

## **Peer reviewing: privilege and responsibility**

### Author

Johnston, Jane, Krauth, Nigel

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## Jane Johnston and Nigel Krauth

### *Peer reviewing: privilege and responsibility*

#### *Abstract*

*Peer review is a central tenet in research across all disciplines. It is a key feature in monitoring the advance of knowledge, especially in academic publishing. This article investigates the development of peer review from the seventeenth century to the present, and analyses significant aspects of the process. It also attempts to clarify some criticisms and make suggestions about the role of peer review in the current climate.*

The Rudd government has announced a new system for recognition and quantification of research in Australian universities. The ERA (Excellence in Research for Australia) system is slated to replace the struggling Research Quality Framework (RQF) of the previous Howard government. Subject to the Australian Research Council approval of a consultation document, the ERA proposal will be circulated to universities and research stakeholders for comment. In this context, it is timely to consider one of the central tenets of the research process: peer reviewing.

#### **Historical development of the peer review process**

The peer review process has its genesis in scientific journals. Henry Oldenburg, the founding editor of the pioneering British scientific journal *The Philosophical Transactions of the Royal Society* established in 1665, is recognised as the earliest journal editor to articulate the need for peer review: 'Oldenburg wrote of grappling with the vexing problems of ensuring authors' intellectual property and vetting their contributed papers' ([Zuckerman and Merton 1986](#)). Prior to this, secrecy characterised seventeenth-century scientific publishing:

At that time, many scientists sought to keep their work secret so that others could not claim it as their own. Prominent figures of the time, including Isaac Newton, were loathe to convey news of their discoveries for fear that someone else would claim priority - a fear that was frequently realized. ([Committee on Science, Engineering and Public Policy 2005](#))

Oldenburg's method used the judgement of peers in the Royal Society as a validating mechanism and also as an official record of original authorship. From its inception peer

review was used as an instrument to distinguish scientific journals from book publishing, ensuring quality control and standards had been met before publication actually took place ([Tobin 2002](#)).

Exactly how peer review further developed appears sketchy. American historian JC Burnham has found:

Practically no historical accounts of the evolution of peer review exist. Biomedical journals appeared in the 19th century as personal organs, following the model of more general journalism. Journal editors viewed themselves primarily as educators. The practice of editorial peer reviewing did not become general until sometime after World War II. Contrary to common assumption, editorial peer review did not grow out of or interact with grant peer review. Editorial peer review procedures did not spread in an orderly way; they were not developed from editorial boards and passed on from journal to journal. Instead, casual referring out of articles on an individual basis may have occurred at any time, beginning in the early to mid-19th century. Institutionalization of the process, however, took place mostly in the 20th century, either to handle new problems in the numbers of articles submitted or to meet the demands for expert authority and objectivity in an increasingly specialized world. ([Burnham 1990](#))

The development of scientific research was based on several key values within the context of seventeenth-century research. These values made up a system described by Merton as the 'scientific ethos' (see [Merton 1949](#)) upon which research was validated. The values - universalism, systematic scepticism, ethical neutrality, communalism and disinterestedness - underpinned this ethos. However, in his article titled 'A dissenting view on the scientific ethos', published in the *British Journal of Sociology*, Rothman suggested a questioning of these values' strengths, arguing that they are flawed ([Rothman 1972](#)). His argument may be summarised thus:

- *Universalism*: this value is based on the understanding that there are universal criteria for scientific knowledge, based on technical norms; BUT it tends to favour the elite level of scientist while discriminating against less-resourced, less-famous researchers.
- *Systematic scepticism*: it is by this means that science protects itself from fraud through careful scrutiny and validation; BUT this does not provide scope for those who use non-conventional methods of research. In addition, scepticism can be dispelled due to the 'Matthew Effect' that refers to recognition based on eminence rather than merit.
- *Communalism*: this value is based on the idea of sharing knowledge; BUT the competitive environment of universities can work counter to it. In addition, closed 'invisible colleges' can emerge and result in inadequate refereeing and scrutiny based on small, elite groupings which look after themselves.
- *Ethical neutrality*: is described as a 'no-hold-barred' approach which should see research proceed, despite the sensitive nature of an issue; BUT responses may be made based on the moral or social position of the work rather than its merit.
- *Disinterestedness*: is based on the idea that research is not undertaken for personal recognition or gain, but rather for the communal good; BUT with professional and public recognition being part of the reality of research, particularly in seeking future funding, this is unrealistic. Taken to the extreme,

in seeking recognition, researchers may seek publication in non-refereed newspapers first and thus be totally 'interested' in furthering their own careers.

