at the other.

At the most deferential end of the spectrum were those editors who described themselves as being ‘only as good’ as their external peer reviewers. These editors spoke of placing the ‘primary decision-making’ in the hands of external reviewers and were critical of editors who did not make good use of external review:

> There have been journals that I’ve worked for (in which) I’ve thought that there was a complete disconnect between the editors-in-chief and the review process and the decision-making. (I’ve thought) that the reviews were often as much ceremonial as anything else, to allow the editor to do whatever he pleased. [Leslie, full-time editor-in-chief]

These more deferential editors described their need at times to continue obtaining reviews until the combined reviews clearly point in a particular direction. They also described using additional reviewers to ‘adjudicate’ between conflicting reviews or to overcome a ‘hung vote’ among editors:

> When we’ve got a set of views and can’t make up our mind, you know: “Is this really new? Is this going to make a difference?” We might then go to a “Solomon” and say, “Look, what do you think?” [Helen, full-time editor]

At the other end of the spectrum, less deferential editors described themselves as being only ‘guided’ by external reviewers. These editors were more likely to depict their reviewers as ‘advisers’, ‘counselors’, ‘guides’ or ‘consultants’, than as adjudicators or decision-makers:

> We think of our reviewers as consultants to the editors. The editors are the decision makers, the reviewers are consultants. It seems my role is (like) a general internist, that specialists are consultants that help me help my patients. But I’m the one taking care of the patient, so their role is to give me input. They don’t make the decision for me. [Glen, full-time editor]

Less deferential editors also emphasised their willingness to override reviewers’ recommendations, and described making their decisions ‘qualitatively’ rather than simply on the ‘quantity’ of positive or negative opinions received. In keeping with this, these less deferential editors made it clear that they call on extra reviewers only when there is a particularly ‘difficult call’. These additional reviews were used as a warning that they, as editors, may need to be particularly cautious, or as a means of ‘tipping the balance’ in what was still ultimately an editorial decision:

> [We might obtain further reviews] just to be safe, to say that we need as broad an idea from researchers as to whether this is good or not. It’s over-arduous on us, but it gives us that little extra bit of security. [Simon, full-time editor]

At the same time, however, it was recognised by even less deferential editors that it may be immensely difficult, in reality, to make a decision that is not heavily influenced by reviewers, or
to override reviewers’ recommendations, particularly where there is significant agreement among reviewers:

I am swayed...When I see three “majors”, I’m not leaning in favour, and I’m looking to see what the criticisms are. I mean, I can’t help it. I shouldn’t, that’s not awfully objective, I know. You can’t help but being swayed by three “majors”. Let’s see what the problems are, rather than seeing what the good is. [Andrew, full-time editor]

Irrespective of the decisions they ultimately make about publication, it was also important to less deferential editors that peer reviewers assume an appropriate level of humility:

What we don’t want is them to try and show you how much they know. I really don’t like it when reviewers feel the need to say stuff just because they want to prove what they know or they want to look clever. [Belinda, full-time editor]

These editors noted that they are particularly offended when reviewers see fit to challenge ‘editorial’ decisions, including the editor’s decision to send out the manuscript for review or the editor’s decision to override a reviewer’s recommendation. While some editors spoke of their appreciation for reviewers making confident editorial recommendations [Cherie], other, less deferential, editors described this as inappropriate and unhelpful [Frank]:

It really helps us enormously (if a reviewers tells us) whether it really is straight up something that we shouldn’t even consider going with. [Cherie, full-time editor]

I almost wish that that recommendation wasn’t part of the review process. [Frank, full-time editor]

Finally, less deferential editors described using external reviews as ‘tools’ for their own editorial decision-making. These editors spoke of deliberately selecting biased reviewers, reviewers who are likely to give a conflicting point of view to that of the author, and reviewers who are likely to disagree with other reviewers:

Often if we know somebody has an obvious conflict of interest we ask that person to review a paper because it gives us a balance to the view point, you know it gives us the other side of the story. [Cherie, full-time editor]

Sometimes I’ll intentionally pick people who I know are going to have a strong view, maybe even someone I know that has a strong view against a particular area to see what the strongest argument could be made against it. [Glen, full-time editor]

When communicating with authors, less deferential editors spoke of their efforts to convey to authors messages other than reviewers’ comments and to filter reviewers’ comments rather than transmitting them unedited to the authors. Less deferential editors also seemed more willing to consider authors’ appeals on the basis of flawed reviews:

