

peer-reviewed functions. Efforts still need to be made in this area to improve the processes for sharing what is in effect the backbone of ecological software: its code. Therefore, there is an urgent need to embrace Stallman's four freedoms paradigm in ecology.

#### Acknowledgments

We would like to thank Anne Ghisla, Luca Delucchi and Tessa Say for valuable suggestions. DR is partially funded by the Autonomous Province of Trento (Italy) within the ACE-SAP project (University and Scientific Research Service regulation number 23, June 12, 2008).

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<http://dx.doi.org/10.1016/j.tree.2012.03.009> Trends in Ecology and Evolution, June 2012, Vol. 27, No. 6

# Type I error hinders recycling: a response to Rohr and Martin

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Rohr and Martin [1] propose that scientific reviews should be recycled in order to reduce the burden of repeated peer-review processes. Many other interesting initiatives are already reducing such burdens in different ways. For instance, and to cite only two examples, PLoS ONE is publishing papers based strictly on scientific quality and not on competition for limited space in a printed journal. This policy naturally reduces the number of reviews that a paper needs before publication, since reviews are only aimed to select *good* (and not *better*) manuscripts. A different enterprise, Peerage of Science, is trying to externalize the review process from journals. Although the system viability raises some doubts, there are also clear advantages [2,3]. Under the Peerage of Science model, journals pick papers that have had a good set of reviews and fit the journal scope and editorial policy. Since there is only one round of reviews, the reduction in the number of reviews needed to publish is evident. However, there is a risk that some papers might never be picked by any journal. The ideas of Rohr and Martin come along with all these novelties and are worth considering; however, their model presents some apparent problems.

The risk of having a Type I error publication bias (false positive) is inherent to any peer review process. At least theoretically, a bad scientific manuscript, if submitted a sufficient number of times, should eventually get published 'just by chance' (a.k.a. 'good luck with the referees'). Although we all want to believe that the number of submissions needed to publish a bad manuscript just by

chance is larger than the patience of a scientist, chances are that it might happen now and then (up to five out of 100 times, according to our current statistical dogma). Therefore, in a perfect editorial world, we should probably need a Bonferroni-like correction (or a Bayesian prior), in which a manuscript reviewed several times will need an increasing number of positive reviews in order to get published.

In the system proposed by Rohr and Martin, if an author decides *when* to forward a review, and decides *which* reviews are to be forwarded, there is a clear risk that only positive reviews will be forwarded, and that just by accumulation of those positives the manuscript will eventually get published. Worryingly, the number of submissions needed before an editor commits a Type I error might become dangerously low. If on each review process only half of the referees provide reasonably positive reviews, after only two or three submissions a questionable manuscript could be accepted for publication. An author might even want to be rejected by some mid-tier journals and then forward a selected collection of positive reviews to a top-tier journal and get their article published.

Some mechanisms could be developed to try to avoid these undesirable problems. For instance, editors might be willing to forward to other editors a *full set* of reviews, at the authors' request. Although this will alleviate the abovementioned problem, there is still a chance that more positive sets of reviews will be requested to be forwarded more often than more negative ones, resulting again in a milder but still significant risk of committing Type I errors. Besides, how many authors would wish to be evaluated

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based on a full set of reviews on which a rejection decision was made?

Another foreseeable problem would be that the same referee might produce a different review of a particular manuscript depending on the journal in which the author aims to publish their manuscript. For instance, a review on one of the top-ten journals would make much emphasis not only on scientific rigor but also on ease of reading, clarity, language style, and fit to the journal scope. However on a bottom-ten journal a referee is likely to give less importance to some of those same factors. Hence, many editors might find it difficult to recycle reviews from other journals.

Although the difficulty in finding referees is challenging to journal editors, ideas to improve the review process have to be pondered carefully before putting them into practice. The idea posed by Rohr and Martin might work, but if it

needs complicated control methods and risks being unattractive to both authors and editors, is it still worth it?

#### Acknowledgments

Thanks to Christopher Lortie for his feedback. Thanks to the Fundação para a Ciência e a Tecnologia – FCT for funding (SFRH/BPD/72595/2010).

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doi:10.1016/j.tree.2012.03.003 Trends in Ecology and Evolution, June 2012, Vol. 27, No. 6

## Type I error is unlikely to hinder review recycling: a reply to Montesinos

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In our recent letter in *TREE* [1], we argued that voluntarily forwarding responses to scientific reviews from rejected manuscripts to a subsequent journal upon resubmission could improve the efficiency of, and alleviate the burden on, the scientific review process. Montesinos [2] argues against voluntary review forwarding because he suggests that authors will only forward positive reviews, which will increase the chances of Type I errors or accepting papers for publication that should have been rejected. Although an increase in Type I errors is possible, his argument entails making several questionable assumptions.

First, Montesinos [2] assumes that editors and reviewers will thoroughly consider the forwarded reviews. As emphasized in our paper [1], editors and reviewers can ignore prior reviews or consider them along with newly solicited reviews. If editors choose the latter, it would provide more reviews than would be available if reviews were not forwarded, which should, on average, reduce rather than increase Type I errors. Even if editors evaluate the forwarded reviews and request the same number of reviews that they would have secured if reviews were not forwarded (which would not alleviate the burden on the scientific review process), at least the work of the previous review process is not being discarded, a serious inefficiency in the present review process raised by several authors [3].

A second assumption by Montesinos [2] is that editors would be influenced by previous reviews and would not consider the probably biased behavior of authors forwarding positive reviews more often than negative ones. We expect that both editors and reviewers will make their own decisions on manuscripts based on scientific quality rather

than peer pressure and that they are capable of weighting previous reviews to account for any bias in review forwarding. Although this requires editors and reviewers to make difficult judgment calls, this is already routine in the scientific review process.

The third assumption made by Montesinos [2] is that authors will only forward positive reviews. Authors generally prefer rapid decisions on manuscripts. Therefore, if editors frequently request fewer reviews when previous reviews are forwarded than when they are not, forwarding reviews should accelerate both publication and rejection decisions. As stated previously [1], these faster decisions should provide an incentive to forwarding even negative reviews if they can be adequately addressed. Montesinos' [2] notion that 'an author might even want to be rejected by some mid-tier journal and then forward a selected collection of positive reviews to a top-tier journal and get it published' is unlikely. Such an approach would be time consuming, would require the author to somehow manipulate the scientific review system to receive both positive reviews and a rejection, and would entail a higher tier journal being positively swayed by rejection at a lower tier journal (because the source of the reviews should also be forwarded). Ultimately, we expect review recycling to promote a scientific environment that encourages authors to address reviewers' concerns rather than mindlessly resubmitting their manuscripts until the 'roulette wheel finally lands on their number', a free-loader strategy that unnecessarily burdens scientific review [3,4].

Finally, Montesinos' [2] fourth assumption is that any costs of review recycling, such as Type I errors, must outweigh the benefits. Besides a verbal argument, there is presently little evidence that review recycling would

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