As part of his critique of the scientific ethos, Rothman notes an insightful observation made in a letter to a 1966 edition of the journal *Science*:

"The work in laboratories is less gay now; the enthusiasm is being misplaced, from acts of discovery to the work of quick publication. The practice of science is becoming less for its own sake than for the advancement of scientists. A slow terror is descending upon us, compounded of fear and pride and envy, of hate and waste and misguided zeal, of lacks of joy and satisfaction; let us stop this before it becomes complete." (Siefevitz cited Rothman 1972: 106)

This perception, made 40 years ago, is still relevant in the competitive, pressurised research environment of the Australian Research Quality Framework (RQF) and the new ERA (Excellence in Research for Australia) to follow it. The idea that there has been a shift 'from acts of discovery to the work of quick publication' resonates in the current system that requires researchers annually to publish peer-reviewed pieces as journal articles, book chapters and monographs. Indeed, it has been argued (of scientists) 'without the production of scientific papers, a scientist ceases to be a scientist' (Price cited [Lindsey, 1979](#)). Academics in all fields are now subject to the requirement to publish.

### **Contemporary peer review**

Emerging alongside the importance of peer-reviewed publications has been the growth in the importance of the peer reviewer her/himself - the peer who must evaluate, critically review and respond to the work of another. By definition, they too will be a researcher and author, with their own work in the publication cycle. Judson notes of the role: 'although peer review and refereeing seem rational, indispensable, and immutable, the histories demonstrate that they are social constructs of recent date. They are not laws of nature, nor of epistemology. They have changed and evolved' ([Judson 1994](#)). They are subject to the pressures of the contexts of the time.

The peer review has changed and evolved yes, but not, it would seem, in any systematic way. Analysts (e.g. Burnham 1990, Tobin 2002) agree that guidelines and processes have emerged *ad hoc*.

For a component of pivotal importance to the progress of science, journals provide scant guidelines to the reviewers. The confidential and anonymous nature of editorial peer review makes it especially difficult for the novice to learn the skill. (Tobin 2002)

So new academic writers face difficulties in having their work reviewed and in reviewing the work of others particularly because the review process is done in isolation - i.e., it is carried out away from the journal, as a private confidential activity, and then submitted. Compounding this is the pace required within the strictures of the publishing process which comprises: *researching, writing, sending for submission, journal editors' screening and identifying the best reviewers, seeking review from reviewer, receiving feedback from*

*reviewer, sending back to the author and quite possibly beginning the cycle again because feedback from the reviewer requires change to the piece.*

New reviewers - and new contributors - are faced with an array of challenges, not least of which is their limited writing experience. Putting one's work forward for refereeing is like playing chess with one's ego - advancing one's pawn into the maw of scholarly battle. Busy old-hand reviewers are not necessarily blessed with a generosity of spirit, and may treat pieces harshly. On the other hand, newly engaged referees may find their reports ignored by editors, for reasons of lack of skill. Writing a review, as with receiving one, involves skills of astuteness and nuancing. This is due to the complexity of the academic publishing process and its professed responsibility to the advancement of knowledge.

