

W(h)ither Gatekeeping? Academic publishing and peer review in complex networked systems

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Digitalisation, the Internet, open access initiatives, and trends towards multidisciplinary scholarship are affecting academic publishing practices in diverse ways. In this essay I focus on questions, problems and issues of academic “gatekeeping” (the conventional quality assurance role of journal editors and reviewers) that arise in complex networked systems. These include the diminishing likelihood of any peer reviews being “blind”, alternatives to peer review made possible by open access publishing, and the unpredictable emergence (cf. planned production) of knowledge within complex, open systems and networks. I argue that these circumstances require that we reconceptualise academic gatekeeping in terms of facilitating boundary crossings, transgressions and transformations, rather than as policing traditional or arbitrary boundaries and borders.

Preambulatory position

My situated and embodied interests in questions, problems and issues of academic publishing and peer review arise in large part from my academic life history, which includes particular experiences of “gatekeeping” such as: (i) 15 years (1986–2000) as Asia-Pacific editor of the *Journal of Curriculum Studies* (a leading international journal in its field since first published in 1968); (ii) eight years (2004–11) as founding editor of an open access electronic journal, *Transnational Curriculum Inquiry* (the journal of the International Association for the Advancement of Curriculum Studies); (iii) four years (1999–2002) as co-editor of the *Australian Journal of Environmental Education*; and (iv) membership of numerous international editorial boards (currently 11). As a past president (2008) and executive member (2007–9) of the Australian Association for Research in Education I have also had an ‘industrial’ interest in the effects on academic workers of government policies/agendas on research quality and evaluation, some of which are tied to assumptions about the measurability of quality in academic publications.

Institutionalising peer review

I was inducted into the culture of academic science during the 1960s, a time when the idea that peer review constitutes some sort of “gold standard” for judging the quality of scientific publications was widely regarded as a truism. John Ziman (1968: 148), an eminent physicist who increasingly turned his attention (and scholarship) to the social dimensions of science, clearly expresses a view that was widespread at the time:

An article in a reputable journal does not merely represent the opinions of its author; it bears the imprimatur of scientific authenticity, as given to it by the editor and the referees he [sic] may have consulted. The referee is the lynchpin about which the whole business of Science is pivoted.

However, as Harriet Zuckerman and Robert Merton (1971: 68) point out, the referee system as we now know it has not always been an integral or unvarying component of the social institution of science but, rather, “evolved in response to the concrete problems encountered in working toward the developing goals of scientific inquiry and as a by-product of the emerging social organisation of scientists”.

In Western Europe, the social invention of the scientific journal can be traced to new scientific societies and academies which, during the seventeenth century, began to engage in more systematic modes of scientific interchange than the letters, pamphlets and books that had previously characterised such communication. As Zuckerman and Merton (1971: 68) observe, these organisations “provided the structure of authority which transformed the mere *printing* of scientific work into its *publication*” (emphasis in original). For example, in England the council of the Royal Society authorised publication of its *Philosophical Transactions* in March 1665. As reported in Charles Weld’s (1848: 177-8) history of the Royal Society, the authorisation of this publication anticipates a form of peer review:

On the 1st March [1665], it was ordered at a Meeting of the Council: “That the *Philosophical Transactions*, to be composed by Mr. Oldenburg [one of the two Secretaries of the Society], be printed the first Monday of every month, if he have sufficient matter for it; and that the tract be licensed under the charter by the Council of the Society, being first reviewed by some of the members of the same; ...” In conformity with this order, the first number of the *Transactions* appeared on Monday, the 6th March.

This summary of the Council’s decision-making anticipates the future organisation of scholarly journals by assigning prime responsibility for the new periodical to one person (Henry Oldenburg), albeit without designating him as editor or specifying his obligations in this role.¹ However, Zuckerman and Merton (1971: 69) note that Oldenburg, with the support of colleagues in the Society, soon “introduced various adaptive expedients which ended up by defining the role of an editor”. Weld’s account of the council’s decision also indicates that it recognised the immediate problem of having “sufficient matter” for the newly conceived periodical. Zuckerman and Merton (1971: 69) speculate that institutional devices were gradually evolved to induce scientists to contribute to the journal and add:

What is perhaps most significant here is that the council, as sponsor of the *Transactions*, was involved with its fate and wanted to have a measure of control over its contents. These adaptive decisions provided a basis for the referee system.

