Pathologies, improvements and optimism

Reply to discussions of "Editorial—The peer review system: prospects and challenges"

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We are very pleased to have received many comments on our editorial article (Kundzewicz & Koutsoyiannis, 2005) and express our gratitude to all who have reacted to this. Several feedbacks were communicated to us quite informally (orally, or in written form), while it was decided to publish four contributions that were submitted formally: Makropoulos *et al*. (2006), Pannell (2006), Schumann (2006) and Wong (2006). This exchange of thoughts corroborates the opinion that the issue of review systems is of primary concern to research scientists. The discussion also demonstrated the power of the Internet. Among the readers of our editorial article in *Hydrological Sciences Journal* (*HSJ*), freely available on the net, were representatives of other disciplines. We were delighted that, for some of them, it was the first contact with *HSJ*.

PATHOLOGIES

All the contributions describe pathologies of the current peer-review system, or imply them in their attempt to propose remedies.

The authors agree with Makropoulos *et al.* (2006) that, at times, the standard peer-review process can be a weak link in the research publication process in terms of duration and quality. This latter statement is counter-intuitive—the reviews are there to warrant the quality of material going to print, rather than creating quality problems themselves. Indeed, "preferred" referees are overbooked as a rule; hence, "less preferred" referees are often approached.

Pannell (2006) embraces "Armstrong's hexalogue", quoted in our article from Armstrong (1982), and provides a sound example of total failure of the peer-review system (a paper rejected from three journals as trivial; then fetching its author the Nobel Prize). Pannell attributes this failure mainly to violation of "Armstrong's hexalogue". In his very interesting earlier paper, which he brought to our attention, Pannell (2002) discusses his own experiences (such as reviewers' abuse, including "stealing" ideas or results from material under review, an extreme case that we had mentioned in our article). Pannell (2002) concludes that the authors "should not be too discouraged about the negative responses of referees, because even the most celebrated researchers have suffered similar slings and arrows". He recommends persistence and provides a simula-

tion showing that, if authors persist in submitting their paper to a new journal after rejection, they will have their paper published after, say 10 trials. As encouraging as this may seem from an author's point of view, it may yet manifest a pathology of the system. Specifically, persistence may be a good remedy for what we called errors of the second kind (i.e. rejecting papers that deserve publication), but it is also likely to enhance errors of the first kind (i.e. publishing papers that do not deserve publication).

Indeed, there are so many competing journals in the water field (as in other fields), that there is a strong chance that a poor paper rejected by one, or even more journal(s) may get accepted somewhere. The pathological characteristics of this process decrease or may even become healthy characteristics when the authors view it as a dynamic learning process. Authors collect useful reviewers' remarks in the process and react correspondingly, in a constructive vein, upgrading their papers. We are aware that this is already quite a common process. There are cases where papers rejected in another journal are submitted to *HSJ* and in some of these cases, authors inform the Editor of the history of the material. Some of these papers are rated highly by *HSJ* referees and accepted for publication and then prove to be quite a success (e.g. as measured by the number of independent citations). This is indeed proof that the papers were not given satisfactory attention in the first place. This issue is also discussed by Wong (2006) who, to minimize errors of the second kind, proposes the idea of inviting authors of rejected submissions to inform the Editor about the fate of their papers. This idea raises mixed feelings, unless there is a clear case of a paper being rejected in one journal and submitted immediately to another journal, and being rated "good" and so published. To minimize the errors of first kind, Wong also proposes monitoring of subsequent discussions of the published papers. However, discussions in *HSJ* (and in other journals in the field) are in fact not very frequent (a few per year) and discussions highlighting flaws in the papers, which should have been detected by referees, are even less frequent. Undoubtedly, discussions should be encouraged; not only in the sense of rectifying errors (failures of the system), but also in the sense of indicating alternative viewpoints and insights, contributing to clarification and better understanding—to the benefit of the readership.