A very useful article, 'How to review a paper' in *Advances in Physiology Education*, provides the following etiquette:

The reviewer should write reviews in a collegial, constructive manner. This is especially helpful to new investigators. There is nothing more discouraging to a new investigator (or even to a more seasoned one) than to receive a sarcastic, destructive review ... No one likes to have a paper rejected, but a carefully worded review with appropriate suggestions for revision can be very helpful. ([Benos et al 2003](#))

There are many anecdotes to prove this advice often goes unheeded. Take for example the following comment, offered in response to a paper written by a PhD student in a Queensland university on her second attempt at academic publishing: 'The paper serves no purpose'. The comment, along with the rejection in the summary section of the reviewer's form, came complete with a typo that indicated the haste in which the review had been put together and sent. (The PhD student has since become a tenured academic at a leading university and learnt much about how not to peer review from this response.)

But this case begs the question: how closely is reviewers' feedback monitored? If reviewers are tardy in their responses, or worse, nasty and unhelpful, are they cast from the list of a journal's future reviewers? One view is that the online system of internet publication has enhanced the rigour of peer review. Editor of the *American Journal of Respiratory and Critical Care Medicine* Martin Tobin notes that the journal has 5,600 reviewers on their database, covering 172 fields of research, with new reviewers regularly added and 'delinquent or superficial reviewers' noted. He adds that the timeline between submission and the first review is 33 days for online peer review and applauds this move to electronic expediency. 'The internet is revolutionising the speed of processing manuscripts ... but the bedrock of science has not changed since the 1660's: experiments are converted into science only after the results have been published in a peer-reviewed journal' (Tobin 2002).

In 2005 the developing RQF system in Australia (to be replaced by ERA from 2008) cautiously asserted the importance of peer review in validating research in the academic publishing process:

Universities currently receive block funds from the Australian Government on the basis of their relative positions in performance-driven formulae comprising research income (including competitive grants); research student load/number of student completions; and number of academic (peer reviewed) publications.

However, there is concern that these mechanisms, particularly the latter, do not sufficiently encourage a focus on research quality, including research impact. ([Commonwealth of Australia 2005: 7](#))

This questioning of the peer review process in journals led to the following:

Many metrics used in the assessment of research impact are of course underpinned implicitly by some element of peer judgement. For instance, in the case of a metric like numbers of publications, there would normally be some involvement of peers in assessing a paper/book for publication, although the degree of rigour in the assessment process would vary considerably depending on the nature of the journal/book publisher. An RQF is predicated on the assumption that there is a need to develop a more consistent and comprehensive approach to assessing the quality and impact of research through the development of more sophisticated quality measures for research than currently exists. The Expert Advisory Group believes that a peer review component is fundamental to a robust RQF. (Commonwealth of Australia 2005: 10-11)

We await guidelines for 'a more consistent and comprehensive approach to assessing the quality and impact of research' and 'the development of more sophisticated quality measures for research than currently exists'. In the meantime, we offer the following discussion and make some suggestions regarding the next phases in the development of peer reviewing.

### **The peer review process**

There are four elements that make up the total picture of the peer review process in the contemporary research environment:

1. the researcher/author seeking peer review (Writer)
2. the role of the reviewer (Reviewer)
3. the philosophy of the journal publishing - or rejecting - the research (Journal)
4. the expectations of the discipline for which the paper is written (Discipline).

The writer of the article must go through all the filters - numbers 2 through 4 above - in seeking publication. The journal sets the benchmark for the writer and the reviewer and often reflects the wider community of the discipline, although all three are closely entwined. We will deal with each of these points in an order that identifies the journal (number 3) as a crucial pivot point in the mix.

#### ***Journal: The philosophy of the journal***

There are many aspects that constitute a journal's philosophy (including how it comes to that philosophy, whether by an elected editorial board, a local managing committee, or the influence of a powerful individual). Key aspects involve:

1. the journal's attitude towards its standards of scholarship
2. its perceived role in its discipline and the nature of that discipline
3. its concern to create debate by inviting various viewpoints in the field, or only to publish a particular school of thought
4. its aggressiveness in the field with regard to other publications, e.g. its priorities regarding

its own status and leadership in the discipline

5. its policies regarding its handling of referees/reviewers - their appointment, the use made of their reviews, etc

6. its policies regarding the work of established researchers

7. its concern to foster new researchers

8. its thoroughness in the revisions processes including the amount of editorial assistance given.

