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"Dispirited, often ineffectual, and in some respects corrupt"?: Re-assessing 'the invisible hand' of peer review

Abstract:

In an international academic research environment of ever-increasing scrutiny, a plethora of regulatory and evaluative frameworks permeates all aspects of university-based research publication and funding. Peer review is at the centre of the quality control of scholarly publication. Yet while peer review has been under scrutiny in the sciences for decades, and especially in individual disciplines such as medicine and mathematics, in the creative arts and humanities there has been less recognition that peer review is only one, relatively recent system of evaluation, with strengths and weaknesses, and one to which refinements can be made. This article reviews the recent literature on peer review to interrogate peer review and its variations, with the intention of beginning a conversation on possible alternatives that might be employed in the creative arts.

Keywords:

Peer review – scholarly publishing – research evaluation in higher education

Biographical notes:

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> Peer review is at the heart of the scientific process yet was until recently largely unexamined ... Peer review is slow, expensive, profligate of academic time, highly subjective, prone to bias, easily abused, poor at detecting gross defects, and almost useless for detecting fraud … The benefits of peer review are harder to pin down, but it is probably more useful for improving what is eventually published than for sorting the wheat from the chaff (Richard Smith, Editor *British Medical Journal*, 1999: 4-5).

Introduction and context

In an academic research environment of ever-increasing management and scrutiny, a plethora of regulatory and evaluative frameworks permeates all aspects of research and its dissemination. In most industrialised countries, how to measure and evaluate the level and quality of research produced by universities and other research bodies has emerged as a key higher education policy issue over the past years (OECD 1998). This is, at least in part, due to current public funding regimes that allocate money and other resources according to measurable scholarly research output and/or performance. Mechanisms for evaluation of the quality of research outputs include the Research Assessment Exercises (RAE) in the UK and Hong Kong, the Excellence in Research for Australia programme (ERA), and the Performance Based Research Funding programme (PBRF) in New Zealand. As these and other such quality assurance mechanisms are developed and refined, they are increasing in their scope, level of organisation and powers, and are consequently affecting all aspects of research activity. In such regimes, producing quality research outcomes has become core business for all research institutions including universities. Following from this, individual academics are increasingly finding that appointment, tenure and advancement depend on research success, and disciplinary credibility too rests on these accomplishments.

Although the new system for measuring research quality in Australia, the ERA programme, operated for the first time in 2010, funding for research in Australian universities has long been directly tied to research performance. Universities receive funds from the Australian Government on the basis of their relative positions in achieving outputs in: research income including competitive grants; research student load; the number of on-time research student completions; and the number of academic, peer reviewed publications. The ERA is prompted by a concern that the above mechanisms focus on the quantity, rather than quality, of the research outputs concerned (see, for instance, Commonwealth of Australia 2005: 7). This concern about a lack of quality control exists despite peer review systems already being the defining control mechanism for both scholarly publication and the award of research grants by the two major funding bodies in Australia: the Australian Research Council and National Health and Medical Research Council. Moreover, a type of peer review (in terms of examination) exists for research student completions, with external examination the norm for examination of doctoral and masters by research theses. Despite this already existing peer review, there was, in planning first for the Research Quality Framework (disbanded in 2008) and for the existing ERA initiative, almost unanimous support for another layer of peer review as a key element of the process:

> Peer review was seen as a fundamental component of an RQF. Consistent with this, there was almost full agreement that expert reviewers should be utilised both to assess research quality (including merit and impact) and broader impact. There was also very strong support for the use of international expert reviewers (Commonwealth of Australia 2005: 55).

In the ERA programme, the value of peer review is similarly reinforced, with the indicators of research quality to be "ranked outlets, citation analysis [both related to peer reviewed journals], ERA peer review, and peer-reviewed Australian and international research income" (ARC 2009: 7). Thus both the RQF and ERA are, like the RAE and PBRF regimes, at least in part based on peer review panels reviewing already peer reviewed research outputs.

When it comes to creative works that can also be considered research outputs, the ERA, like the RAE, has instituted a further level of oversight to determine the research value of those works: not only must a selection of the submitted works—such as poems, play scripts, paintings and performance—be subjected to peer review, but the producers of those works must also write a brief statement of the research content and context of the work, which is evaluated by the reviewers alongside the work itself. This seems, intuitively, a sensible approach: not only is there a lack of certainty about what it means to referee creative works *as* academic works, but also, any peer review of creative works is likely to have been based on their contributions to professional or aesthetic interests, rather than to the domain of knowledge. The ERA requirement of a research statement also serves as a very clear articulation of what is a truism in poststructural theory: that no text, whether creative object or research data, speaks for itself, but must be made to speak through translation and interpretation. We may agree that research often emerges from, and is disseminated through, creative work, but there is less agreement about the transparency of such research outcomes in those creative works. In this, the managers of the ERA process have come down on the side of writers like Nietzsche and Gadamer who insist that the text itself is mute, and must be given voice through acts of translation and interpretation. The research statements attached to the creative texts submitted to ERA produce and articulate this voice, and in the process provide the reviewers of those texts a way to frame their context, and to 'hear' their research content.

