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Editorial

The exchange economy of peer review

A paper was submitted to this journal. It was discussed by the editor and the coeditors and a decision was made to proceed to review. Six possible referee names were suggested. Three were asked. One said no, one said yes, one didn't reply. Two more were asked—one immediately and one after the 'no response' had been chased to no avail. One replied, one didn't, and finally one report arrived. The final original referee on the list of suggestions was asked; another two names were thought of, and they were asked too. One of these said they would do it. Three reminders later this generated a second report. Eight referees asked; six months; two reports; one disgruntled author.

Making decisions on two reports is rarely easy; a revise and resubmit was offered; the author eventually delivered a revised manuscript. It went back to the two original referees. One of these delivered the report; the other after much chasing never replied. Such is, all too often, the experience of peer review.

If you were the referee who delivered the original report, and then offered a view on the revised paper: thank you. If you quickly replied to say you couldn't review the paper, it is at least useful to know immediately so we can ask someone else. The worst is never receiving a reply: are people so disorganised that they can't even reply?

Exchange economy

Nonetheless the number of refusals—either clear and upfront or implicit through lack of reply; or saying they will do it and then never delivering—is a real problem. After two years as editor of this journal, certain trends in reviewing have become apparent. While there are a large number of very willing reviewers, many people, including those who should have a special stake in the journal—that is, authors—are refusing to review. This raises the question of the exchange economy that is at the heart of peer review, and what might need to be done to ensure this works effectively.

In late 2007 there was a discussion on the crit-geog-forum e-mail list concerning paying for journals. One of the comments raised the question of the 'free labour' that is inherent in peer review, suggesting that the 'reserve army of the professoriat' was mobilised to do this work by 'large multinational publishing corporations' and their editors.

Yet this view is surely wrong. It suggests that because peer review is not compensated after the fact (except perhaps through publisher discounts that are sometimes offered; or of course the benefit of seeing new work in your field) it is not compensated at all. It is work that 'we' do for editors or publishers with no recompense. Because editors are therefore indebted to referees, and should be grateful that we receive any reports, it's unreasonable to expect prompt reviews, unfair to chase, and basically there is nothing to complain about.

But there is someone else in this whole process: the author. And, of course, authors are also referees; and vice versa. Who produces the need for peer review? Authors. Every submission to a peer-reviewed journal (that meets basic standards and criteria of suitability) produces the need for three reports, even if editors sometimes are forced to make decisions on the basis of simply two.

What does this mean? That, as a basic rule, we should referee three times as many papers as we submit—because every submission, even if rejected by one journal and

submitted elsewhere, creates the need for referees. If you send a piece to two or three journals before acceptance, the paper has created the need for six or nine reviews.

In other words, the exchange is not simply one-way—no recompense after reviewing—but a process, where my paper is reviewed, and in turn I review. I might review papers in advance of my own submission; I might be asked by a journal after submitting there. But it is a process and over time it should even out. Essentially there is an implicit bargain in submitting a piece to a journal: you are effectively agreeing to review three papers over the next couple of years. If you are a regular reviewer anyway, this is of course working well in advance. Yet we are having far too many people refusing to review for the journal even when they have papers under submission or recently published. The refusal to referee is a major problem anyway, and is taking up a huge amount of time, but it's especially problematic from authors.

Three times as many papers as you submit. Clearly some people are refereeing far more than this; but from the experience of editing this journal it's clear that many more people are doing substantially less. Something in this system is breaking down.

Possible reasons

Of course, we are all increasingly busy. The sheer volume of correspondence, demands from students, our own research, pointless bureaucratic issues, etc. Faced with a range of other pressures, it might well be that a request from a journal to review a piece is low on the list.

Yet it is not simply senior scholars, but early-career lecturers and people with freshly minted PhDs declining to review. So it seems that the basic idea is not being absorbed as people become academics—in a sense this is not surprising, because there are an unusual number of people who find the whole peer-review process somewhat mystifying. Some people seem to think one submission is paid off by doing one report; others decline to do reports but then wonder why their piece takes so long to review. Of course, there are times when it is acceptable to decline to review a paper. If there is a conflict of interest, if you are on leave for personal reasons, or perhaps the paper has been misdirected, then it is only proper to decline.