Regarding points 1 through 4, it is apparent that most academic journals spring up because an individual or group see 'a gap in the market' with regards to publication coverage of an established discipline or field, or a need to represent a newly-emerging field/discipline. Standards and *modus operandi* vary according to the priorities or whims of the editors and committees/administrators who run journals. Journals can change their profiles and motivations radically and suddenly, or slowly over time, in accordance with the desires of the personnel who run them. However, many journals establish an individual style, ethos and character - an expectation in the readership - which is difficult to change.

Regarding points 5 through 8, there are matters in the operation of a journal that are significantly the domain of the editors. Editors have noteworthy power in determining how the day-to-day editorial operations of a journal are handled. A look at the journal *Hermes* provides insights especially regarding points 7 and 8 above.

The journal of language and communication studies *Hermes* is based in the Aarhus School of Business (ASB) in Denmark. Journal editor Helle V Dam has provided an insightful analysis ([Dam 2005](#)) because, she says, the journal focuses on communications/language and also because she wished to raise issues regarding the journal's balance between fostering young researchers while gaining international status and credibility.

Dam explains that *Hermes*, founded in 1988, was created as a vehicle for the publication of local researchers and young scholars and, while it has grown into an international journal, it has nevertheless maintained its 'local roots' and continued its philosophy of nurturing scholarly development. Significantly, editors had initially been drawn from ASB and reviewers had been local until the journal took a strategic change of direction. In 2005, a policy change was taken to include 'external' referees as well as locals. The rationale for this is explained:

It is quite clear that in the scientific community, blind reviews performed by scholars with no involvement in the journal are considered a *sine qua non* for a high-quality journal. Still, highly qualified and dedicated internal referees may in principle do their job at least as well as external, independent referees would. Our policy change is therefore admittedly just as much a question of achieving more prestige as it is a question of ensuring higher quality. (Dam 2005)

Nevertheless, the journal remained committed to publishing the work of up-coming researchers, fostering the development of less experienced scholars. The editorial board of *Hermes* lists three main ambitions:

1. to run an international journal that publishes high-quality research papers;
2. to offer publication space also to young scholars;
3. to offer fast publication. (Dam 2005)

With a policy of 'thorough-reviews-rather-than-immediate-rejection' and three rounds of revisions sometimes being required for inexperienced scholars, the second and third ambitions could be seen to counteract each other. Dam notes that this has been overcome by two strategies:

- the first is the use of local referees who are willing to work fast;
- the second is the printing of the journal locally at ASB. (Dam 2005)

The philosophy of prioritising the output of young scholars - irrespective of the extra work this may place on the journal and the discipline - is central to the role of some journals, particularly in emerging disciplines where the journal itself is a major contributor to the development and growth of the discipline (e.g. also *TEXT* in Australia).

**Writer: *The researcher/author seeking peer review***

While some academics are highly skilled at preparing work for peer review, any analysis of the peer review process should also include a focus on the flaws of the inexperienced or rushed researcher seeking review. Dam notes typical weaknesses with manuscripts:

- the purpose of the paper is not clear/lacks focus
- the literature does not reflect the state of the art
- excessive use of quotations
- problems with the relationship between theory and analysis
- undocumented claims and over-generalisations
- the conclusion is not a real conclusion
- style problems
- the abstract is not sufficiently informative. (Dam 2005: 7)

These issues can arise out of hasty submission, laziness, professional pressures to publish, immaturity of the researcher, prematurity of the research write-up, or a mixture of these.

Manuscripts are sometimes sent hastily to a journal, perhaps to meet a deadline, with the writer relying on the astute reviewers to plug the argument gaps, or the editors to fill in from the style guides. Anecdotal evidence supports this contention. However, the reverse is also argued. Gannon says that authors tend to raise the standard of their work knowing it will be scrutinised by another ([Gannon 2001](#)).