Whether we applaud the underlying principles of ERA or see it as another example of the surveillance society, whether we enthusiastically embrace the principle of art as knowledge or fear that aesthetic excellence will be crushed in the drive to achieve academic outcomes, it is clear that artist-academics will need to accommodate its imperatives for the foreseeable future. It is, therefore, important to think through some of the issues associated with the manifestation of art-as-research, and the processes—including peer review—by which this can be effected. Those committed to art-as-knowledge benefit from a more clearly articulated set of principles about how such knowledge is constituted. Those more doubtful about the benefits of this new paradigm also benefit because increased awareness may help them to develop tactics to survive in the new environment.

This article arises out of our sense of the need to explore the context more thoroughly. On reviewing the literature on peer review, it appeared to us that, apart from isolated studies, there has been little recognition of either this examination or the problems that prompt it in the creative arts and humanities. Most of discussion about peer review originates from and takes place in medicine, mathematics and other sciences. It appears that, although those in the creative arts and humanities ascribe to similar peer reviewing conventions as do those in other disciplines, scholars in these areas are less concerned with questioning the merits of, and approaches to, peer review than are those in the sciences. With respect to the creative disciplines, it is probably fair to say that there are really no thoroughly tested approaches to, or mechanisms for, the evaluation of the research rigour, content and value of creative works, other than ad hoc or post hoc measures. We suggest that there is value in adding to the growing body of knowledge about research in and through creative work, more knowledge about the institutional frameworks and mechanisms that are impressing themselves on our practice. Johnston and Krauth (2008), Nelson (2009) and the current Special Issue of *TEXT* (October 2010) discuss the challenges and opportunities, and seek to define and refine the processes, associated with ERA's requirements to document creative writing as research. This article takes one step back to interrogate and evaluate peer review itself and, necessarily, looks for the most part to research on the topic conducted outside our own field and research discipline.

In her discussion of creative writing, Camilla Nelson claims that "One of the key differences between creative writing and almost every other discipline in the humanities is that it lacks a scholarly apparatus that assigns value to published works through the process of peer review" (2009). While there are various forms of peer review in operation in relation to discrete aspects of creative writing (including the examination of postgraduate creative writing theses and the selection of works for publication), it is true to say that within the discipline of writing, the question of peer review has only become an issue in the past decade or so, following the establishment of the discipline itself in and across institutions, and the manifesto put into circulation with the 1998 publication of the Strand Report. It has been canvassed from time to time in essays in the journal *TEXT*; was brought under consideration for the short period in which the Australian government permitted the inclusion of creative works among the annual reports of research outputs, and was the subject of active discussion and lobbying while the Australian tertiary sector was gearing up for the proposed Research Quality Framework initiative. However, it is primarily in response to the ERA programme's inclusion of creative works as research that the writing academic community has begun to focus on the fit between local practice and institutional imperatives, with respect to creative research and the managing and reporting of its outputs.

Peer review is an important aspect of this process, but it is worth reflecting on the fact that it is only one system of scholarly evaluation, a relatively recent one at that, and one whose flaws are thoroughly identified in the scholarly literature. Despite this, it is an important element of the knowledge system: Barker and de Kock note that "the academic journal arguably plays its most important role in the formation and development of academic careers" (2007: 111), and it is *refereed* journals that count, within the contemporary academic economy, for the building of a portfolio of

published work that becomes an individual academic's professional capital. But peer review is not a single system: there are a number of variants in operation, each of which has strengths and weaknesses. These have been extensively canvassed, with peer review under scrutiny in the sciences, most especially in medicine and mathematics, for at least the past two decades. In 2009, the *Journal of the American Medical Association* (JAMA) and the *British Medical Journal* (BMJ) jointly held their Sixth International Congress on Peer Review and Biomedical Publication in Vancouver, Canada (the first was held in 1986). Topics included the full gamut of concerns about peer review: authorship; conflicts of interest; journal guidelines and policies; ethical concerns; the peer review process; funding/grant review; publication bias; rhetoric and spin; the effects of journal indexing, open access, the Web/Internet and impact factor; publication pathways; the quality of journal articles; and the quality of reporting of research and clinical trials (JAMA & BMJ 2009). This questioning of the efficiency and efficacy of peer review in the medical discipline is reflected in many articles that cite the "considerable evidence" (Altman 2002: 2765) that peer review has neither improved nor policed the quality of research published. There is, for instance:

> considerable evidence that many published reports of randomized controlled trials (RCTs) are poor or even wrong, despite their clear importance … Poor methodology and reporting are widespread … Serious statistical errors were found in 40% of 164 articles published in a psychiatry journal and in 19% of 145 articles published in an obstetrics and gynecology journal … there is evidence of frequent misapplication of newer advanced [statistical] techniques … Many authors interpret their findings narrowly, failing to either identify previous studies or place their findings in the context of those previous studies … Peer review can and should weed out serious methodological errors. However, expert methodological input is in short supply. Only a third of high-impact journals reported statistical review of all published manuscripts (Altman 2002: 2765-66).

