Commentary

The journal impact factor contest leads to erosion of quality of peer review

David A. Wardle

David A. Wardle (david.wardle@slu.se), Department of Forest Ecology and Management, Swedish University of Agricultural Sciences, SE901-83, Umeå, Sweden

In his commentary, Hochberg (2014) makes the case that the quality of scientific research is maintained and enhanced over time through a process akin to Darwinian evolution, and that high quality peer review is a necessary ingredient for this to occur. It is a good analogy. This is not to mean that peer review is infallible, and there are many cases in which it has helped impede publication of truly innovative work, including several studies that have subsequently delivered Nobel prizes (Campanario 2009). As such, it has been claimed that peer review ‘favors unadventurous nibblings at the margin of truth rather than quantum leaps’ (Lock 1985). Ecology is not immune from this problem; the ‘why is the world green’ paper by Hairston, Smith and Slobodkin (1960), arguably the most influential publication in trophic ecology in the past 60 years, was first rejected by *Ecology* (Schoener 1989). Nevertheless, peer review overall does more good than bad, and so long as that is the case, its contribution to the evolutionary process that Hochberg (2014) describes should be positive overall. Erosion of the quality of peer review, when combined with the shortcomings that the peer review process already has, will inevitably retard this evolutionary process.

Hochberg (2014) also makes the case that overexploitation of reviewers (i.e., ‘tragedy of the reviewer commons’) is likely to reduce the effectiveness of reviewers which will then push overall scientific quality downwards. He then identifies three mechanisms that should counter this effect. However, I suggest that Hochberg overlooks an important issue contributing to reviewer exploitation, and that until this is resolved by the scientific community to some satisfaction, declining effectiveness of the peer-review process in maintaining scientific quality is inevitable.

The issue in question relates to journal ‘impact factors’ (hereafter IFs) and the obsession that many journals and scientists have with them. This appears to have contributed to many ecological journals implementing ever-decreasing acceptance rates (now 10–20% for most of the main ecological journals), on the belief that being more selective and publishing only work that is likely to be generously cited will elevate their impact factor relative to that of competing journals. It has also contributed to scientists flooding high-IF journals with submissions. Inevitably many of these manuscripts will be submitted to three or four (or more) journals over time before publication, and may consume the time of many reviewers and editors in the process (not to mention greatly delaying communication of the science to those who might find it useful). This may not always be due to the authors aiming too high—given that the fate of any manuscript following submission to any highly selective journal is partly determined by stochastic factors (i.e., based on whose desk it happens to land on), an author of even an excellent paper might need to submit to two or more leading journals before they happen to encounter the ‘right’ combination of reviewers and editors. This problem is a key contributor to the overexploitation of the reviewer pool or the ‘tragedy of the reviewer commons’ and, potentially, evolutionary decline in manuscript quality. I suggest two shifts that will be needed in the scientific community for this problem to be reversed:

The first is for the scientific (and science publishing) community to abandon journal IFs. Impact factors are widely recognized as seriously flawed indicators of scientific merit for several reasons (Seglen 1997), including that high journal IFs are driven by a tiny proportion of manuscripts that are extremely heavily

cited and with most of the remainder attracting only modest citations—note that nearly a quarter of the ecological papers in *PLoS ONE* (IF = 4.6, acceptance rate = 69%) are cited more often than the median paper in *Ecology Letters* (IF = 15.2, acceptance rate = 11%) (Wardle 2012). If the scientific community were to assess the merits of a paper based only on its own impact (i.e., its quality) rather than the IF of the journal in which it appeared, then over time this should reduce the volume of manuscripts directed towards those journals with high IFs and high rejection rates, which would reduce the number of submissions required for eventual publication of many manuscripts, and thereby reduce the number of reviewers required to evaluate each manuscript between its first submission and eventual acceptance. This problem is being recognized as a serious issue in some quarters, as is apparent through the San Francisco Declaration on Research Assessment (DORA; <http://www.ascb.org/dora/>) and its many signatories. However for the abandonment of impact factors to have an effect in freeing up the reviewer pool would require many changes, such as other journals following the lead of the PLoS suite of journals in actively not publishing or advertising their impact factors, and those evaluating research performance (whether for grants, departmental evaluations, or individual promotion or recruitment) following the lead of the British Research Assessment Exercise in not considering journal impact factors in their recommendations and decisions.

The second is for the main ecological journals to move away from maintaining unnecessarily high rejection rates, a process that appears to be driven largely by journals competing with each other for high IFs (given recent developments in online publishing, the old argument of low acceptance rates being needed to adhere to publisher-imposed page limits is scarcely credible). Given that rejections by highly selective journals are often arbitrary (e.g. based on opinions of whether the work is in the most ‘novel’ 10%), and that many papers that they reject may well be just as important as those that they publish, many journals would not suffer in reputation if their acceptance rates were to increase modestly (note that citations of ecological manuscripts in *PLoS ONE* with a 31% rejection rate are on par with those in ecological journals that exercise 85% rejection rates; Wardle 2012). Even a modest increase in acceptance rates would likely greatly reduce the burden on reviewers, because most papers would involve fewer submissions (and thus rounds of review) before publication. As an example, a recent paper of ours was submitted in sequence to 5 journals of comparable standing (all with acceptance rates of about 15%) before acceptance—in all cases the evaluations were generally positive, and the manuscript was not seen as being much below the bar by any of them (the fifth time we just

happened to strike the right combination of reviewers and editor). The process seemed very stochastic, took about 18 months, and involved evaluation by 18 people (13 reviewers and 5 editors). If the first journal we tried instead had an acceptance rate of 25% or even 20% it would probably have been accepted in that journal, meaning that it would have consumed the time of only three people (two reviewers and an editor), which would have then freed up time for the 15 others that were also involved in its evaluation. My observation is that cases like this are not uncommon.

Hochberg (2014) identifies three mechanisms to counter the decline in evolution of scientific quality stemming from the ‘tragedy of reviewer commons’. In summary I suggest two further mechanisms which would greatly alleviate the burden on the reviewer pool and thus enhance the quality of the reviewing progress. One involves the abandonment of impact factors, and with assessment of science quality based solely on the impact of the work itself. The second is for journals to move away from maintaining unreasonably and unnecessarily low manuscript acceptance rates. Both will over time serve to reduce the number of journals many manuscripts are submitted to prior to acceptance, and therefore the number of reviewers required to evaluate them before they are eventually published. An added benefit is that this will reduce the time between submission and final publication of many manuscripts, which will help speed up scientific communication, understanding, and the evolutionary process that Hochberg (2014) describes.

References

- Campanario, J. M. 2009. Rejecting and resisting Nobel class discoveries: accounts by Nobel laureates. *Scientometrics* 81: 549–565. [CrossRef](#)
- Hairston, N. G., Smith, F. E. and Slobodkin, L. B. 1960. Community structure, population control and competition. *American Naturalist* 94:421–425. [CrossRef](#)
- Hochberg, M. E. 2014. Good science depends on good peer review. *Ideas in Ecology and Evolution* 7:77–83. [CrossRef](#)
- Lock, S. 1985. Letter to P. B. S. Fowler. *British Medical Journal* 290: 1560.
- Schoener, T. W. 1989. Food webs from the small to the large. *Ecology* 70: 1559–1589. [CrossRef](#)
- Seglen, P. O. 1997. Why the impact factor of journals should not be used for evaluating research. *British Medical Journal* 314:498–502. [CrossRef](#)
- Wardle, D. A. 2012. On plummeting manuscript acceptance rates by the main ecological journals and the progress of ecology. *Ideas in ecology and Evolution* 5: 13–15. [CrossRef](#)