I might say that editors have a lot of discretion in the whole process and we’re very, very powerful to some extent, but we do have an appeals process. So I must say that we acknowledged quite freely that we weren’t infallible and there was an external mechanism. [Cherie, full-time editor]
Factors determining the degree of deference

This is not to suggest that editors were fixed in their level of deference to external reviewers, and several editors spoke of the ways in which their deference had evolved over time:

- I guess when you’re first starting out you very much rely on (reviewers) to pick up errors or inconsistencies or methodological problems. As you go further, I guess you start seeing reviewers more as adding value, and you start maybe to be a bit more selective in the way that you use their comments. So for me it’s been probably an evolving process of how I view reviewers’ comments. [Anna, full-time editor]

Editors also described a number of different factors which would determine the extent of their deference to external reviewers at a particular point in time. First, the degree of deference seemed to depend upon whether the issue at stake was ‘scientific’, rather than ‘editorial’, with editors being more willing to override reviewers’ opinions on editorial matters. Decisions described as ‘editorial’ included the decision not to publish work that is adequate, but not as good as other submitted manuscripts, as well as the decision to publish topical work of non-ideal quality, even in the face of negative reviews:

- For instance (a reviewer) would say, “look we really need a randomised control trial to answer this question”, but you think, well, that would be nice in an ideal world, but we need to make the first steps into a process, and a series of case studies on a really novel topic that’s perhaps very topical, you know, being discussed in the media all the time at the moment or very worrying for public health reasons, that’s fine. We’ll go ahead with it for those sorts of reasons. So you don’t just use pure science as your criterion for assessing a paper, even if that’s what the reviewer’s opinion is. [Cherie, full-time editor]

Second, editors observed that the extent of their deference depended upon the type of manuscript being reviewed, with a number expressing the view that it was relatively easy to ‘ignore’ a reviewer’s opinion when opinion pieces were being reviewed ‘because you can’t all have the same opinion about everything in science’. Finally, editors made it clear that they assume different degrees of deference according to the quality of the reviews they receive, and the extent to which they are convinced by the reviewers’ assessments. All but the most deferential editors recognised that reviewers’ opinions are not always ‘well informed’ or based on sound and obvious reasoning and that it is important to be critical of the reviews received:

- You’ve got to look to the reviews and work it out from there. You might say, “Okay, although this person has pointed out lots and lots and lots of flaws, the two other people say it’s fantastic, there must be a good reason for that, maybe it’s an emerging topic, maybe it’s very new groundbreaking research and that’s why this old diehard doesn’t really like it”. [Cherie, full-time editor]
Editors’ deference to their editorial colleagues

Editors spoke of strengthening their decision-making not only by deferring to external peer reviewers, but also by invoking the authority of their editorial colleagues. Not surprisingly, junior editors spoke at most length about need to defer to, and learn from, both their more senior colleagues. The learning process, and the acquisition of sufficient epistemic authority to make unsupervised editorial decisions, was, however, described as being generally rapid, and several editors observed that it is not long before novice editors feel sufficiently confident to make their own decisions.

Deference to other editors at times involved deferring to the journal’s editor-in-chief, particularly when an author has appealed an editorial decision. But even this deference to editorial leaders was described as a complex process taking into consideration the authors and their claims; the views of the original managing editor; the views of the original reviewers; and (in some cases) the views of a new set of external peer reviewers. Indeed, some editors-in-chief emphasised their efforts to assign final decision-making authority to their team of editors or to their editorial board members rather than taking on the role of primary decision-maker:

In some ways I view myself as somewhat the conductor of a great symphony. It’s not necessary that I play the violin better than anyone else, or play the cello better than anyone else, but I need to make sure that I know how to make all the great musicians work together so we get a great sound. I allow them to work as mini editors-in-chief of their sections with me overseeing them. [Leslie, full-time editor-in-chief]

Importantly, the epistemic authority of the editor-in-chief appeared to extend beyond adjudicating in individual editorial decision, as he or she was seen to have the authority to determine the ‘colour’ of the journal, shaping its overall mission and its epistemological values (within the resource limits described above).