While the stated aim of the *JAMA* and *BMJ* congress—"to improve the quality and credibility of biomedical peer review and publication and to help advance the efficiency, effectiveness, and equitability of the dissemination of biomedical information throughout the world" (JAMA & BMJ 2009)—is obviously focused on biomedical publication, the key issues this congress has dealt with since 1986 resonate with the peer review of research publication in the arts and humanities. What is materially required of peer review in each sector, of course, differs. Where reviewers in the sciences may focus on methodological flaws, in the arts and humanities they are more likely to focus on argumentative flaws. But in each case, reviewers and editors can be expected to focus their efforts on quality and credibility, and on the dissemination of knowledge that has been constructed in accordance with accepted principles of research process.

Peer review: applications and definitions

Peer review, when used as a noun, is usually understood to be the evaluation (review) of academic, professional or other work by others in the same field (peers). As Biggs

has noted, this contains a built in tension as it "connotes genteel collegial cooperation, while 'refereeing' suggests the boxing ring, the football field, the objective mediator pressured by impassioned opponents" (1990: 145). Biggs describes how this tension is animated in the gap between the theory and practice of peer review:

> Though both collegial and objective in theory, in practice the process is corruptible by ignorance, timidity, envy, greed, bias, and other common sins. It is this gap between the ideal and the real, coupled with peer review's extraordinary impact on scholars' professional futures and immediate feelings, that makes it so controversial (1990: 145).

In practice, when used as a verb, peer review has a number of applications. These include the evaluation of articles submitted for publication in scholarly journals, papers and other submissions (such as poster presentations) submitted to conferences for presentation, and conference papers submitted for publication in refereed conference proceedings. It is also a mechanism for evaluating grant submissions, and measuring performance in various disciplines (which commonly include teaching and medicine). It is used in some academic systems to assess and improve work units such as university departments, research groups and degree programs. The peer review process is also used to generate multiple instances of feedback on student work across disciplines, and to improve government policy, being used this way in the European Union and by the US government. Although not named as such, mechanisms in the creative arts discipline of writing such as the writing workshop and writers' group feedback processes are peer review instruments.

In terms of the evaluation of scholarly/academic/scientific publications and research grants, the peer review system is generally understood by its supporters as "a rational and necessary system of self regulatory practices designed to protect against the publication of flawed research or unsound scholarship, and to recognize and promote innovative and cutting-edge studies" (Bochner 2008: 3). It serves as a test of quality: "for a submitted article to be published, it must pass the important hurdle of being read and recommended for publication by a number of other knowledgeable people in the field of the submission" (O'Gorman 2008: 3). So much so that, in common with the various national evaluation systems, Barker and de Kock (2007) propose that peer reviewed publications (in the form of academic articles) are "the gold standard of intellectual achievement, and the index of intellectual output in a discipline" (106), a "privileged discourse … which largely constitutes 'knowledge' in the discipline" (115). Editor of *Sociology of Religion*, David Yamane, although admitting that the "peer review process is … not perfect", describes peer review as having a "legitimating function" in each discipline (2007: vii). The Mathewson-IGT Knowledge Centre [library] at the University of Nevada thus represents that: "articles accepted for publication through a peer review process implicitly meet the discipline's expected standards of expertise" (2008). In 1986, John S. Rigden, then editor of the *American Journal of Physics*, suggested that this expertise was wider than just the content of an article as:

> The quality of the writing, the perceived significance of the thesis developed, the inherent interest of the subject, the appropriateness of the context chosen for the subject, and the treatment of the subject-context interaction all influence a recommendation (Rigden 1986: 491).

In a recent report, the British Academy posited that peer review "is used to judge the quality of research *and writing* in many disciplines" (British Academy 2007: iii; emphasis added).

Recent research has shown that academics across a range of disciplines believe that the peer review of a range of scholarly publications (principally books, book chapters, journal articles and conference papers) is the foundation of scholarly communication. In 2008, a sizeable survey of US academics found peer review was widely believed to be the "essential component of scholarly communication, the mechanism that facilitates the publication of primary research in academic journals" (MWC 2008: 1). Although seeking to address issues relating to the current practice of peer review for the purposes of publication in journals and awarding research grants, in their 2007 report on peer review, *Peer Review: The Challenges for the Humanities and Social Sciences*, the British Academy firmly endorsed its use:

> The high international reputation of UK research in the humanities and social sciences is based on the rigour with which peer review is used, and the care with which practical decisions to fund or publish are based on it (British Academy 2007: iii).

Others note flaws in the peer review system, but express confidence in it nevertheless. Coley, for instance, suggests the peer review of scholarly publications is a "broken" system because of the (often) lengthy timelines involved between submission, review and publication but believes "like the majority in our field, that peer review (even, and especially, blind peer review) provides certain checks and balances, collaboration, and prestige to our publications" (2008: 326). Similarly, although O'Gorman finds peer review of publication "frustrating" due to a lack of people willing to take on the work of reviewing (2008: 3) and, as a result, the same reviewers reviewing many papers and distorting what is understood as acceptable quality in some smaller, specialist fields (2008: 4), he also finds that, "the system ultimately works well for readers of these papers. This is because the quality of papers directly correlates with the quality of the peer review process—even as it is today" (2008: 3).