However, although peer review is undoubtedly embedded in institutionalised academic publishing, it might be misleading to refer to *the* referee system as if it was a unitary singularity. We can trace practices that resemble peer review back to the seventeenth century but, as Paul Manske (1997: 768) asserts:

It was not until the post-World War II period, some 200 years after its inception, that the process became universally accepted. Just as there is no specific time when the concept of peer review was adopted, so also the process of peer review has never taken a standardized form and continues to vary from journal to journal.

Ann Weller’s (2001: 3-8) exhaustive studies of reviewing practices confirm that prior to World War Two peer review was often uncodified, and editors frequently made all decisions themselves with informal advice from colleagues. An anonymous Editorial in *Nature*, “Coping with peer rejection” (2003), alludes to what are perhaps the most celebrated examples of such unilateral editorial judgements, namely, the five extraordinary papers (on topics including special relativity and the photoelectric effect) that Albert Einstein published in *Annalen der Physik* in 1905. These papers were not peer-reviewed by anyone other than the journal’s editor, Max Planck. Although Planck was clearly Einstein’s peer, he did not deploy a panel of ‘blind’ reviewers, because at that time established authors and editors were given more latitude in their journalistic discretion. As the *Nature* Editorial asserts:

[Planck] recognized the virtue of publishing [Einstein's] outlandish ideas, but there was also a policy that allowed authors much latitude after their first publication. Indeed, in journals in those days, the burden of proof was generally on the opponents rather than the proponents of new ideas.

Understood historically, it seems clear to me that the relatively recent widespread adoption of what Fytton Rowland (2002: 248) calls “the paradigmatic ‘editor plus two referees’ system” is, to paraphrase Zuckerman and Merton (1971), an evolutionary response to specific problems and circumstances encountered by academics as they work towards developing goals of scholarly inquiry and invent mechanisms of social organisation that support their work. In this light, I would argue that the present conventions of academic gatekeeping have been shaped by the specific problems and circumstances of print-based publication and its intersection with discipline-based scholarship, and that these conventions need to be rethought in the light of the new problems and circumstances of digital publication and knowledge production in complex networked systems.

Reconceptualising peer review in a digital era

Digitalisation, the Internet and open access initiatives challenge print-based academic publishing practices in diverse ways. Most obviously, open access initiatives pose an economic threat to print-based publishers who provide academic authors—at a cost (to someone)—with evidence of their work's quality and/or impact by reference to rejection rates and citation indices. Alternatives to print-based publishers are plentiful; for example, the American Educational Research Association's Communication of Research Special Interest Group currently lists 258 scholarly education journals that are peer-reviewed, full text and openly accessible at no cost to readers.

The conventions of “double-blind” refereeing in print-era academic publishing presume that the author does not know the referee's identity and *vice versa*. But my experience is that double-blind refereeing tends not to work because: (i) authorship can be obvious to a knowledgeable reader; and (ii) search engines such as Google Scholar make it easy to determine authorship of an ‘anonymised’ paper (e.g., I have recently reviewed several papers whose authors had already advertised their submission to the respective journals on their institutional homepages and/or personal websites). Rowland (2002: 249) reports that studies in the biomedical sciences have led prestigious journals such as the *British Medical Journal* (BMJ) to use open peer review (where the referee is identified to the author), which has led to more constructive reviews, fewer abusive reviews, and helps to prevent plagiarism.

Rowland (2002) notes that discussions about new approaches to online publication usually distinguish between peer review (in which a small number of individuals pass judgement on a paper), and peer commentary, where after publication other scholars may append notes or comments to a paper. Many advocates of change argue that existing review systems perpetuate an outdated approach to the distribution of research results—that they were needed to ration space in print journals, and that they are unnecessary in the relatively cost-free environment of the Internet because authors can post all their material and allow readers to sort out what they want to read. In the short term, national regulatory/compliance frameworks might determine the extent to which open peer commentary after publication is accepted as a valuable adjunct to traditional refereeing rather than a replacement for it.

