

## DISCUSSION of “Editorial—The peer-review system: prospects and challenges”\*

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As editors of an academic journal (*Urban Water Journal*) we have been actively engaged in the academic peer-review process, which the editorial by Kundzewicz & Koutsoyiannis (2005) concisely summarises and debates. Indeed, we have found the standard peer-review process (by anonymous reviewers), as a quality assurance mechanism, to be the most common “weak link” in the research publication process, particularly in terms of duration and (sometimes) even in terms of quality assurance *per se*. Often this is due to reviewer *fatigue*, as “preferred” reviewers (respected, knowledgeable and thorough) are also (by default) the most busy ones and are generally already overloaded with their own (successful) work. They tend to get more and more papers to review until they reach a saturation point, beyond which their reviews are either delayed, or less thorough. The (obvious) option of switching from them to “less preferred” reviewers (who would probably have more time in their hands) is not without its compromises...

The debate on quality assurance mechanisms will certainly only gain in importance as the impact of the Internet on scientific publishing is acknowledged and its consequent impact on original research assessed, a fact which is supported by the existence of a forum on “future e-access to the primary literature” in no less a journal than *Nature* (Odlyzko, 2001). We are clearly experiencing a faster pace of communication, including e-prints, but also other informal means, primarily e-mail, which in turn is creating or pushing towards accelerated publication (Odlyzko, 2001). This accelerated publication requirement implies a need for acceleration, or at least for a serious rethinking of the peer-review process which currently supports it. Engineering has, arguably, not been in the forefront of this debate, which was mainly spearheaded by Medicine and the Social Sciences (a fact that can be also observed by looking at the references provided in Kundzewicz & Koutsoyiannis’s editorial).

The current system, which dates from the 1700s, when the Royal Society of London set up a “Committee on Papers” with the power to solicit expert opinions, has recently been under question, even becoming a conference theme by itself: in 1986, Drummond Rennie, then deputy editor at the *Journal of the American Medical Association*, announced the first conference on peer review (the International Congress on Peer Review and Biomedical Publication: <http://www.ama-assn.org/public/peer/peerhome.htm>). After 20 years and with five conferences now complete, it is debatable whether the really fundamental questions on the actual effectiveness (or lack thereof) of the peer-review system have been satisfactorily answered. That said, there have been interesting findings over the years, some supporting open review processes, others suggesting the need for professional

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reviewers and editors. There have even been suggestions of publishing un-reviewed pre-prints and allowing the community to assess which papers could merit a more thorough review based on interest. The parallels with the open source movement and its “public” quality assurance processes are evident (see also Harvey & Han, 2002, and their discussion on the development of the Linux project—<http://www.linux.org/>).

Publishing more and allowing fast access to material (including datasets, tools, prototypes, etc.) is certainly one way to increase the information/effort ratio demanded by current ICT-driven, productivity-oriented *modus operandi* of the research community. Another way, which is employed by the *Behavioral and Brain Sciences Journal* (which carries an impact factor of 10), consists of publishing invited reviews in the form of discussion papers by single authors/reviewers or teams of authors/reviewers, together with the original paper and a short response paper by the authors. Although this process is not designed (primarily) to assure quality—yet does deliver on this too, almost by default—it allows a number of benefits: from the point of view of the reader it exposes a wide range of discussions (and possibly contradictory views) on a given subject “at one go”, which drastically increases the information load received when reading the paper. From the point of view of the reviewer, it is certainly a “publication”, and an exposure of his/her views within a well structured scientific discussion. From the point of view of the author, the process has the advantages of open review, which have been clearly identified in Kundzewicz & Koutsoyiannis (2005). Arguably, this is a difficult model to implement and, probably, unsuitable for some very technical, highly specialised publications which do not lend themselves particularly well to debates; yet, for the (significant) number of papers for which this process could be applied, it could result in an increased visibility and could ensure a minimum of “proper” dissemination. This would certainly have positive side effects for journals in the form of increased impact factors, although a discussion on impact factors and their significance goes beyond our current theme. Another approach could be the one adopted by *The Lancet* where preprints of papers can be posted at a server while the paper is undergoing review, or the *British Medical Journal*'s intention (Godlee, 2002) to go towards real-time online open reviews followed by an open commentary session prior to publication, enabling, at least in principle, the “best of both worlds”.

It could be further argued that open commentaries and moderated discussion sessions on published work, accessible and therefore peer reviewed by the academic community at large, rather than a few individuals (similar to web-based knowledge dissemination platforms such as Wikipedia—[http://en.wikipedia.org/wiki/Main\\_Page](http://en.wikipedia.org/wiki/Main_Page)—based on wiki—<http://wiki.org/wiki.cgi?WhatIsWiki>) could eventually change the role of Editors towards that of Moderators. This is, however, a long way away and for it to work, significant changes are required, not only to the scientific publishing domain, but, perhaps more importantly, to the criteria and indicators of academic performance, which ultimately dictate the form of academic publication authors select for their work.

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Kundzewicz & Koutsoyiannis (2005) provide an excellent review of some of the key issues around the peer-review system. I am sure that these issues are just as relevant in other disciplines as they are in hydrology.

My own field of resource economics is a little unusual in that double-blind reviewing is the norm. I noted the comment that double-blind reviewing is “costly and difficult and technically cumbersome”. In my experience with this system, including as an editor of the *Australian Journal of Agricultural and Resource Economics*, I don’t perceive that it has any significant extra cost or difficulty. This is particularly so now that we handle submissions through a web site, which automatically converts papers into pdf format before they are sent to reviewers. We require authors to submit papers with their names and acknowledgements omitted, and they are well trained in this. The only slight complexity is that our assistant has to check that the names are in fact excluded, but all submissions must be checked for other purposes as well, so the extra work is really minimal.