Peer review's history

Despite such endorsements, it is rarely noted that it is only relatively recently (post second world war) that the formalised editorial peer review system has become the norm for publication verification in academic journals and other scholarly fora. Burnham's groundbreaking work on the history of peer review revealed not only that almost no historical account of its development existed to that point, but also that editorial peer review did not evolve from the peer review processes that had preceded it (for reviewing grants) nor did editorial procedures spread in any orderly way through scholarly publishing once it had become a practice (Burnham 1990). Although the origins of the concept of peer review, in terms of the referee system, have been traced back to the seventeenth century by some (see Zuckerman & Merton 1971), and informal one-off refereeing processes were occurring in the nineteenth century (Burnham 1990), individual journal editors largely solely made the decision of what was to be included, or not, in scholarly journals before the 1950s. Tipler cites the example of Einstein's Nobel Prize winning ideas—ideas that were published in

major German physics journals but not peer refereed—meaning that either the journal's editor-in-chief or the specific subject editor made the decision to publish (2003: 3). It is important to remember in association with this practice that, before the current 'knowledge explosion', such editors could have a good, or at least reasonable, handle on entire fields of knowledge. Tipler, moreover, outlines how, in the 1950s, so few articles were submitted to physics journals that very few were rejected, and if they were, there were publication venues (some scientific society journals, for instance) where every paper written by a member received publication. In this system, where "there was no way in which referees or editors could stop an idea from appearing in the professional journals" (Tipler 2003: 3), what operated was a kind of post-publication review where these ideas and theories, once published, would then be discussed and either credited or discounted by members of the field. However, as universities increased in numbers and expanded the range of subjects they offered, so too did the numbers of academics, research higher degree students, disciplines, specialist areas within disciplines and research methodologies. The result has been an explosion of publication and the knowledge embedded in these publications, over which no single editor could ever claim expert status. From the necessary practice of calling upon the assistance of colleagues and associates (who evolved into associate editors) who then, in turn, called upon wider assistance, the system of peer review evolved.

Criticisms of peer review

There are many criticisms of the peer review of publications. A large study in 2002 by Jefferson *et al.* asserted that editorial peer review, although widely used, is not only largely untested, but has uncertain effects. Ware's study of US academics found peer review to be:

> unreliable, unfair and [that it] fails to validate or authenticate; that it is unstandardised and idiosyncratic; that its secrecy leads to irresponsibility on the part of reviewers; that it stifles innovation; that it causes delay in publication (MWC 2008: 1).

While others identify multiple issues, a series of themes emerge in these criticisms. Richard Smith, past editor of the *British Medical Journal*, identified that refereeing is "expensive, inaccurate, subject to bias or worse, doesn't catch errors let alone fraud—and there is no objective evidence that it works better than the independent judgment of a qualified editor" (qtd. in Bochner 2008: 4). Richard Horton, editor of the British *Lancet*, agrees, stating that "peer review is biased, unjust, unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish, and frequently wrong" (Horton 2000). Other reviewers of peer review concur: "peer review and refereeing are moribund. They have become dispirited, often ineffectual, and in some respects corrupt, infested with politics, rife with temptation to plagiarize" (Judson 2004: 7). This latter point has been identified by Calabrese and Roberts (2004) who categorise the plagiarism of others' works during the refereeing process as part of an environment of increasing levels of academic misconduct in relation to academic publishing, becoming especially prevalent as academic appointment and promotion become increasingly linked to publication output.

Other criticisms of referees focus on the low standard of referees more generally. In 2003, Bedeian reports on a survey of 173 lead authors of articles published in the *Academy of Management Journal* and *Academy of Management Review*, over the period 1999 to 2001. Among the survey's more disturbing findings, more than one third of the responding authors reported that recommended revisions in their manuscripts were based on an editor's or referee's personal preferences, and almost 25% indicated that in revising their manuscripts as required for publication they had actually made changes they felt were incorrect (2003). Bornmann and Daniel (2009) have investigated biases on the part of reviewers and editors by looking at the peer review process at a single journal: *Angewandte Chemie International Edition*. The results showed that the number of institutions mentioned in the Acknowledgements' section of the manuscript, the share of authors having institutional affiliations in Germany, the institutional address of the referee (in Germany or not in Germany), and whether the author suggested a referee for the manuscript or not, had statistically significant effects on the referees' recommendations—with the number of institutions that are mentioned in the Acknowledgements and the share of authors having institutional affiliations in Germany potential sources of bias in the editors' decisions.

