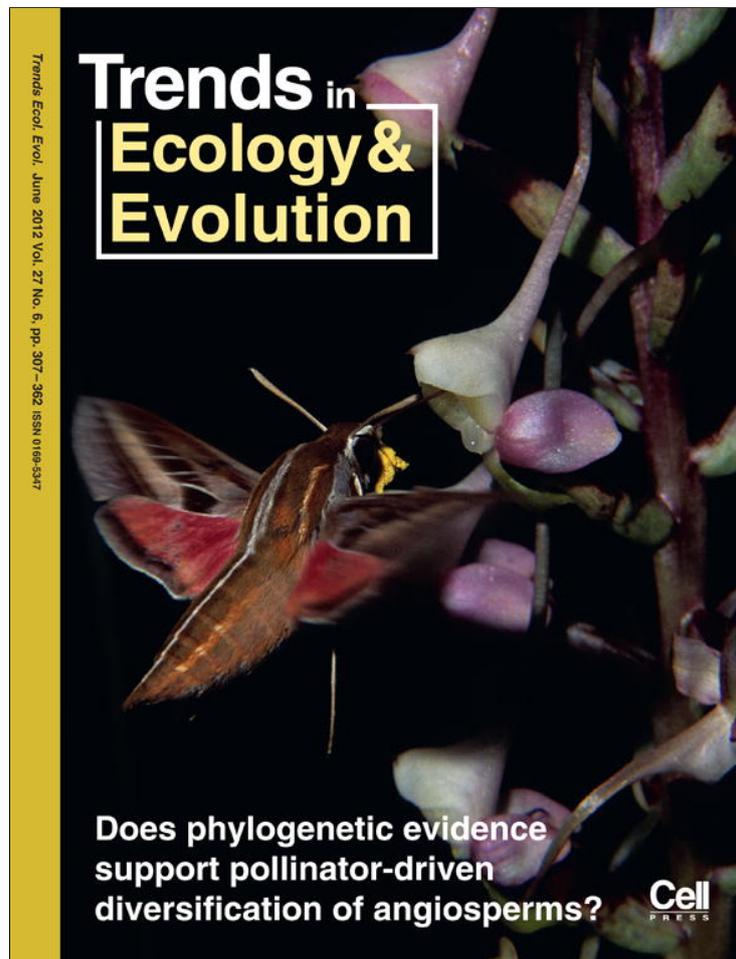


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based on a full set of reviews on which a rejection decision was made?

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?

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## 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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increase the publication rate of lower quality papers (see arguments above). However, there is compelling evidence that reducing reviewer burden increases the quality of scientific reviews ([http://www.nsf.gov/od/ipamm/ipamm\\_jtornow\\_finalreportnsb\\_070808.pdf](http://www.nsf.gov/od/ipamm/ipamm_jtornow_finalreportnsb_070808.pdf)). The net effect of review recycling on the quality and efficiency of science remains to be determined, but we doubt that there would be any noticeable reduction in the quality of published science if review recycling became common.

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