Both contentions are correct, and can be correct for the same researcher at different times in her/his career. The editors at *TEXT* have seen every quality of submission from the most perfectly polished and refined academic pieces (which evoked only gasps of praise from the referees) to the high-school level mishmash (so poor, in fact, the work was rejected before being sent for review). Oddly enough, submissions also arrive which are clearly not suitable for the journal - not even dealing with the journal's disciplinary focus - and therefore provide evidence that some writers don't read the journal they submit to.

Having a strong knowledge of the range of articles published by the targeted journal is of prime importance. This not only provides an understanding of the preferred style of the publication, it also leads to avoiding that embarrassing reviewer report which says: 'Previous articles in this journal have already covered this topic'.



### ***Reviewer: The role of the reviewer***

The reviewer is engaged to uphold the standards of the journal and further the causes of the discipline in the context of fostering new knowledge and new debate. But, being individuals (and, of course, being academics) no two reviewers have the same methods or the same viewpoint. This is usually a benefit for the reviewing process, and not a drawback.

The reviewer's role is to some extent circumscribed by the philosophy of the journal (as outlined above). Individual reviewers can be selected because the journal editors know these reviewers are likely to agree with each other or with the submission, or on the other hand, because opposing views are sought. Reviewers known for writing tough or aggressive reports might be engaged for particular submissions, while referees with a lighter touch employed on others (e.g. from new researchers). Often in the case of research entering a new area, the reviewers are not fully expert in the matter under scrutiny, and here the reviewer must be equipped to be perceptive and flexible. Some reviewers are ideal for the job of nurturing new ideas, and for providing useful responses in the circumstance; some aren't. Editors often canvass a spectrum of views by sending a submission to two or more very differently-oriented reviewers.

The key role of the reviewer/referee is to interpret and represent the interests of the journal's readership. However, reviewers differ in their responses for individual, political, philosophical, cultural, school of thought and other academic reasons. Klopffer and Heinrich note how, in young, multidisciplinary academic fields such as communications or creative writing - which don't have the decades of experience in publishing enjoyed by the sciences - reviewers may come to opposite conclusions because of the lack of an accumulated archive of research in the field ([Klopffer and Heinrich 1999](#)). In older fields, of course, the very massiveness of that archive can create difference in reviewers' interpretations and opinions.

Why do referees referee? Journal editors may sometimes think it an imposition on busy academics' time. But there is an element of being 'ahead of the game' when a referee sees new research at its earliest manifestation. And there is an element of power involved because the privileged reviewer is given an opportunity to have an influence on the new work. Referees are frequently given the opportunity to be at the cutting edge of the discipline.

The combination of privilege and responsibility involved in the peer reviewer's work is not often enough articulated. Reviewers hold in their hands keys to success for all three levels - for the writer, the journal and the discipline. It is important work, not to be taken lightly, especially because a reviewer also lays her/his own reputation on the line in delivering a review.

### ***Discipline: The expectations of the discipline***

The development of knowledge requires quality control. Peer review is the system disciplines have established in pursuit of objective quality control. A discipline's advance is reliant on two factors: the quality of its original research and the quality of its critique of that research.

Disciplines are shifting, convoluted arrangements. Expectations within them involve the multitude and range of the expectations of the individuals involved. A good discipline for a researcher to work in is one where open, fair discourse prevails. A discipline should expect that its peer reviewers - along with its researchers and the journals themselves - will cultivate open, fair discourse.

Good journal editors are acutely aware of the positioning of their publication within its discipline; much time is spent orienting and steering a journal in accordance with the discipline's compass points and the winds of change. When editors make decisions they set a course for their journal and for the discipline. Good peer reviewers also need to be aware of the currents, the shoals, and the goals within the discipline.

### **Anonymity in the process**

There are two aspects which set the peer review process apart from more general reviewing such as that done in the popular media. These are:

1. the peer review is an examination of academic researchers by peers who are academic contemporaries (compared with media reviews of filmmakers by journalists, for example);
2. the peer review is anonymous (those in popular media are named, bylined or identifiable).