But in general terms, if we want the system to work, then we—as authors—must be prepared to be referees. If the *Journal of Goat Husbandry* wants me to review a piece that uses Foucault, then I might think there is a case to reasonably decline on the basis that this is not a journal I am ever likely to submit to. But *Society and Space* does use a wide range of referees whose expertise in a particular area is invaluable, but who might not see it as a place for their own work. We obviously hope that their willingness will continue. And if a top geography journal that I have never submitted to—*Annals*, say—requests a review, then I feel a professional obligation. If it is a journal to which I have submitted work in the past or am intending to in the future, then what basis would I have for refusing?[(1)]

We get around 150 submissions a year. We don't review everything—one of the plans when I took over as editor was to reduce the referee load by a more thorough process of prescreening papers. I read every submission; I consult with the four coeditors on each submission. Only papers that we think have a reasonable chance of making it through peer-review go out. If a paper is clearly inappropriate, better suited to a different journal, poorly executed, etc, then we get back to the author with

[(1)] Editorial board members are, of course, asked more often than regular readers and authors. But there is something of a worrying trend where sometimes even they are refusing to review. We do not have a board constituted of huge names for prestige, but a working board of scholars who represent a wide range of disciplinary interests, geographical backgrounds, etc. It is surely not unreasonable to ask editorial board members to review a paper quarterly, for instance, although we generally ask much less than this.

suggestions for what would need to be done to make it suitable for review, alternative journals to consider, etc This is not an attempt to police work, but rather to use referees only when we need them. If there is disagreement between the coeditors, we usually give the paper the benefit of review. But better—I think—to hear from editors within a couple of weeks than to hear through referees after months, as well as the extra work this has generated for referees.

Approximately 100 papers go out. Ideally we'd ask 300 referees. But more often we are asking about six, sometimes more, referees per paper. This is creating a huge amount of work for the editorial assistant, as well as creating delays for authors.

Any alternative system—say paying referees for their time—is fraught with difficulty. It might feel good to say that Elsevier or Wiley-Blackwell are huge corporations and they could afford it, and that may well be true. But do we really think they will cut into their profit margins in order to pay referees? If referees were paid, say, £30 to review, then we could easily envision a £100 submission fee for authors—3 × £30 and a £10 'convenience charge' (I'm thinking of Ticketmaster-style pricing here). Journals with smaller independent publishing houses, or online free-to-access journals would face a whole range of different pressures.

A policy change: making the implicit explicit

This is not an argument about whether peer review is the best way to assess whether a paper is worth publishing—such debates have been had elsewhere. It is equally not an argument about the wider political economy of journal publication, including questions of charging for journals and the politics of state-funded research generating profit for private corporations. But it is an argument about how—if we decide that peer review is the least worst option—it needs to work. For this reason we have changed the conditions of submission to the journal. These currently read:

Submission of a paper implies acceptance of the following conditions:
(1) the paper reports unpublished work;
(2) the paper is not being submitted to any other journal;
(3) you are fully authorised to submit the material for publication;
(4) if accepted, the paper will not be republished without the consent of the publishers.[2]
We have added a fifth condition, that was surely implicit, but we have now made explicit:
—that you agree to act as a referee for other submissions to the journal, bearing in mind that each submission generates the need for three reports.

This will appear as condition (4), moving the current fourth condition down to (5). This is because every paper you submit to a journal creates the need for referees, and thus the exchange economy—one might even add the moral economy—is that you then act in turn as a referee. It is not just accepted papers that create the need, and it is not just one submission and one report, but three times as many. If we all did that number, averaged out over time and a range of journals, the system would work. When we don't, it breaks down. Hence the delays in the review process, and the number of requests you are likely to get if you are a good, dependable, prompt referee.

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[2] http://www.envplan.com/dauthors.html

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