Reliance on "reciprocal altruism"

With a recent UK study estimating the value of unpaid peer review activity worldwide as some £1.9 billion (with some £165 million of this activity occurring in Great Britain) (Corbyn 2008: 17), it is no surprise that academic publishing's reliance on what has been identified as "reciprocal altruism" (Birkhead 2005: 62)—referees' sense of "professional obligation, generosity, and good humor" (Biggs 1990: 149)—has been identified as a weakness in the system. The attendant "fatigue factor" (Bochner 2008) for reviewers who suffer "request fatigue" (Birkhead 2009: 29) means that some reviewers withdraw their services and refuse to complete reviews. Senior academics in some disciplines report receiving several requests to review manuscripts each day (Birkhead 2009: 29) with the result that "a high and interesting duty has become a wearisome … endless and onerous chore" (Judson 1994: 92). This reviewer exhaustion poses a threat to the integrity of the whole system, which is grounded on the premise of the highest level of not only expert and objective, but also thoughtful, assessment.

The national evaluative mechanisms recently instituted have increased the institutional imperatives regarding publication and have led to growing numbers of manuscript submissions to journals. Brady and Krauth identified this: "Whatever way the particular institution divides it up, the publication of a refereed article equates to expendable currency" (2005). This, together with the growth of academic publishing noted above, has resulted in an increase in the production of scholarly articles "by more than a factor of a thousand over the past fifty years" (Tipler 2003: 2). The large number of submissions has had the flow-on effect of rising backlogs and bottlenecks in journal production processes (Pannell & Williams 2003). At the same time, those (often academics) involved in producing these journals in countries where these evaluative systems operate are increasingly finding such work classed as community or professional service—definitely a 'second rate' category after research and

teaching in terms of academic appointment or promotion. In many cases, the already largely nominal allocations of time for such work are in the process of being slashed or withdrawn completely. In an article that represents the views of a number of journal editors and experienced reviewers in the field of marine science, Hans Ulrik Riisgård notes that while referees are the backbone of quality control and need more recognition for their work, their ranks should be increased by compelling more article writers to become reviewers:

> It should no longer be 'free' to submit a manuscript to a scientific journal. While cash payment for reviews is not considered a good idea, a 'payback in kind' system is favored: i.e., if you want to submit papers to a journal you must be willing to review for that journal (2000: 305).

A number of contributors to this article calculate this "payback in kind" precisely: as authors performing the number of reviews for a journal they expect that journal to provide for their own work. There was, however, recognition that this responsibility will be spread over a career, with early career researchers reviewing less, and established researchers making this deficit up later in their working lives. A number of other writers on this subject agree, with O'Gorman, for instance, calculating that: "If an author submits n papers per year, then at a 3-reviewers per submission rate, that author should be reviewing 3n papers" (2008: 4). There also should be penalties for not following this rule with those "who consistently refuse to review mss should not expect to have their mss reviewed by others" (Riisgård 2000: 312).

Bringing firm and responsible editorial judgment to bear in restricting the number of submissions that are sent out for peer review is another suggestion for reducing strain on reviewers and is widely practiced in some, but not all, disciplines in the creative arts and humanities. The 2007 study by the British Academy found that such a prescreening process whereby unsuitable papers are returned to authors was "broadly typical" across the social sciences and humanities (Croll 2007: 347). However, the President of the US National Communication Association (NCA), Arthur P Bochner, only recently proclaimed that their association should "allow (and encourage) each NCA journal editor to exercise greater discretion in rejecting manuscripts they deem unsuitable without sending them out to referees for review" (Bochner 2008: 4). While undoubtedly reducing the burden on individual referees, this could well worsen another issue (of lengthening timelines to print, and the slowing down of the dissemination of new knowledge), as this shifts a larger percentage of the refereeing burden to a smaller number of journal editors. Dean has argued that these lengthening timelines are increasingly making humanities journals "irrelevant":

> The massive delays that many humanities journals permit irritate authors and diminish the utility of the papers they publish. Indeed, in spheres such as socioeconomic policy, authors often release "work in progress" as pamphlets and web documents during such delays, which may further undermine the utility of the eventual journal article (Dean 2004).

This may be somewhat overstating the case: few topics in the humanities are as time dependent as this seems to suggest, and there would be very few cases where the utility of the knowledge packaged in a humanities essay would fall in value; though

certainly the delays can irritate authors. A more serious issue is the pressure on journal editors, who must secure, usually, two appropriate academic referees for every submission deemed relevant to the journal; liaise with those referees; moderate and package the reports; and then communicate and negotiate with the authors—all prior to the work of copyediting, proof reading, page make-up, production of the journal as a whole, and finally its distribution. Feng-Nian Wang is one of many who have identified the specific problem of editorial overload:

> Obviously the success of a journal is inseparable from the creativity of its editors. But in real life, editors are so swamped with editing, reviewing, and proofreading that they may become set in their ways and unwilling to take risks on new ways of thinking, working, or editing. When the selection of topics, the soliciting of submissions, and the revision of papers are all approached in new ways, the result is more creative papers and journals (Wang 2008: 414).