The second of these aspects is the more contentious. Anonymity is seen as a critical element of peer reviewing: Klopffer and Heinrich, editors of the *International Journal of Life Cycle Assessment* argue that 'the anonymous and strictly confidentially-performed review procedure ... is the backbone of this process, and we take care of it with our minds and souls' (Klopffer and Heinrich 1999: 61).

However, this is not a universal point of view, especially in light of changes within the contemporary academic environment which has moved toward openness and transparency: 'Many view the powerful role that reviewers play in scientific publishing with suspicion, and feel that the anonymity of the process is contrary to the current demands for transparency' (Gannon 2001).

Young and upcoming researchers have their own perspective on the process. Writing on behalf of the World Academy of Young Scientists (a forum created under the auspices of UNESCO in 2003), Mainguy, Motamedi and Mietchen (2005) identify problems with single-blind peer reviewing (SBPR) of young researchers. Basing their views on work done by Wenneras (1997), [Laband et al \(1994\)](#), Katz et al (2002) and others, they suggest:

Even though peer review is universally accepted as an essential element of research, considerable debate persists on how to implement it. The vast majority of our members, especially from developing countries, were concerned about the apparent unfairness of the current procedure, a perception that is prone to generate frustration, fear of discrimination, and distrust. We reached a consensus that slight modifications to the current review process would help in getting more objective reviews based on the quality of the research rather than the age, affiliation, gender, or pedigree of the authors.

Single-blind peer review (SBPR), in which the reviewer knows the identity of the author but not vice versa, is the currently accepted practice. Because SBPR can be vulnerable to sexism and nepotism, its ethical foundations have come under criticism; the method is frequently recognized to be biased against new ideas, women, young scientists, career changers, and scholars from less prestigious universities and/or from developing countries ... ([Mainguy et al 2005](#))

Mainguy et al propose two means to eliminate bias from the peer-review process: open peer review (OPR) and double-blind peer review (DBPR).

In open peer review, the identities of both authors and reviewers are revealed, affording the authors the ability to identify the reviewers' comments to a person. Even though this might be an equitable strategy to prevent unfair rejections, this process has no safeguard against unfair acceptance of papers - reviewers, and especially newcomers, may feel pressured into accepting a mediocre paper from a more established lab in fear of future reprisals. (Mainguy et al 2005)

As a concept, OPR is as bold as it is fascinating. Although an obvious device, it is not an accepted part of the research publishing ethos for journals or for monographs (where anonymous - and sometimes paid - readers are employed to assess). Academics' general acceptance of the anonymity of the reviewer is surprising in a culture where striving to reveal truths is the principal motivation. Some research journal editors would surmise that revealing the identities of reviewers could lead to bloodshed. Still, there is an unusual contradiction in the veiling of the process which monitors the drive towards unveiling new knowledge.

On the other hand, the Young Scientists also canvass the possibilities of DBPR, a method now prevalent in several disciplines including computer science, philosophy, economics, communications and media studies:

DBPR, in which both the reviewers and the authors remain anonymous to each other, is thought to disentangle the peer-review process from non-scientific factors, thereby presenting an appealing alternative. The a priori case for masking and blinding is strong, and several studies have suggested that articles published in DBPR journals were cited significantly more often than articles published in non-DBPR journals. However, other studies have been less convincing; critics of DBPR argue that it is difficult to hide the identity of the institution, laboratory, and/or authors of a paper from the reviewers, especially in smaller specializations. For instance, in a DBPR policy trial, despite explicit instructions to authors, 34% of prospectively evaluated manuscripts contained hints to unblind the authors, and editors correctly identified the authors or institutions of 25% of the manuscripts. The disconnection between principle and practice is evident, and so far, few journals, and even fewer in biomedical sciences, have implemented DBPR policies. The reasons appear to be partly historical, as journals are used to SBPR, and partly intellectual, as the benefits of DBPR still remain controversial. (Mainguy et al 2005)

In its earlier years, *TEXT* used SBPR but has moved more recently towards greater use of DBPR. No significant difference in the two techniques has been noticed by the editors, except that with DBPR established scholars are probably given a harder time in terms of their use of punctuation! It goes without saying that the old game of the writer guessing at the identity of the referee is now also played by the referee guessing at the identity of the writer.