In addition to deferring to editors-in-chief, journal editors spoke at length about their collaboration with their editorial colleagues. Even when manuscripts were accepted without external review (as, for example, in the case of some opinion pieces), a level of informal review was described:

[A creative writing manuscript] is about the only thing that doesn’t get peer reviewed, although it gets read by ten editors before it’s published, and we often change things. So that’s another part of peer review that goes on here. We’re constantly reviewing over each other’s shoulders. It’s just really constant, constant. [Yvonne, full-time editor]

At some journals (particularly those with full-time editorial staff), this process is formalised such that all editors give their opinion on manuscripts that are being considered for publication. The consideration of other editors’ comments was seen as an important means of showing editors that there are ‘lots of good ways of interpreting’ manuscripts and reviews, and that these interpretations may differ from their original impressions. Indeed, it was assumed that, unless the original editor assigned the manuscript was an expert on the subject, his or her decision would be influenced by other editors’ arguments:
It’s also very interesting to see at the meeting, say, Editor A writes his comments and then all the other editors over the course of the day write their own, and everyone disagrees with Editor A. By the time it gets to the meeting, Editor A has changed their mind about what they wrote. And partly that’s perfectly understandable because everyone else has now made arguments that they hadn’t thought of and they’ve completely convinced them. [Belinda, full-time editor]

This process of involving more than one editor (or of involving editorial board members) was seen both as an important part of being ‘fair’ to, and ‘protective’ of, authors, and as an important justification for editorial decisions to reject a commissioned manuscript or a manuscript that has received positive external reviews. Indeed, editors spoke of using their option of invoking the expertise of their colleagues to justify their very right to edit a prestigious journal:

People have said to me before: “God, who are you to judge a [top tier journal] paper?” And you think, well, you know, I have done the research and done the training myself, but we do have people who are a lot more qualified than me who have been working in it for a very long time. They probably know more about epidemiology, as far as research techniques go, than the people writing the papers. So they are absolutely qualified. [Amy, full-time editor]

Interestingly, editors spoke of deferring to their editorial colleagues not just because they can provide expert advice as knowledgeable individuals, but also because editorial decision-making needs to occur at the level of the collective. Editors argued that while the views of the group are important because they represent a ‘microcosm’ of readers, the main reason for invoking the collective was because it generated some kind of group consensus (or at the very least ‘collective acquiescence’). This kind of editorial decision-making process was described as a ‘team decision’ or a ‘group decision’, where the ‘editors as a body’ or ‘the whole editorial committee’ came to a decision about a manuscript. Various metaphors were used to describe this collective reasoning, such as a ‘common brain’:

We all have our sort of specialties. But it’s funny, we’ve all got almost like a common brain in terms of what we do and don’t send out. We all more or less send out roughly the same amount of papers and we have roughly the same success rate. We think like [journal name] editors. You’ve got to learn to think like a [journal name] editor. You learnt that by going to those meetings, by listening to what we want and what we don’t want. [Gabrielle, full-time editor]

Indeed, several editors argued that collective reasoning is the main strength of the editorial process and it is this ‘team decision’ that gives editors the ultimate confidence to make a decision about publication—particularly one that overrides, or occurs in the absence of, clear reviewer recommendations:

My strategy (for handling complaints): “It’s the editors, not me”. We all do that. “It’s an editorial, joint decision at the manuscript meeting”. And it is. [Penny, full-time editor]

At the same time, collective decision-making was recognised as having the potential to stifle individual thought. This was seen to be particularly the case for junior editors, who may feel
that they wish to disagree with the group but don’t know whether they ‘trust themselves’ enough to do so. Editors were conscious that, unless individual editors make a deliberate effort to make their own assessment before the group meeting, or otherwise remain separate from the group, this can result in a situation of ‘sheep following’ and the collective decision-making process can ‘take on a life of its own’:

One (paper) went through because it was getting the nod, and everyone said, “yeah, yeah, seems to be OK, seems to be OK”. The questions that had been asked by the reviewers had been answered, but the underlying questions just got forgotten in the hype, almost. It took on a life of its own. It’s just taking on a life of its own and getting through. [Simon, full-time editor]

Some editors thus emphasised the importance of there being at least some ‘decentralised’ decision-making (by individual editors) while others spoke of making a deliberate effort to ensure that they make their own assessments prior to editorial meetings, and ‘speak up’ even if they are a ‘lone, contrary voice’, so that their voice is not lost in that of the collective:

I try and leave the [sheet containing other editors’ comments] to the end. So I read the paper, make up my mind about the paper, the reviewers’ comments, and then think, “What would I say if I was putting the sheet together...?” And if it’s very different to what the editor whose paper it is has said, then you need to say that. [Belinda, full-time editor]

I would perhaps disagree with the (other editors’) interpretations, but I always accepted that people disagree with my interpretations as well, and I’d fight for them. [Simon, full-time editor]