A related approach is to mount papers online—identified as unrefereed—and then solicit comment on them. On the basis of comments received, the author then revises and improves the paper, after which it is refereed in its improved form in the usual way. If it is published as a refereed paper it undergoes no further changes. This overcomes many of the objections to non-refereeing. In effect this is what happens in the fields of physics, mathematics, computer science, quantitative biology and statistics via Cornell University's e-print archive,² which mounts both unrefereed and published papers (each identified as such). Authors take pre-publication comments about their

papers into account when submitting their papers to conventional journals. After publication the pre-print version is removed and replaced by the published version. This system has been slow to catch on in other fields, although attempts have been made in both economics and psychology.

A more radical idea comes from John Smith (1999), whose hypothetical “deconstructed journal” (I would prefer to call it a ‘distributed’ or ‘disassembled’ journal) is based on a suggestion by the originator of the Cornell e-print archive, that journals could be “overlaid” on that archive. In Smith’s model, an author writes a paper, places it on a server, and notifies one or more “evaluator organisations” of its existence. They review the article, require changes in the usual way, and eventually approve the paper. At this point the author notifies the relevant “subject focal points” who select material that is relevant to their subject areas and insert links to the paper if they think it is appropriate for inclusion. This model separates out the storage and provision of access to the paper (provided by the owners of the server where it is mounted), quality control (provided by the evaluator organisation) and the subject-specific grouping of documents (provided by the subject focal point). The server might belong to the institution that employs the author, although national libraries could also provide secure long-term archiving. The subject focal points are similar to subject gateways that already exist, but they would also be the closest approximation of a ‘journal’ in this model. The evaluator organisations could be learned societies. But who pays for the different functions? So far as the evaluator organisations are concerned, Smith proposes that the author pays a fee for having the paper evaluated, effectively similar to the author-pays model of some electronic publications. It is not clear how the subject focal points could be financed, although their costs would be minimal because they host little except links. An advantage of Smith’s model is that papers in cross-disciplinary fields could be included in the subject focal points of all the fields that they encompassed. Indeed, they could be separately evaluated by each field, according to the refereeing standards prevalent in that particular discipline.

One advantage of publishing in unrefereed open archives is that it helps to preserve scholarship that might be undervalued within understandings of what counts as knowledge at a given time. For example, Benoît Mandelbrot’s inquiries in the mathematical representation of chaos were triggered by what he found among data discarded by the scientific establishment: “I got the habit of literally looking in the trash cans of science”.³ He recalls that after finding an article on measuring coastlines in a Yorktown (US) library’s rubbish, “I systematically went through obscure journals. And again and again I found in those journals the idea that the world is complicated, erratic, bizarre, unclassifiable”. This anecdote contains salutary lessons for those of us who hold gatekeeper roles in journals with high rejection rates. We live with the possibility that what we reject—and which may thus remain unpublished or be relegated to “obscure journals”—could be highly significant within frames of reference different from those we use as criteria for an article’s acceptability for publication.

I was mindful of this anecdote when I founded the open access electronic journal, *Transnational Curriculum Inquiry (TCI)*, on behalf of the International Association for the Advancement of Curriculum Studies, and devised its peer review policies and procedures. These were deliberately designed to “enact and facilitate transnational conversations in curriculum inquiry” and “subject the process of peer review to transparent peer review”.⁴ Authors could choose to anonymise their papers if they wished, but the majority did not do so. Each referee’s signed review was circulated to the other referees, and at my discretion manuscripts accepted for publication were published together with the referees’ comments and the author’s response. Referees were also advised as follows:

In deciding whether to accept or reject an article, the editors of *TCI* advise reviewers to ask themselves if the international community of curriculum scholars is better served by publishing or not publishing the article. In the absence of compelling reasons to reject the article, the editors of *TCI* advise reviewers to recommend acceptance, because the quality of the article will be judged eventually by the scholarly community after its publication.

These modifications to conventional peer review practices encouraged some reviewers to *respond* constructively and creatively to the submissions they appraised. For example, in their response to Pauline Sameshima and Rita Irwin's (2008), "Rendering dimensions of liminal *currere*", Warren and Marg Sellers (2008: 69-70) wrote:

we envision our approach as responding to an invitation to make opportunistic interconnections ... Or, plainly put, a chance for us to join with Pauline and Rita in elaborating their ideas. For some readers this may seem a strange way to review, but our poststructural reading of reviewing calls on us to be excessive, rather than intercessive, and to contribute more than critique. That is, we choose to be both celebratory with and salutary to our colleagues ...