The interest and comment of Pannell, who is a visitor to *HSJ* coming from the field of resource economics, indicates how common the concerns about the peer-review system are in all fields. Makropoulos *et al*. (2006) correctly point out that the discussions about it are spearheaded by the Social and Medical Sciences. There are obvious reasons for this: the Social Sciences because the review system manifests a social behaviour and should be studied as such; the Medical Sciences because they interest all people. Thus, it is not surprising that pathologies of the review system were recently brought to the attention of the general public by the *New York Times* by Dobbs (2006), who provides two stories from the general Medical Sciences.

The first refers to two "breakthrough" papers in stem cell research published in *Science* (in 2004 and 2005), initially celebrated and then retracted (2006), after a major fraud was found (classified in our article as "fabrication of results") resulting from serious research misconduct. A chronology of events leading to the highly publicized retraction of the papers is presented at AAAS (2006). This case clearly demonstrates that the peer-review system cannot prevent editorial errors of the first kind, even in the most prominent journals. Donald Kennedy, Editor-in-Chief of *Science*, states "Peer review cannot detect [fraud] if it is artfully done" and Martin Blume, Editor-in-Chief of the American Physical Society and its nine journals, clarifies "Peer review doesn't

necessarily say that a paper is right. It says it's worth publishing" (Couzin, 2006). Another interesting point of this story is that the fraud was not uncovered by means of formal journal procedures (i.e. discussion papers), but through online exchanges (Hauben, 2005; Dobbs, 2006; Chong & Normile, 2006). The positive message of the story is that fraud was (and will be) eventually uncovered and, thanks to the Internet, today this can be done faster than ever; thus, the story may discourage research misconduct in the future.

The second story is an interesting study of the review system by Ioannidis (2005), an epidemiologist, who states (among others): (a) "There is increasing concern that in modern research, false findings may be the majority or even the vast majority of published research claims"; (b) "The greater the financial and other interests and prejudices in a scientific field, the less likely the research findings are to be true"; and (c) "The hotter a scientific field (with more scientific teams involved), the less likely the research findings are to be true". Perhaps quotation (a) targets the Medical Sciences, but scientists from other fields including hydrological sciences should not totally ignore it. Quotations (b) and (c) manifest the difficulties that the scientific community has to overcome in topical areas, crowded by researchers, in which, in addition, several conflicting interests are present.

IMPROVEMENTS

The four contributions include proposals for improvements, or discuss our own proposals. In turn, we discuss them, particularly from the perspective of *HSJ*.

"Kitchen" *vs* "dining room"

The idea of publishing a paper together with reviews (converted to early discussions), brought up by Makropoulos *et al*. (2006), is interesting. However, in our opinion (expressed also in our article) the typical readership is not interested in the "kitchen" of the process (documentation of the development that the paper has passed from the moment of submission to publication, including reviews). For most readers, reading the final revised paper (which passed the quality control and is published in the journal—metaphorically served in the "dining room") is sufficient.

Nevertheless, the idea that the "kitchen" is accessible by the (few) interested readers may be not bad. The novel style of *Hydrology and Earth Systems Sciences* (*HESS*; Roth *et al*., 2005) clearly makes this distinction of "kitchen" (the accompanying electronic journal *HESSD*) and "dining room" (*HESS*). Here, one is not only allowed to visit the "kitchen", but can directly enter a comment in *HESSD* on any paper during its review process. However, the very small number of such unsolicited comments (as opposed to invited reviews), which can be seen by visiting the web pages of *HESSD*, harmonizes with our claim of low interest in the "kitchen". In addition, some authors may find it scary that their initial drafts (which may contain errors) along with their (perhaps ruthless and/or anonymous) reviews will be perpetually accessible by anyone; if their paper is rejected, this may also create difficulties in resubmitting it to another journal (cf. the above discussion on persistence).

In some topical areas, where there are at least two camps of scientists whose opinions on a given subject are contradictory, publishing a set of items in one go could make sense. This is not excluded in *HSJ*, but it is not common. The tradition in *HSJ* is that referees are informed about the progress of the paper they reviewed and about the Editor's final decision. So, if a paper is published, a reviewer from the opposing camp knows it and, in principle, may prepare a discussion.