Editors’ deference to readers

Finally, journal editors described how at times they chose to de-privilege both their own epistemic authority and that of their external reviewers by relying on readers to assess the quality of published material. Here epistemic authority was placed in the hands of the collective of practising scientists:

People will say: “Well, you can’t just have everybody stuffing their stuff on the internet because nobody will know whether or not it’s any good”. And my answer to that is that the person who should decide whether something is any good should always be the reader anyway, and that the idea that somebody else should decide whether something is any good is contrary to the whole ethos of science. [Eric, author, reviewer and specialty journal editor]

It is notable that by ‘readers’, most participants meant practising clinicians and scientists and not lay people or policy-makers. It is also notable that, while most editors interviewed were generally in favour of post-publication critique, only two of the interviewees argued for complete replacement of traditional pre-publication review with open publication and assessment by readers (a finding which could, of course, stem from the fact that the majority of interviewees were practicing editors).
DISCUSSION

It is clear from this research that power relations in manuscript review are complex and dynamic, may have positive and negative features and that even where power is experienced as controlling, restrictive and illegitimate, it can also be resisted. While authors were generally seen as being vulnerable during the review process, it was also recognised that, at times, authors may be more powerful than reviewers and editors, having the power to retaliate against critical reviewers, and the power to decide whether or not to (re)submit their manuscripts to a particular journal. Similar context-specific power relations were seen to exist between journal editors and external peer reviewers. While on the one hand reviewers spoke of feeling obliged to participate in the process of review, ultimately the decision to act as a reviewer was seen as a voluntary one. Editors thus appeared to depend on their reviewers, and to feel pressure to keep their reviewers happy, even to the extent that they felt forced to give special treatment to regular reviewers who subsequently choose to submit a manuscript of their own. Editors were also seen to be vulnerable in the sense of needing to work with limited resources.

Perceptions of epistemic authority were similarly complex, with some editors and reviewers seeing themselves as possessing significant authority, and others emphasising the need to defer to others. When reflecting on their experience as external peer reviewers, participants spoke of regularly invoking the authority of editors, particularly in relation to classically ‘editorial’ decisions, and also of deferring to other reviewers, either by suggesting that editors place more weight on the other reviewers’ opinions or by suggesting alternative reviewers. In describing their editorial practices, participants described a complex process of deciding when and how to invoke the authority of external peer reviewers. Several reasons were given for deference to reviewers (many of them social), and the degree of deference ranged from very weak to very strong. In addition to deferring to external reviewers, editors spoke about invoking the authority of their editorial colleagues, both formally and informally and both as individuals and as a collective. And, finally, editors noted that they place some weight on the authority of their readers, who are expected to criticise published manuscripts.

To some extent, these findings corroborate the existing critical discourse on manuscript review, which emphasise manuscript review as a power-laden enterprise. But these findings also add to the critical literature on manuscript review in a variety of ways. First, this study shows that power in manuscript review is conceptualised both as facilitatory (‘power-to’) and as controlling (‘power-over’), and that negotiations around legitimate epistemic authority are at least as central to peer review as concerns about abuses of power. Moreover, these findings show that even the most prescriptive ‘power-over’ is complicated and modulated by various resistance strategies on the part of authors and by the context-specificity of power relations. While some of these complexities are alluded to in the existing critical literature, for the most part these qualifications are obscured by discussions of outright abuses of ‘power-over’ and on the perceived need to find ways of limiting this power or preventing its exploitation. This research therefore both corroborates and extends existing ‘critical’ understandings of the process of manuscript review.
A shifting ‘net’ of power relations in manuscript review

The results of this study support the idea that power is not simply a unidirectional and self-reinforcing phenomenon but rather complex, dynamic and deeply subjective. This study is therefore consistent with critical accounts of power that recognise that shifts in power may occur as a result of changes in perception among those on its receiving end, who may at one time see power as facilitatory and legitimate, and at another time see power as controlling and illegitimate, in which case resistance is likely to occur. Power is also seen to be context- and resource-specific and to shift due to self-modulation by those who hold power (Olsen 1970; Wartenberg 1992; Hindess 1996; Ryn 2001; DiPalma 2004; Meehan 2004; Spector Person 2004; Collinson 2005). Foucault, for example, distinguished power from domination (which, he argued, arises when the possibility of effective resistance has been removed and when it is no longer meaningful to speak in terms of ‘relations’ of power) (Foucault 1980; Hindess 1996; Moss 1998; Patton 1998; Mills 2003) and saw discursive formations consisting of net-like relationships:

Power must be analysed as something which circulates, or rather something which only functions in the form of a chain. It is never localised here or there, never in anybody’s hands, never appropriated as a commodity or piece of wealth. Power is employed and exercised through a net-like organisation. And not only do individuals circulate between its threads; they are always in the position of simultaneously undergoing and exercising this power. They are not only its inert or consenting target they are always also the elements of its articulation. In other words, individuals are the vehicles of power, not its points of application. (Foucault 1980, 98).