Writing about *TEXT* journal, editors Tess Brady and Nigel Krauth assert that their core concern was to make the journal "interesting, lively and scholarly publication which asks questions in the discipline and which helps to set out research paradigms". However, the role of editor is also concerned with far more foundational issues, such as developing and cultivating authors and referees: "the journal is healthy if there are sufficient scholars to write for it and sufficient scholars to act as referees. Both need to be nurtured, our editing role juggles these balls" (Brady & Krauth 2005).

To blind or not to blind?

Peer review is employed to ensure quality manuscripts, free of errors. What is known as 'pre-publication' refereeing can be 'open', 'single blind' or 'double blind'. In the open system, both authors and referees are identified to each other during the process. In the single blind system (sometimes just known as 'blind'), authors are identified to reviewers, but reviewers are not identified to authors. This form is commonly followed by journals in the sciences, including the influential journal *Nature*, for instance. In the double blind method, neither reviewers nor reviewed are informed of each others' identities. The double blind system is meant to ensure objectivity and the fairness of the process (see Bochner 2008: 3) and minimise any potential conflict of interest for reviewers. Double blind review is the standard in the creative arts and humanities in Australia.

However, the practice in both blind and double blind refereeing of shielding the referee's identity has been questioned since at least the early 1990s (see, for example, Biggs 1990: 148-50). Many have advocated open review for the very reason of increasing the thoroughness and timeliness of reviews, and the respect with which they deal with the texts and authors. It also stems any propensity for intellectual property theft on the part of referees, by stripping away reviewer anonymity. It also requires that they stand behind their position on the essay, and take responsibility not only for their comments, but for the tone in which those comments are conveyed. It is relevant to note here that most published book reviews are signed by the reviewer. In 1994 Judson, in *The Journal of the American Medical Association,* predicted an end to the peer review system due to the corruptions inherent in it, most notably because

the chief competitors in a given research area often are the ones most qualified to review work on that subject and, therefore, the most vulnerable to the temptations of stealing intellectual property (Judson 1994: 92). This situation, of course, also makes such reviewers also the most likely to delay the publication of work that competes with their own. There is little evidence that this is in fact the case, and in any event, journal editors should be able to mitigate any such effects; in addition, in the arts and humanities sector, which is notoriously under-funded and unlikely to generate quantities of patentable or otherwise commercial knowledge, it is hardly an issue. Of more concern is Biggs' point that "substantial numbers of peer reviews are compromised by prejudice, ignorance, carelessness, hurry, or uncertainty or misapprehension about the journal's values" (Biggs 1990: 158). Again, journal editors should provide sufficient information to ensure that reviewers do understand the journal's values; and ideally have sufficient knowledge of the sector that they would avoid sending work to referees who possess those more negative qualities that Biggs lists above. However, undoubtedly there are referees reports that are presented in a way that is really not useful for evaluating the quality of knowledge and writing in a submitted work. Possibly formal training for editors and reviewers would improve standards, and perhaps go some way to counterbalance the effects of lack of time or lack of understanding of the work of a referee, but there is no evidence that this happens other than through informal mentoring, or in isolated discipline examples.

Since 1999, after a series of trials to find out whether either double or single blind review improved the quality of peer review (and finding it did not), the *British Medical Journal* has opted to utilise a completely open peer review system, giving the reason then that blind peer review was leading to poor research being published and so was "introducing open review—for largely ethical reasons" (Smith 1999: 4). Drummond Rennie, Deputy Editor of the *British Medical Journal,* in 1999 argued that open review identified the reviewer's

> privilege and duty, by reminding the reviewer that with power comes responsibility: that the scientist invested with the mantle of the judge cannot be arbitrary in his or her judgment and must be a constructive critic (qtd. in Smith 1999: 5).

In the humanities, the President of the US National Communication Association (NCA) followed this a decade later: "We simply cannot justify the practice of allowing individuals to make important and decisive judgments about other people's work in secret and without accountability to those they judge" (Bochner 2008: 4). His recommendations were to initiate new guidelines for NCA journals. These included removing any blindness in the system:

> Identify the names of the authors to reviewers; Identify the names of reviewers to authors and include the entire submitted and signed review when corresponding with the authors; Include the names of reviewers on all articles and monographs published in NCA journals (Bochner 2008: 4).

Publishing referees' names alongside articles, and thus effectively giving them joint responsibility for any errors in the text, is advocated by a number of studies. Godlee argues strongly that open review is "ethically superior" and has the potential to balance "greater accountability for reviewers with credit for the work they do" (2002:

2762). This point of credit is well made as, without this, the work of referees is largely invisible. Godlee notes that the barriers to open review are less persuasive, based as they are on the conservatism of the research community, and that openness "makes editors publically responsible for their choice of reviewers and their interpretation of reviewers' comments" (2002: 2762). It is, however, worth considering whether academics would be prepared to review submissions, or to be sufficiently critical and analytical in their review reports, if they knew that their comments would be publicly available and attributed to them. Particularly in the smaller disciplines (creative writing, for instance), a negative report of even a demonstrably poor contribution could result in social and professional tensions, and it would be unsurprising if prospective reviewers chose not to put themselves in that position.