### **The next phases of peer review**

It seems that the major critics of the peer review process are those who defined it in the first place: the scientists. [Linkov et al \(2007\)](#), scrutinising peer reviewing in medical education for online publication, note that until we have properly defined the objectives of peer review, it

will remain almost impossible to assess or improve its effectiveness: 'The research needed to understand the broader effects of peer review poses many methodological problems and would require the cooperation of many parts of the scientific community' (Linkov et al 2007: 250). And Benos et al concede that: 'Very little definitive research into the practice and effectiveness of peer review has been done' (Benos et al 2003).

Yet, while it is acknowledged to be a flawed process, it appears to remain the best way we have to ensure quality through checks and balances. As Gannon notes: 'The benefits of peer review are real, whereas the alternative - giving up peer review in favour of a scientific "freedom of expression" - would create many problems of its own' (Gannon 2001). In the sciences, engineering and economics this could result, at the extreme, in errors in patient treatment, collapsed buildings and unjustified stock market impacts. In the humanities and creative disciplines, it might undo the democratisation of ideas that persists there.

So how can these issues be dealt with and problems overcome? Benos et al (2003) make the point that 'reviewing is both a privilege and responsibility'. Gannon supports this, arguing that we must devote time to the process of reviewing others' work and, while he raises the question of payment as an incentive, dismisses this for non-profit journals (Gannon 2001).

With this in mind, and for advancement within the non-scientific disciplines, we might review the centuries-old scientific ethos indicators raised earlier - universalism, systematic scepticism, ethical neutrality, communalism and disinterestedness - and reconsider their principles:

- *Universalism*: when reviewed in a postmodern light, universalism as a value rebuts the concept of 'technical norms' and universal truths, accepting new research papers as potential groundbreaking work, free of cultural and political constrictions and regimens.
- *Systematic scepticism*: this indicator should accept that scepticism is a healthy part of any review process but would seek to reposition itself in acknowledging that unconventional methods may indeed hold merit.
- *Communalism*: this must be reinvigorated to foster and encourage new ideas and new alliances within a discipline, whether or not they are within the traditional paradigms of teaching or research.
- *Ethical neutrality*: would see reviewers accepting work on which they may hold an opposing ethical position, but which they can nevertheless accept on its scholarly merits.
- *Disinterestedness*: would accept the economic realities of research and seek that reviewers diligently commit to their task, using the same care in reviewing that they would apply to their own best research work.

And to these might be added, reflecting the requirements of monitoring in new and emerging disciplines:

- *Flexibility*: understanding that a range of ideas and methodologies will come your way as a peer reviewer. You may not agree with them all, or they may appear wanting, due to your own limitations.
- *Transparency*: the concept of anonymity enshrined in single- and double-blind reviewing may be vulnerable in the growing culture of freedom of information

(FoI). A preparedness to justify a judgment without the cloak of anonymity may be forced on reviewers in the future.

- *Clarity*: journals need to better address the provision of clear and concise criteria for referees to work from and researchers to work towards.
- *Encouragement*: As Benos et al say: 'when peer reviewing, follow the golden rule: treat other manuscripts as you would want your own to be treated' (Benos et al 2003).

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*Dr Jane Johnston teaches journalism and public relations at Griffith University, Gold Coast. Her most recent book is Media Relations: Issues and Strategies (Allen & Unwin 2007) and she is currently co-editing (with Clara Zawawi) a third edition of Public Relations: Theory and Practice (forthcoming Allen & Unwin).*

*Associate Professor Nigel Krauth teaches writing at Griffith University, Gold Coast. With Jen Webb he is co-editor of TEXT [www.textjournal.com.au](http://www.textjournal.com.au) and with Tess Brady is co-editor of Creative Writing: Theory beyond Practice (Post Pressed 2006).*