We argue that manuscript review process can usefully (re)-conceptualised in light of theories that conceptualise power as:

(A) complex social presence that exists in an intricate network of overlapping and contradictory relations. (Wartenberg 1992, xix).

Practical implications: rethinking the ‘ethics’ of journal manuscript review

There are several potential practical implications of these findings. First, insofar as this research corroborates the existing critical discourse on manuscript review, it highlights the need to continue to identify and prevent abuses of power. Measures such as those described in the introduction (e.g. asking reviewers to sign their reviews and declare their competing interests, and having in place a variety of oversight processes) therefore clearly have their place. It is difficult to say whether such measures would actually reduce abuses of power. To take the practice of double anonymisation as an example, this could on the one hand prevent power-hungry reviewers from discriminating against their competitors or against authors they simply do not like. But it could also enable reviewers to enact their power without needing to take responsibility for the tone or content of their review. Whatever the effects of such “dominance-
management” strategies, this research suggests that to focus only on dominance, and to view power relations as entirely illegitimate and destructive, is simplistic and misleading.

First, it might be helpful to encourage authors (particularly those just beginning to publish) to reflect upon and develop their own set of ‘resistance’ techniques for preventing and managing the (inevitable) pain of an imperfect and competitive process. Second, reviewers and editors might be encouraged to reflect more openly on their own experiences of power and vulnerability, and on the ways in which these might have shaped their decisions or recommendations. Such an approach would be a natural extension to the now standard practice of asking reviewers and editors to declare conflicts of interest, but would differ in that experiences of power would be viewed in neutral, or even positive, rather than strictly negative terms. With respect to epistemic authority (‘power-to’) there might also be benefit in training reviewers and editors to think more clearly about their roles and responsibilities, and the various ways in which they might defer to others. Journals might also provide clearer guidance to reviewers as to what the journal expects of them and why a particular review is being sought (e.g. to help justify an editorial decision, or to adjudicate where other reviews conflict). Finally, journal editors might reflect more upon their editorial processes, asking, for example, how and why collective authority might best be invoked and utilised without obscuring individual voices.

While increased awareness of, and openness about, power relations might be effective in improving the manuscript review process, an understanding of the complex power dynamics underpinning manuscript review might also be a justification for more radical, structural changes to the publishing process. If, for example, editors’ dependence upon reluctant external reviewers is compromising the quality of reviews, then the focus might shift to ways in which manuscript review might become a genuinely valued academic activity linked in meaningful ways to academic promotion. Similarly, if it is clear that the editorial process is being compromised by resource limitations (particularly in the case of specialty journals) then there might be a need to develop different funding models for academic journals. At the same time, more resources might be dedicated to post-publication systems of review so that the authority of readers (both lay and biomedical) might be more effectively recognised and harnessed.

It remains to be seen whether any of these measures will actually make manuscript review more effective in achieving its goals of ensuring manuscript quality and facilitating the dissemination of important ideas. In the absence of empirical evidence, we do not advocate for any particular approach or set of approaches. Rather, we suggest that these strategies need to be tested as part of the systematic program of research called for in the Cochrane review, and that existing approaches, which either ignore power relations (pretending that the process is entirely impersonal and ‘scientific’) or attempt to eradicate them (seeing them only as forms of dominance), are unlikely to succeed in improving manuscript review in any measurable way. Whatever practical strategies are adopted, we would argue for a more sophisticated conversation about power relations and the ‘ethics’ of journal peer review, in which such relations are understood as being both inevitable and not entirely morally undesirable.
References


Dickersin, K., E. Ssemanda, C. Mansell, and D. Rennie. 2007. What do the JAMA editors say when they discuss manuscripts that they are considering for publication? Developing a schema for classifying the content of editorial discussion. *BMC Medical Research Methodology* 7: 44.


Scheurich, J. 2005. That which is troubling many new scholars about the nature of research and publishing is the harbinger of the changes that we are living. Harvard Educational Review 75 (3): 274-77.