I can see the arguments about the advantages of zero-blind reviewing, but also am conscious of the difficulty of securing good reviewers. Some years ago, I did start signing my reviews, but after some of my experiences with the peer-review system documented in Pannell (2002), I decided that some participants in the system do not act in good faith, so I ceased the practice. Perhaps I should reconsider.

I was particularly taken by the set of rules from Armstrong (1982) that you listed on page 579. I don’t think these rules are hard-and-fast, but there is certainly a tendency for them to be true. Based on my own experiences described in Pannell (2002), and other horror stories documented there, I do agree with Armstrong that you are likely to have less difficulty getting published if you do NOT (i) pick an important problem; (ii) challenge existing beliefs; (iii) obtain surprising results; (iv) use simple methods; (v) provide full disclosure; or (vi) write clearly. A great example of (vi) is Akerlof’s (1971) paper on the economics of information, “The market for ‘lemons’”, which was intentionally written in an interesting style that avoided the usual dryness of academic articles (Gans & Shepherd, 1994). One suspects that this was at least part of the reason it was rejected by several of the leading journals in economics:

- the *American Economics Review* (“I got a reply from the editor which said that the article was interesting but the AER did not publish such trivial stuff”);
- the *Journal of Political Economy* (“the JPE’s referee’s report asserted the opposite: that the paper was too general to be true”); and
- the *Review of Economic Studies* (again “it was rejected on the grounds that it was trivial”).

Finally the *Quarterly Journal of Economics* accepted the paper. Being readable was not Akerlof’s only sin; he violated several of Armstrong’s other rules as well.

The punch line is that Akerlof was eventually awarded the Nobel Prize for economics for the work that this paper kicked off. I think the issues behind Armstrong’s rules are very important and I believe every journal editor should attempt to be conscious of them, especially when considering reviews of innovative papers.

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I wish to extend the interesting discussion of the review system presented by Kundzewicz & Koutsoyiannis (2005) by raising a related point.

One of the features of the anonymous review system is that referees are not involved in direct interaction with the authors unless editors agree to serve as intermediaries. This issue can be looked at from at least two viewpoints: the one of the authors and the one of the referees.

If reviewers' names are disclosed to the authors, the latter may start a debate about their contribution, trying to bargain. Authors may be unhappy about the result of the review process, especially if the recommendation of rejection of their paper is issued. They may expect more helpful comments about the scientific contents of the paper, its structure, format, style, etc. than typically given by reviewers. They may wish to contact the reviewers in order to seek clarification and to request referees to be more specific in their comments and to offer more detailed advice.

If authors approach a referee, he or she may react positively, in a constructive spirit. However, this may open the door to a potentially iterative, extensive and long-lasting exchange of communications. If a reviewer enters into such a correspondence, he/she may be requested to state his/her opinion several times, screening a suite of drafts of the paper, and trying to convince the importunate authors that the paper has to be changed in all necessary points. In this way the reviewer becomes an advisor or in extreme cases even a co-author. This is not the intention of the review process. Such a supervisory function may be unbearable and the referees may regret their decision to have agreed to the review request and may reject any future requests from a given journal. The time and effort needed to accomplish such work in a decent way may be much higher than expected and this makes it difficult to accommodate such activity in the overloaded agenda.

This is not a theoretical case. It illustrates experience of the present writer resulting from long-lasting discussions with authors of papers submitted to IAHS symposia.

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The authors (Kundzewicz & Koutsoyiannis, 2005) are commended on opening up a forum to discuss the peer-review system. This is of course an important issue for any scientific periodical that aspires to become a respected journal. A journal that has a good, professional review system commands the confidence of authors who are then more willing to submit their papers to that journal. This discussion offers suggestions which can improve the existing half-blind peer-review system.

### **Editorial errors of the first kind**

In order to minimise the editorial “errors of the first kind” (i.e. publishing papers that do not deserve publication), the editors can monitor the subsequent discussions of the papers. The discussions may highlight certain flaws in the papers which ought to have been detected by the reviewers. The editors can then keep the names of these reviewers and give due consideration when they are asked to review further papers. Another practice which can also reduce the errors is to publish the names of the reviewers in the published papers. This is similar to the practice of including the names of the examiners in approved PhD theses. If the reviewers know that their names will be published with the papers, they may be more careful in recommending publication of such papers. On the other hand, viewing this practice in a positive way, it in fact rewards reviewers to have their names associated with good papers.

### **Editorial errors of the second kind**

In order to minimise the editorial “errors of the second kind” (i.e. rejecting papers that deserve publication), and in a way this is a more serious error because it means that good papers have escaped the journal, the editors can invite the authors of rejected papers to inform them if their papers are subsequently accepted by another journal. Authors are likely to accept the invitation because it proves that the reviewers have mis-judged the quality of their papers. Editors can also keep the names of these reviewers and give due consideration when they are asked to review further papers.

### **Disadvantages of half-blind review system**

As stated in Table 1 of Kundzewicz & Koutsoyiannis (2005), the main disadvantages of the half-blind review system are: “Allows subjectivity, bias, abuse; affords the

referees the possibility to be rude, vindictive and lazy.” All these disadvantages can be overcome if the editors are vigilant in reviewing the reviews. By carrying out such reviews, they can ensure that bias, abusive, rude or vindictive reviews are not passed on to the authors. If editors receive such reviews, they can ask the reviewers to revise their reviews. If the reviewers refuse to respond, they can dismiss the reviews, and ask another reviewer to review the paper. While the editors have a responsibility to ensure that only reasonable papers are passed on to the reviewers, they also have a responsibility to ensure that only reasonable reviews are passed back to the authors.

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