Anonymity (half or full) *vs* eponymity

All the contributions above are concerned with the issue of half-blind (half-anonymous), double-blind (completely anonymous) and open (eponymous) reviewing. Informal comments received by the authors also discussed this issue. Among these was a declaration of an eminent scientist that he is prepared to review in the anonymous mode only.

Interestingly, while half-anonymous reviewing is the dominant system in the field of hydrology, this is not the case in other fields. Pannell (2006) explains that, in his field (Economics), the norm is the completely anonymous system and his experience is that it has no extra cost or difficulty; the only complexity is that the assistant has to check that the names are in fact excluded. However, in our opinion (which we also expressed in our article saying that this system is difficult to implement, or even infeasible) omitting the names, affiliations and acknowledgements, which is indeed easy, does not necessarily hide the authors' identity. If the author wishes that his or her name be known to the reviewers (because perhaps he or she is a famous and respected scientist), it would be very easy to disclose it by inserting a list of own references, or discussing details of own works. In addition, the opposite case (the author wishes to hide his or her name) is difficult, as there are other tracks which make it possible to decipher someone's identity. Such elements, in addition to references of own studies, are: the choice of case studies, geographical referencing, the continuation (follow-up) of an earlier work and even the line of thinking.

Schumann (2006) raised an interesting point, which was not touched upon in Kundzewicz & Koutsoyiannis (2005): the potential bargaining between authors and reviewers in the case of eponymous reviewing, and other potential problems (for reviewers) such as iterative and long-lasting exchange of communications. Indeed, the case of importunate authors who try to bargain with referees is a pathology, but it is not a frequent practice. The second author (DK), who for the last eight years has done only eponymous reviews (about 130), has not reported any cases of this potential pathology. Attempts to bargain with the Editor do occur more commonly, though.

Some reviewers of manuscripts submitted to *HSJ* accept the mentor's role guiding the authors through the stages of development of their papers from a clearly non-acceptable format to acceptable drafts. There are cases in which authors' acknowledgements to the reviewers are really well deserved. If, based on interaction between the authors and the referees, a joint paper develops in the future, this may be regarded in a positive vein.

On the other side, Wong (2006) suggests publishing the names of the reviewers in the published paper. This is rather radical and quite different from the *HSJ* practice of publishing (once a year, as a token of recognition) the names of experts who reviewed

papers over the past year. Wong claims that if the reviewers know that their names will be published with the papers, they may be more careful in recommending publication of bad papers—an interesting point which we did not discuss in our article. Essentially, Wong's proposal is equivalent to adopting eponymous reviewing as the only review system. Makropoulos *et al.* (2006) also mention that some current discussions support open review processes. The article by Dobbs (2006) mentioned above clearly endorses open reviewing, arguing that the few journals which are already using it produce good papers and that it discourages fraud.

In our article we discussed thoroughly the advantages of open, eponymous reviewing, but simultaneously expressed our opinion that its radical and general adoption in *HSJ* is unrealistic due to the reluctance of reviewers to adapt themselves into this system. It seems that the situation is similar in other water-related journals, which have not proceeded to such radical changes (see e.g. Roth *et al.*, 2005 for *HESS*; Parlange *et al.*, 2005 for *Water Resources Research* (*WRR*); and Makropoulos *et al.*, 2006, for *Urban Water Journal*). On the other hand, it appears that there is a general recognition of the virtues of open, eponymous reviewing and a gradual movement towards it by encouraging (but not demanding) eponymity. This is the case for *HSJ* (Kundzewicz & Koutsoyiannis, 2005), for *WRR*, which gently pushes toward more open review with satisfactory results so far (Amilcare Porporato, personal communication) and certainly with *HESS*, which provides accountability of reviews (obviously, the eponymous ones) by posting them onto *HESSD*.

The role of the Internet

Makropoulos *et al.* (2006) discuss the role of the Internet as a tool facilitating scientific publishing (e.g. accelerating publication), as well as a medium enabling novel systems (e.g. in Wikipedia). There is no doubt that the role of the Internet in scientific publishing is major and multiple (related to all the issues discussed above). Thus, any vision of the future of scientific journals should include analysis of options and possibilities that are made available by the Internet.

