

# Double-blind review favours increased representation of female authors

Amber E. Budden<sup>1,2</sup>, Tom Tregenza<sup>3</sup>, Lonnie W. Aarssen<sup>4</sup>, Julia Koricheva<sup>5</sup>, Roosa Leimu<sup>6</sup> and Christopher J. Lortie<sup>7</sup>

<sup>1</sup> Department of Ecology & Evolutionary Biology, University of Toronto, Toronto, Ontario, Canada M5S 3G5

<sup>2</sup> National Center for Ecological Analysis and Synthesis, University of California, Santa Barbara, CA 93101, USA

<sup>3</sup> Centre for Ecology and Conservation, University of Exeter, Cornwall Campus, TR10 9EZ, UK

<sup>4</sup> Department of Biology, Queens University, Ontario, Canada, K7L 3N6

<sup>5</sup> School of Biological Sciences, Royal Holloway, University of London, London, TW20 0EX, UK

<sup>6</sup> Institute for Biochemistry and Biology, University of Potsdam, Potsdam, Germany, 14476

<sup>7</sup> Biology Department, York University, Ontario, Canada, M3J 1P3

**Double-blind peer review, in which neither author nor reviewer identity are revealed, is rarely practised in ecology or evolution journals. However, in 2001, double-blind review was introduced by the journal *Behavioral Ecology*. Following this policy change, there was a significant increase in female first-authored papers, a pattern not observed in a very similar journal that provides reviewers with author information. No negative effects could be identified, suggesting that double-blind review should be considered by other journals.**

## Single and double-blind review

Peer review is an operational standard that ensures the fair assessment of research quality. However, a widespread concern that the perceived merit of a research paper, grant application or researcher might be affected by factors other than scientific quality seems valid [1]. Gender, familiarity and country of origin have been shown to affect reviewer behaviour [2,3], suggesting that the practice of single-blind review, in which reviewer but not author identity is concealed, might be biased. Double-blind review (in which both author and reviewer identity are concealed) is an obvious solution, so why do so few journals outside of medicine, psychology and economics [4] practice this policy?

One concern is that it might be possible to infer author identity from information such as the study system or cited work [4,5]. However, evidence from other disciplines has shown that when encouraged to guess the identity of the author or institution, reviewers were correct on only 25–42% of occasions [6–8]. Other concerns include the decreased ability to detect publication of the same data across multiple papers [9], lost potential for increased feedback to more junior authors [10] and concerns regarding increased workload associated with double-blind review [9–11].

Although the greater scientific community largely favours double-blind review [4,10–12], few ecology and evolutionary biology journals practise this policy. One

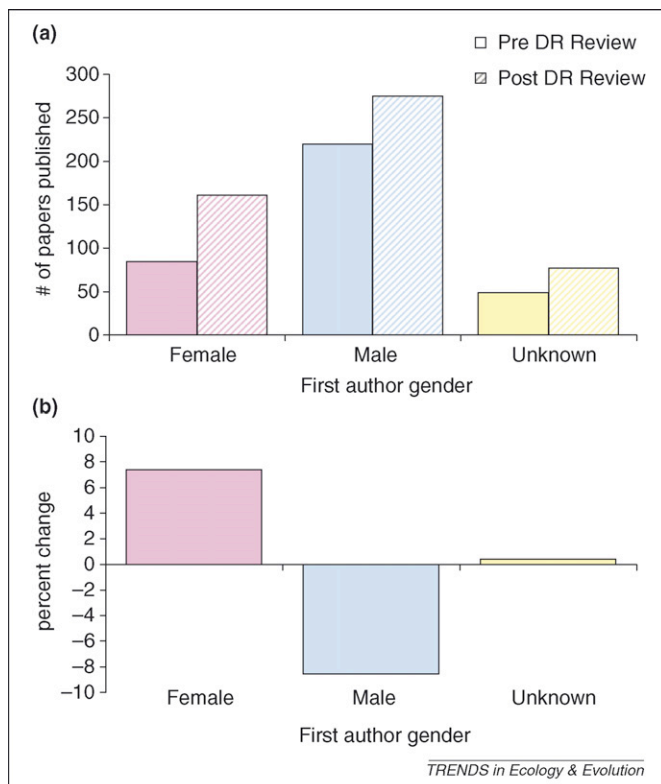
notable exception is *Behavioral Ecology* (BE), which initiated double-blind review in 2001 in an attempt to enhance the review process [13]. This policy change provides a unique opportunity to examine variation in author demographics associated with the review method. We compared BE with *Behavioral Ecology and Sociobiology* (BES), a single-blind journal with very similar subject matter and impact factor, and with an additional subset of ecology and evolutionary biology journals.

## A case study

Using online tables of contents, we generated a database of all papers published in BE between 1997 and 2005 ( $n = 867$ ). We omitted the year 2001, to accommodate the change in editorial policy, and removed book reviews, editorials, errata and miscellany. For each paper, gender was assigned to the first author using first names. Gender was classified as ‘unknown’ if the author provided only initials, if the name was gender neutral or if the name could not be assigned to either gender [14]. Personal knowledge of the gender of specific individuals was not used. We extracted the same data from an out-group set of primary research journals listed by ISI as being in the category ‘Ecology’ or ‘Evolutionary Biology’, with a 2004 impact factor of 2.0–2.5 (similar to that of BE). These journals also had online tables of contents that listed author first names. This provided an additional five journals: BES;  $n = 1040$ ), *Animal Behaviour* (AB;  $n = 2178$ ), *Journal of Biogeography* (JB;  $n = 1040$ ), *Biological Conservation* (BC;  $n = 1719$ ) and *Landscape Ecology* (LE;  $n = 419$ ). Missing data from complete issues omitted from the table of contents were inserted using ISI (JB and LE; four issues).

