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doi:10.1016/j.tree.2008.04.004 Available online 24 May 2008

Letters

Does double-blind review benefit female authors?

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Peer review is widely held to be essential for enhancing the quality of scientific communications [1]. Opinions differ, however, as to how to ensure that reviews are as fair and objective as possible. Most ecology and evolution journals employ a single-blind system, which conceals reviewer, but not author, identity. An alternative is double-blind review (both author and reviewer identity is concealed), which, surveys of authors and reviewers suggested, is popular [1] despite the fact that many of those surveyed actually had not experienced the system and that reviewers often like to know the identity of an author so

that new work can be placed in context [1,2]. Editors, too, have resisted a switch to double-blind review [1,2], citing a lack of evidence that it is really beneficial [2]. A recent claim of Budden and colleagues [3] that double-blind review favours increased representation of female authors is important, therefore, as it promises tangible evidence in favour of this system.

Budden and colleagues base their conclusion on the journal *Behavioral Ecology (BE)*, which switched to double-blind review in 2001; they compare the number of male and female first authors immediately before and after this switch for *BE* and five other journals. We summarise the trend across all six journals in Figure 1. Testing each journal separately, Budden and colleagues conclude

DOI of original article: 10.1016/j.tree.2008.04.001.

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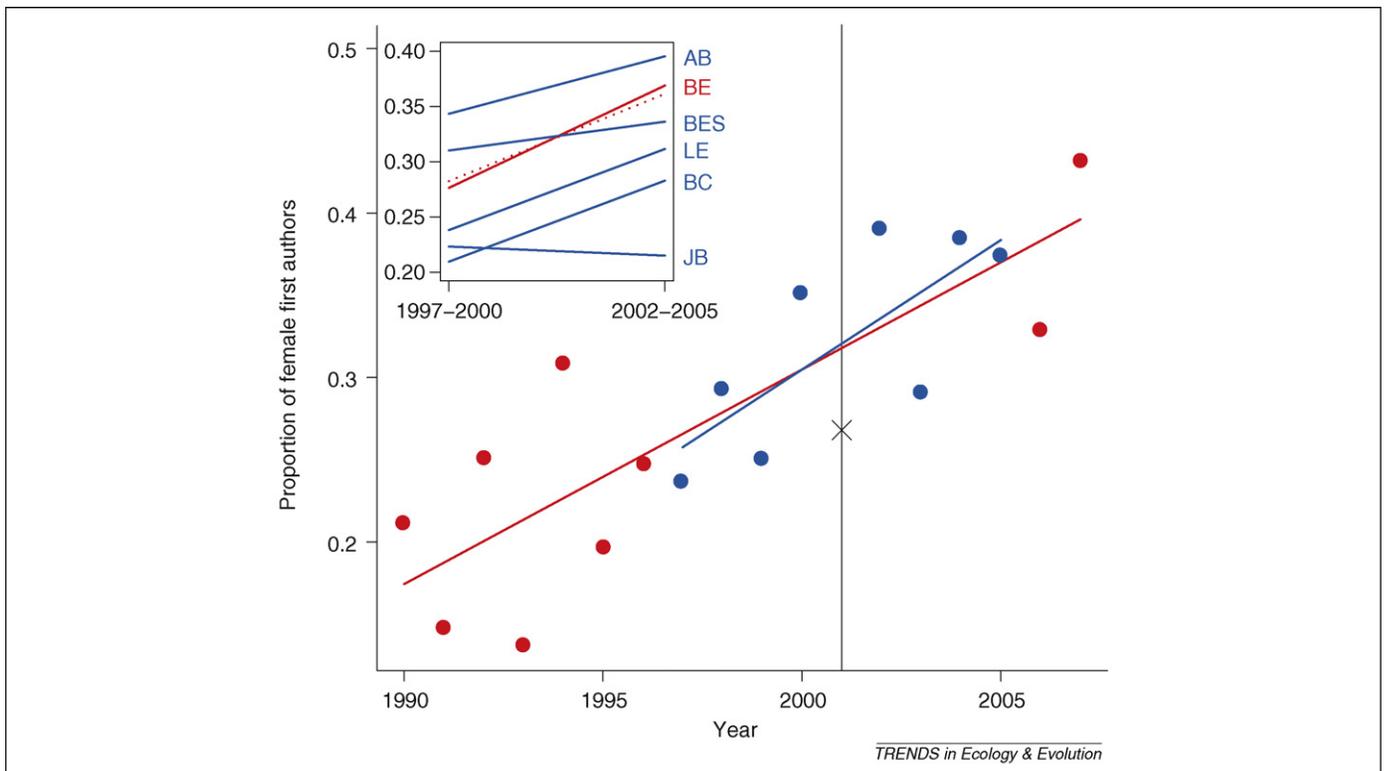