The role of editors and reviewers

Makropoulos *et al.* (2006) mention the need for professional reviewers and editors, and point out that other developments, particularly related to the Internet, could eventually change the role of editors towards that of moderators. Perhaps some journals already operate with such a role for editors (an option not necessarily related to the Internet *per se*), but in *HSJ* the Editor has a more active role. It is generally recognized that the authors appreciate such an active role, especially when different reviews are conflicting.

On the other hand, Wong (2006) envisages an even more active role for editors, suggesting that they should be vigilant in reviewing the reviews, ensuring that only reasonable reviews are passed back to the authors, and keeping track of the names of reviewers who perform poorly. Indeed, the *HSJ* Editor has a list of referees to avoid due to a history of negative experience (e.g. no review, despite promises, or unfair

review). However, editors have to remember that referees are indeed precious, doing community work of value to a journal, and they have to be cherished.

Indeed, it is an Editor's duty to take care that no abusive, rude, or vindictive reviews are passed to the authors. However, it may not be easy in some cases, especially when reviews are very long. Editors, who typically cannot devote their full time to the journal, are very busy with their work on accepted papers; thus, sensitive comments, which could be hidden in lengthy reviews of rejected papers, may escape their attention. Hence, implementation of the basically good ideas proposed by Wong (2006), such as the filtering of reviews, would be difficult, being time- and labour-consuming. Similarly, seeking new reviewers who are more professional may not be feasible. However, getting more professional reviews from the present pool of reviewers may be more feasible, but presupposes accountability of this service; this will be achieved when open eponymous reviewing becomes the rule rather than the exception. Other institutional changes, promoting quality of published papers *vs* quantity, could help significantly if applied worldwide. However, the latter issue (touched upon by Makropoulos *et al*., 2006, and posed in several informal discussions with the authors) cannot be affected by the policy of scientific journals.

OPTIMISM

Are there elements in the current status of scientific publishing that allow one to be optimistic? Our view on this question is positive. Despite its pathologies, the review system is a great achievement of the scientific community, so democratic as to virtually embrace all scientists, who act on a voluntary basis providing community work without direct compensation. In fact, the pathologies reflect academic ethics and in a more general sense certain social behaviours of the scientific community. In this respect, there is a two-way interaction between improvements in scientific publishing and improvements in academic ethics. We regard the discussion of problems, as done with our own article and the present contributions, or even with publishing personal experiences, such as in Pannell (2002), as a necessary condition for any improvement. The discussion may be enhanced into a real dialogue if we are ready to change established behaviours and abandon past stereotypes.

We may have disappointed some readers of our article (as testified from some informal comments we received) for not proposing more radical change. We believe, however, that *HSJ*, the oldest journal in the hydrology and water resources arena, should proceed very carefully, without challenging the majority of its authors, reviewers and readers. Yet we believe that gradual change (towards a more open review system) is possible and will receive positive feed-back by improved ethics and behaviours. We also believe that small improvements, even by single individuals, may eventually have a more global effect. For example, if we (the authors of this article) can do our jobs (ZWK as Editor and DK as an Associate Editor) a little bit better, we will offer a small additional service to *HSJ* and the scientific community; and we offered this article in this direction.

Are we optimists about the prospect of *HSJ*? Objective data, such as the rise in the value of the impact factor and the rise in the number of submissions, and the fact that the journal is accessible online, allow a positive answer. The number of submissions

over the last 12 months has exceeded 200 which means that only one in three papers is likely to be accepted. However, many of the new submissions are rated by referees in the category "poor to fair", while the number rated "very good to excellent" is not high. Nevertheless, as communicated to us by an eminent hydrologist, his best known, and widely quoted, papers started from ruthless, critical reviews of the original manuscripts. Several papers that are rated as poor, and are rejected today, may have considerable potential for improvements. It is excellent if constructive reviews indicate how an author can move forward, even if rejection is recommended.

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