David Greenwood (2008: 336) draws attention to this response as being "remarkable because of how much it differs from the typical academic disagreements journals sometimes publish" and sees it as an example of "scholarly collaboration that suggests how possibilities and opportunities are lost when individual critics work at building opposition rather than connection".

Reconceptualising gatekeeping in complex networked systems

Deborah Osberg, William Doll and Donna Trueit (2008: iv) address the implications for academic publishing of new conceptions of knowledge production in their discussion of what they call "gatekeeping-as-policing" in relation to their work as editors of *Complicity: An International Journal of Complexity and Education*:

Gatekeeper-police impose (and enforce) an already existing "official" order on the system (or academic field) —i.e., an external order—which ensures that "renegade" or "sub-standard" outsiders are prevented from entering. This policing activity, however, can be considered fundamentally anti-complexity if we understand complexity in terms of self-organization and the spontaneous emergence of new dynamic forms of order from within (rather than outside of) the complex system itself. In short, the idea of externally imposed order (policing) is anathema to complexity thinking. Policing reduces complexity and inhibits emergence. It is also antithetical to research, the purpose of which is to go beyond the already known to open new spaces of thought (or forms of order) which cannot be judged in terms of the standards of the known (Osberg, Doll & Trueit, 2008: iii-iv).

Gatekeeping-as-policing assumes that the boundaries and standards of a particular academic discipline or field are well defined. However, the boundaries of many areas or foci of scholarly inquiry are fluid and open. For example, with respect to the conjunction of complexity and education as a research field, Brent Davis and Renata Phelps (2005) argue that contributions can be inter-, cross-, or transdisciplinary. Thus, Osberg, Doll and Trueit (2008) propose understanding gatekeeping in complexivist terms, which follows from new understandings of knowledge in complexivist or "emergentist" terms. The open relational logic of complexity invites us to understand all knowledge as already interconnected in complex non-linear relationships. Knowledge emerges in relational interactions and, in Gert Biesta's (2006: 47) terms, involves a "coming into presence" that cannot be predicted from any analysis of the interacting elements that facilitated its appearance. Such new knowledge cannot be assessed in terms of prior standards. As Osberg, Doll and Trueit (2008: vii) conclude:

If these are the conditions under which new knowledge comes into being, then the free (unpoliced) exchange of ideas is the primary tool by means of which such knowledge comes into being ... Gatekeeping in this regard is no longer an activity of policing, *but an activity of facilitating engagement between different forms of knowledge, different meanings*, so that something else can take place. What takes place cannot be described before it appears as we cannot know where the search for connected meaning will take us ... In this sense *gatekeepers hold the gate open*.

Not a conclusion

I share Susanne Kappeler's (1986: 212) perspective on concluding an essay:

I do not really wish to conclude and sum up, rounding of the argument so as to dump it in a nutshell on the reader. A lot more could be said about any of the topics I have touched upon ... I have meant to ask the questions, to break the frame ... The point is not a set of answers, but making possible a different practice.

Some questions that, if addressed, might make different practices possible include:

- To what extent is digital/internet publishing reproducing the norms of print scholarship rather than exploring new alternatives and possibilities in representing and performing scholarly work?
- What alternatives to conventional gatekeeping and peer review can we imagine and invent?
- How much quality control is possible and desirable given that scholarship is not only more widely interconnected than ever, but also is interconnecting in new (and complexly emergent) ways?
- What are our individual and collective responsibilities as scholars—as authors, reviewers, editors—for the directions scholarship takes in an era of open access and enhanced interconnectivity in complex networked systems?

Notes

1. Present-day editors will no doubt be astonished by Oldenburg's efficiency in producing the first issue of *Philosophical Transactions* just five days after the Council authorised its publication.
2. See <http://arXiv.org/>
3. My initial source for this quotation and the one that follows was an article in the now defunct weekly magazine, *The Bulletin* (20 February 1990: 33) which I transcribed for use in a conference presentation (Gough, 1991). James Gleick (1987: 81) also quotes Mandelbrot as saying, "I started looking in the trash cans of science for such phenomena, because I suspected that what I was observing was not an exception but perhaps very widespread". Nigel Lesmoir-Gordon's (2010) obituary also recalls that, during the 1960s Mandelbrot "scoured through forgotten and obscure journals", finding crucial clues for his inquiries in journals that were about to be pulped.
4. <http://ojs.library.ubc.ca/index.php/tci/about/editorialPolicies#peerReviewProcess>

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