Figure 1. The proportion of papers with a female first author in *BE*, volumes 1 (1990) to 18 (2007), with the years used by Budden and colleagues shown in blue. We counted the number of male and female first authors for all complete volumes of *BE* from tables of contents available at <http://beheco.oxfordjournals.org/archive>, omitting authors who only provided initials, had a gender-neutral name or a name of unknown gender (i.e. the ‘unknown’ category of Budden and colleagues). We exclude the unknown category from our calculation of proportional female authorship, although this has no effect on our conclusions. Fitted regressions using all data (red line) or only the years considered by Budden and colleagues (blue line) show the linear increase in female authorship over time. In all cases (all years or restricted years, female authorship as a proportion of all papers or of those in which author gender was known) there was no observable additional effect of the switch to double-blind review in 2001 (vertical line), as models assuming a simple linear increase over time in proportional female authorship always performed better (lower Akaike’s Information Criterion) than models in which an abrupt change in female authorship post-2001 was assumed (either in addition to or instead of the linear trend). The inset (axes as for the main figure) shows the general increase in the proportion of female-authored papers across six ecology and evolution journals between 1997 and 2000 and 2002 and 2005, from data in table 1 of Budden and colleagues (using only papers for which the first author’s gender was known). All journals practised single-blind review in both periods apart from *BE* which introduced double-blind review in 2001. *AB*, *Animal Behaviour*; *JB*, *Journal of Biogeography*; *LE*, *Landscape Ecology*. For comparison, the annual data that we collected for *BE* were summed over the same time periods and are shown as a dotted line.

that the increase in proportional female authorship in *BE* is greater than that seen in most of the single-blind journals [*Biological Conservation (BC)* is the exception]. However, as **Figure 1** makes clear, the upward trend in *BE* is seen across the six journals. We tested whether the increase in *BE* has been exceptional by fitting a generalized linear mixed model (GLMM) with Poisson errors and a log-link using the `lme4` package in R 2.6.2 [4,5]. We modeled the number of papers as a function of gender, time (pre-2001 or post-2001) and whether the publication switched to double-blind review (true for *BE*, false for all other journals), with journal fitted as a random effect. We found a significant interaction between gender and time (coefficient \pm SE = 0.25 ± 0.060 , $P < 0.0001$), reflecting the higher female authorship post-2001 than pre-2001, but there was no significant interaction between gender and review type (-0.15 ± 0.102 , $P = 0.134$), indicating that the increase in female authorship over time in *BE* is not exceptionally different from the changes in the other journals in the field. This conclusion is robust to the details of data [e.g. if authors of unknown gender are included or if a paired comparison with *Behavioural Ecology and Sociobiology (BES)* is performed] and to the modeling framework employed (log-linear models produce comparable results). Furthermore, annual data on the number of

female authors in *BE* show that the increase in proportional female authorship has been linear over the life of this journal, with no additional effect of the switch to double-blind review in 2001 (**Figure 1**).

Budden and colleagues call for the ecological and evolutionary community to revisit the issue of peer review in light of their study of *BE*, and their claim that double-blind review benefits female authors already has been cited in an editorial in *Nature* [2] that has generated heated online discussion [Peer-to-Peer: Working double-blind (http://blogs.nature.com/peer-to-peer/2008/02/working_doubleblind.html)] and has been used to support the piloting of double-blind peer review in the National Institutes of Health [National Institutes of Health 2007–2008 Peer Review Self-study, Final Draft (<http://enhancing-peer-review.nih.gov/meetings/NIHPeerReviewReportFINALDRAFT.pdf>)]. This is despite the fact that the only evidence they supply is an increase in female first authorship in a single journal (an increase also seen in other journals that did not switch to double-blind review) rather than anything more compelling, such as a demonstration that the ratio of accepted to submitted manuscripts had increased for females relative to males after the introduction of double-blind review. An extensive randomised study

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.

Acknowledgements

Thanks to Alison Holt for discussion and to Katja Bargum and two anonymous referees for valuable comments. T.J.W. is a Leverhulme Early

Career Research Fellow. R.P.F. is a Royal Society University Research Fellow.

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doi:10.1016/j.tree.2008.03.003 Available online 29 April 2008

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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