Another complex issue related to peer review is the charge that refereed journals enforce prevailing beliefs and attitudes, and stifle innovation:

> Today, the refereeing process works primarily to enforce orthodoxy … "peer" review is *not* peer review: the referee is quite often not as intellectually able as the author whose work he judges. We have pygmies standing in judgment on giants (Tipler 2003: 2)

Tipler cites examples of Noble Prize winners and others (including the developer of chaos theory, the inventor of the laser, and Stephen W Hawking's ideas about black holes) who could not get papers outlining their revolutionary theories or discoveries published in peer refereed journals (2003). *Lancet* editor Horton agrees that peer referees test the *acceptability*, rather than the *validity*, of content (Horton 2000). In 1978, Henry Small suggested that each academic paper was a "concept symbol" that came to be renowned and cited (or not) for one message, theory, finding or idea. However, that one concept has to get past the gatekeepers, that is, it has to be accepted by the peer reviewers.

In contrast to the pre-publication review, which much of the above discussion has concentrated on, there is a fourth type of peer review—'post-publication review' whereby material is published and then reviewed by readers and others who offer comment and correct any errors. In the online information commons, where information can be freely shared, a collaborative system sometimes called 'open group review' is becoming more attractive to many (van Rooyen *et al*. 1999). This can also operate alongside pre-publication review. Afifi (2006) argues that the letters section of a scientific journal is an essential part of post-publication peer review as it provides a mechanism to identify unsound work that has, despite this, been accepted for publication. Some online publications have a mechanism whereby letters and any subsequent corrections are identified when the article is retrieved, but others do not, placing the responsibility on tracing any such corrections onto the reader. While this approach has an organic validity, it is most unlikely to be an effective approach: there are, typically, so many submissions to any journal, and so many of those are likely to be first drafts, or irrelevant to that journal's interests, and therefore not publishable even under the logic of post-publication review. This approach is also likely to place even more work on the shoulders of the editors, because they would have to vet each submission, and at least copy edit and complete the page make-up, all without any confidence that the work would add value to the journal and/or to the discipline.

All the same, such mechanisms are developing and evolving alongside more collaborative wiki-type cyclic modes of continuous authorship and review. Collapsed forms of writing, review and dissemination, such as collaboratively composed and edited web-based wikis, it is argued (see, for an example, Ware 2005) not only utilise more sophisticated systems of creation, inspection and validation but are, moreover, more democratic and in line with twenty-first century modes of understanding authorship and knowledge transfer. Although such newer forms are not preferred by all, they do have benefits, for instance, in many cases offering significantly more timely, iterative and useful responses for authors to consider (MWC 2008: 4). Pöschl and Koop (2008) argue that collaborative forms of peer review facilitate and enhance the overall quality of final papers, as well as maximising the value of referees.

Conclusion

While there are many factors that influence authors in their choice of refereed journal for manuscript submission, there are common factors across disciplines. Authors consider variables including the focus of the journal and how well it matches the topic of the manuscript, the timeliness of the editorial process and whether feedback is constructive, as well as the journal's visibility and accessibility and any costs to the author for publishing (Thompson 2007: 126). In today's research environment, the journal's ranking or impact factor are also important for authors to consider. The high status of publications such as *Nature* and *Science* is at least in part evidenced in their very high rejection rate for submitted papers—so much so that to have even a note published in one of these journals is considered a mark of excellence. Of course, the quality of a journal is predicated on far more than the proportion of submissions accepted or rejected; but the higher its ranking, the more submissions it is likely to receive, the more it is therefore likely to reject, and the more responsibility is placed on both editors and referees to make the best choices for inclusion and rejection.

The input of referees is not universally considered significant in maintaining quality. Tipler has advocated that leading journals establish a "two-tier" system, the first being the usual referee system, the second offering an alternative that enables the dissemination of new ideas. The new tier does not overturn peer review, but rather outsources the task of finding referees to authors, requiring them to obtain recommendations to publish from leading experts in the field. Once these are presented, the article would be accepted and published. This procedure has precedents.

> Crick and Watson followed this procedure in the case of their famous paper on the double helix structure of DNA. The paper was never sent to referees … Instead the paper was submitted to *Nature* with a "publish" covering letter from Sir Lawrence Bragg, the head of the Cavendish Laboratory at Cambridge University, and also a Nobel Prize winner (Tipler 2003: 11).

We can, however, see many problems with such a system. Not least is that such experts would be bombarded with a tsunami of papers to endorse, but also that this methodology—where a few figures have the power of proclaiming publication or not—has potential for corruption or, at least, of causing more backlogs and delays.

