addressing this very issue in the *American Economic Review* found no significant effect of double-blind review on relative acceptance rates of papers including female authors [6], and the analysis of *BE* provides no additional evidence to suggest that double-blind review favours increased representation of female authors.

We recognise that women remain poorly represented at senior levels across the sciences [7–11], including ecology and evolution. Given the lack of evidence that double-blind review favours female authors, we suggest that efforts to address this imbalance should be directed into supporting innovative schemes [e.g. UKRC - Athena SWAN (http://www.athenaswan.org.uk)] aiming to change working conditions, including initiatives such as increased flexibility of working hours, support for scientists returning to research after career breaks and mentoring schemes [10]. We see no reason to direct time and resources into overcoming the acknowledged resistance of editors [1,2] and the scepticism of many in the field [1] regarding the alleged benefits of double-blind review.

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Letters Response

Response to Webb *et al.*: Double-blind review: accept with minor revisions

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With any given data set there is a large number of potential statistical and practical issues. In a study of the relative importance of double-blind review [1], we selected the most straightforward analytical approach *a priori* for comparison of data before and after a change in review policy, and for a simple comparison with other similar journals. Admittedly, a cornucopia of alternative post hoc approaches can be applied, some of which probably would fail to detect any change [2]. However, we were clear that our study was observational and that the changes occurring at the journal where double-blind review was introduced might be due to alternate variables. We recognise that one replicate does not constitute an effective test of a hypothesis; however, it was, unfortunately, all that was available. Nonetheless, the study is a compelling indication that changes in review policy can increase female representation through editor, reviewer or author behaviour. It is 7 years since *Behavioral Ecology* (*BE*) introduced double-blind review, and no other journals in the field have followed its example despite demonstrated differences in outcomes between reviewing methods [3] and prior evidence of bias in single-blind reviewing [4]. Until we have a number of replicate journals, we are unconvinced that the mixed-modelling techniques and critique proposed by Webb *et al.* [2] on a small and unbalanced data set are particularly illuminating.

We agree that data on manuscript submissions, acceptances and rejections would provide more insight into the value of double-blind review. However, these data are maintained by journals and are not freely available because of concerns over confidentiality and limitations of data extraction prior to electronic manuscript handling. We recognize that such data might reveal

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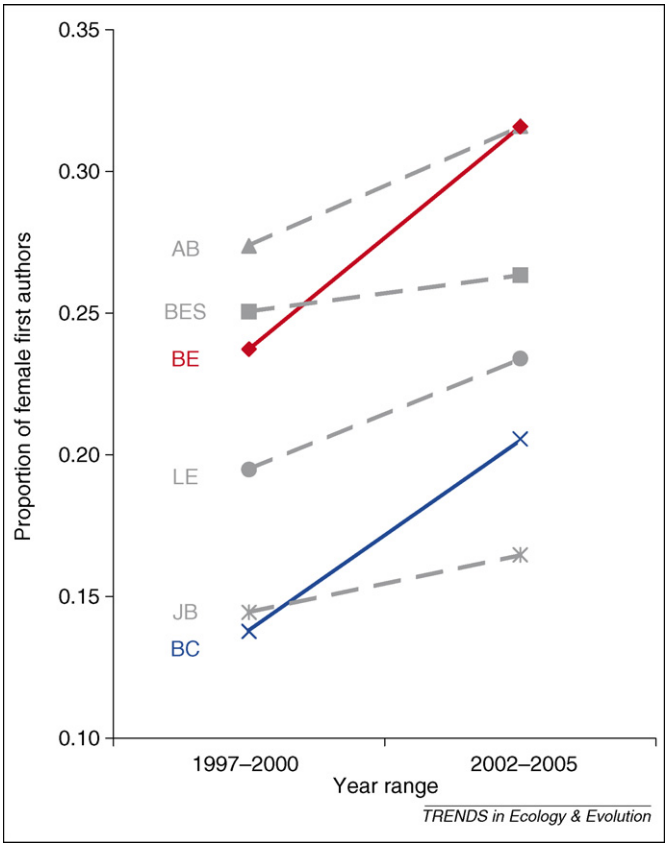

Figure 1. Change in the proportion of female first authors of papers published in six ecology and evolutionary biology journals taken from data in Budden and colleagues [1]. The figure calculates proportion of females by including authors of unknown gender (and, hence, more accurately reflects the analyses conducted by Budden *et al.* [1] than the inset figure in Webb *et al.* [2]). The journal *BE* (red) switched from single-blind to double-blind review in 2001 and exhibits the greatest change. The only other journal exhibiting a similar increase is *BC* (blue), a journal that suggests a double-blind system might be used in manuscript review. Abbreviations: *Animal Behaviour, AB; Behavioral Ecology, BE; Behavioral Ecology and Sociobiology, BES; Biological Conservation, BC; Journal of Biogeography, JB; Landscape Ecology, LE*.

increased submissions by females, and in our original article we did not suggest that the temporal increase in female first authors results from review policy alone, as inferred by Webb *et al*. [2]. However, a positive significant change was observed only in the double-blind journal.

The increase in female first authors within *BE* was 7.9% whereas the mean of the other journals was 3.7% ± 2.1 SD (Figure 1). So, *BE* falls on the 95% confidence interval (0.6%–7.9%). However, if we were to remove data from *Biological Conservation* (*BC*), the mean and standard deviation become 2.87 ± 1.4 SD, and *BE* falls well outside the 95% confidence interval (we note, however, that removal of *BC* does not impact the interpretations of the analyses by Webb *et al*. [2]). As indicated in our original article, the instructions to authors within *BC* suggest that a double-blind system '*may be*' utilized [1], and we cannot rule out this affecting author behaviour. Perception can be a strong driver of change within science in terms of the studies we conduct and ideas we test, and it is also probable that the journals we support might be selected based on attributes related to the review process. Indeed, preliminary results from an online survey suggest that females and less experienced authors show a preference for submission to a double-blind journal (Budden, A.E. *et al*, unpublished).

We find it discouraging that Webb *et al*. [2] find no reason to advocate consideration of alternative review policies. We do not propose double-blind review at the expense of any other initiatives dedicated to increasing the representation of women in science. However, the alternative of maintaining the status quo at the expense of a more objective review process that might benefit women, junior researchers and international scholars, because of the challenge in overcoming community scepticism, appears short-sighted and will limit the ability for more comprehensive questions about the effectiveness of journal review policies to be addressed in the future. Should the burden of proof not be on journals to examine existing data and explore alternative practices?

Note added in proof

Following acceptance of this letter, in April 2008 *Biological Conservation* changed their online instructions to authors removing reference to an anonymous cover page. The authors support increased transparency regarding review methods by all journals such as the clarification made by *Biological Conservation*.

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