As early as 1989, Austin proposed that the "custom of vetting"—having colleagues read and comment on drafts of a paper before submitting it to a journal—could be a substitute for peer review. Barker and de Kock, indeed, describe how in South Africa a number of academic literary journals in the period from 1958 to 2004 "applied vetting procedures involving peers in the selection of articles" (2007: 110). This would be the case for many journals in many countries during this period. But it is not actually or necessarily the answer to problems with the peer review process. Austin suggests that although it appears to offer genuine value, in fact, "The unique self-serving twist is that each writer is completely in control of the vetting. Each writer has the discretion to select and publicly identify those "referees" that can most effectively furnish creditability or some other benefit." He continues, rather damningly:

> Public listing of vetters invites self-serving, and hence ethically doubtful, manipulation. The vetting custom can be used by established professors to build or expand a private network of followers by gratuitously including young people in the list of vetters. The Author does not expect, or get, serious review from these people but instead exchanges recognition as a vetter for fealty. Likewise one can become part of a network of referees by citing friends, who are then honor bound to reciprocate. Similar to footnote reciprocity, it is the "academic equivalent of I'm OK, You're OK" (Austin 1989).

Which leaves us without a clear way forward, when it comes to the system of peer review. It seems simply too flawed to produce the expected outcomes: improved quality of research and its dissemination. But other systems have their flaws too, as was made evident in the so-called Sokal Hoax of 1996. Alan Sokal, a professor of physics, submitted an article to the cultural studies journal *Social Text*, quite deliberately to expose what he saw as the journal's (and, by implication, the humanities') lack of intellectual rigour. In the opening paragraphs of the article he writes, "It has … become increasingly apparent that physical 'reality', no less than social 'reality', is at bottom a social and linguistic construct" (Sokal 1996a). A careful reader might hear alarm bells at this stage, but he immediately continues, listing other "apparent evidences":

> that scientific 'knowledge', far from being objective, reflects and encodes the dominant ideologies and power relations of the culture that produced it; that the truth claims of science are inherently theory-laden and self-referential; and consequently, that the discourse of the scientific community, for all its undeniable value, cannot assert a privileged epistemological status with respect to counter-hegemonic narratives emanating from dissident or marginalized communities (Sokal 1996a).

It is easy to see how his use of these 'truisms' of cultural studies might overshadow any concerns about the non-reality of reality in the preceding sentence; especially coming from a well-credentialed physicist; especially when editors would not expect to receive a disingenuous or malevolent submission. Presumably the editors of *Social Text* did not expect this; and it neither checked the facts in the journal, nor sought an opinion from an expert in physics. Rather, its editors accepted the science and the social argument at face value. Sokal made their error of judgment very public in a number of subsequent pronouncements and publications, as for instance, in *Lingua Franca* where he states:

> I intentionally wrote the article so that any competent physicist or mathematician (or undergraduate physics or math major) would realize that it is a spoof. Evidently the editors of *Social Text* felt comfortable publishing an article on quantum physics without bothering to consult anyone knowledgeable in the subject (Sokal 1996b).

While we might find his approach to the humanities in general, and *Social Text* in particular, self-serving, an instance of a mismatch of discourse rather than a flaw in editorial processes, or part of the sibling squabbles between the sciences and the humanities, there is a serious point at the heart of this hoax. Sokal articulates this:

> Of course, I'm not oblivious to the ethical issues involved in my rather unorthodox experiment. Professional communities operate largely on trust; deception undercuts that trust. But it is important to understand exactly what I did. My article is a theoretical essay based entirely on publicly available sources, all of which I have meticulously footnoted. All works cited are real, and all quotations are rigorously accurate; none are invented. Now, it's true that the author doesn't believe his own argument. But why should that matter? The editors' duty as scholars is to judge the validity and interest of ideas, without regard for their provenance. (That is why many scholarly journals practice blind refereeing) (Sokal 1996b).

"The editors' duty as scholars": this is where Sokal lays the blame; and it is difficult, as scholars, to argue with him on this point. The editors are the gatekeepers, for any journal; and they are also the public face of the journal, the ones bearing responsibility for its quality and its intellectual value. In the case of *Social Text* this is inevitable; it was not a peer-reviewed journal, and the decision to assess the merit of the submission, and then to publish it, rests with the editors alone. It is possible that if the journal had relied on peer reviewers, the simple act of selecting referees with appropriate credentials—or indeed, simply ensuring that there were fresh eyes cast over the work and, moreover, that the reviewers were genuinely 'blind' to the contributor and thus not 'blinded' by his credentials as a physicist—might have changed the outcome of this hoax. Indeed, it is noteworthy that Sokal chose to target a non-refereed journal, rather than another cultural studies journal that subjected its contributions to peer review.

Of course, there are problems with the peer review system, whether single or double blind. But we reflect on Winston Churchill's dictum: that "democracy is the worst form of government except all those other forms that have been tried" (in the UK *Hansard* of 11 November 1947). In the light of the Sokal hoax, and of other problems in the dissemination of published material, perhaps it is reasonable to conclude that peer review is the worst approach to quality control in knowledge production, except for all the other forms whether vetting, editorial fiat, post-publication review or wiki-style publication. Certainly peer review is flawed; certainly it does not necessarily, of itself, produce quality. But it is an important element of the machine of knowledge production. It behoves all scholars to critically assess it from time to time, to find ways to refine and refresh it, and to retune it so that it will meet the different needs and imperatives of different disciplines at specific moments in history. What this means for the academic field of creative production is the subject of another paper.

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