Representation of female, male and unknown first authors was examined using a Chi-square test of independence across the three gender categories and two time periods (1997–2000 and 2002–2005) within each journal. Where a significant association was found to exist between time and gender, we used a z-test to compare the proportion of female first-author publications represented in the early versus recent time period.

Corresponding author: Budden, A.E. (aebudden@nceas.ucsb.edu).



**Figure 1.** Papers published in Behavioral Ecology by first-author gender. (a) Total number of papers published in BE in the four years before and after the implementation of a double-blind review policy in 2001. (b) Percentage change in author representation.

### Double-blind review in ecology and evolutionary biology

The number of papers published by BE has shown a significant increase since 1997 ( $F_{1,7} = 49.53$ ,  $P < 0.01$ ) and this is comparable to BES, which continues to practice

single-blind review (test of slopes  $t_{14} = 0.75$ ,  $P = 0.46$ ). Similarly, the annual mean number of citations per paper did not vary between these journals (test of slopes  $t_{14} = 0.29$ ,  $P = 0.77$ ). Therefore, it would seem that there have been no negative effects following the change in editorial policy.

In the four years following the introduction of double-blind review, BE published more papers by both genders ( $\chi^2_2 = 6.99$ ;  $P = 0.03$ ) (Figure 1a and Table 1). However, the magnitude of this difference was significantly larger for females than for males. Following double-blind review, there was a 7.9% increase in the proportion of papers with a female first author ( $z = 2.57$ ,  $P = 0.01$ ) and a corresponding decrease in papers with a male first author (Figure 1b and Table 1). Such a response to double-blind review has not been previously documented. If the proportion of women in the field has increased, or increased in productivity, we would predict a commensurate change in authors publishing in BES. However, we found no significant difference in gender representation across the same time period in BES ( $\chi^2_2 = 1.58$ ,  $P = 0.45$ ), which strongly suggests that the change is directly related to review policy.

We extended our analysis across the journals detailed above. Of these, only BC offers a double-blind review option, indicating that authors should:

*...set up two title pages ... the first title page contains all authors' contact information ... and may be separated from the manuscript for the review process*

([http://www.elsevier.com/wps/find/journaldescription.cws\\_home/405853/authorinstructions](http://www.elsevier.com/wps/find/journaldescription.cws_home/405853/authorinstructions)). There was no significant increase in the percentage of papers published by female first authors over the same time period, with the exception of BC ( $\chi^2_2 = 16.36$ ,  $P < 0.01$ ;  $z = 3.67$ ,  $P < 0.01$ )

**Table 1.** Number of papers published in six ecology and evolutionary biology journals in the years 1997–2000 and 2002–2005 by gender of the first author

	Gender			Chi-square				z-test	
	Female	Male	Unknown	$\chi^2_2$	$P$	$w$	Power	$\Delta F^a$	$\Delta M$
<b>BE</b>									
1997–2000	84	220	50	6.99	<b>0.03</b>	0.19	1.00	↑* <sup>b</sup>	↓*
2002–2005	162	277	74						
<b>BES</b>									
1997–2000	116	258	89	1.58	0.45	0.09	0.74	NA <sup>c</sup>	NA
2002–2005	152	300	125						
<b>AB</b>									
1997–2000	306	585	227	5.07	0.08	0.11	1.00	NA	NA
2002–2005	335	512	213						
<b>BC</b>									
1997–2000	84	317	209	16.36	<b>&lt;0.01</b>	0.22	1.00	↑**	<b>NS</b>
2002–2005	228	578	303						
<b>JB</b>									
1997–2000	52	181	127	16.77	<b>&lt;0.01</b>	0.25	1.00	<b>NS</b>	↑*
2002–2005	112	409	159						
<b>LE</b>									
1997–2000	30	96	28	4.63	0.10	0.22	0.98	NA	NA
2002–2005	62	137	66						

Chi-square and associated  $P$  values, effect sizes ( $w$ ) and power estimates reflect analysis across the three gender categories and two time periods. Significant results and associated z-tests highlighted in bold.

<sup>a</sup> $\Delta F$  and  $\Delta M$  represent the results of a *post hoc* z-test of proportions on the representation of female or male first authors across the two time periods.

<sup>b</sup>Upward arrows represent an increase; downward arrows represent a decrease.

<sup>c</sup>NS,  $P > 0.05$ ; \* $P < 0.05$ ; \*\* $P < 0.01$ ; NA, *post hoc* analyses not warranted.

(Table 1). This variation might reflect a demographic shift in discipline of conservation biology, which might be independent of changes in behavioural ecology. It might also reflect variation in author submission behaviour, given the perception of increased anonymity. However although the most intuitive explanation is that the review policy reduced the potential for bias in the review process, double-blind review is not practised at BC, and increased submissions by females probably explains the observed pattern (R. Marrs, personal communication).

The variation in gender representation in JB (Table 1) was not attributable to a change in the proportion of papers published by female first authors ( $z = 0.87$ ,  $P = 0.39$ ) but, rather, increased publishing by male first authors (male first-authored papers;  $z = 3.05$ ,  $P = 0.01$ ).

### Potential impact of journal review policies

A difference of 7.9% in the proportion of female first-authored papers following the implementation of double-blind review in BE is three times greater than the recorded increase in female ecology graduates in the USA across the same time period (<http://www.nsf.gov/statistics/nsf07305/>) and represents a 33% increase in the representation of female authors. Furthermore, this increased representation of female authors more accurately reflects the (US) life sciences academic workforce composition, which is 37% female (<http://www.nsf.gov/statistics/wmpd/employ.htm>).

The consequences of this shift could extend beyond publications. If females are less successful in publishing research on account of their gender, then given the current practices associated with appointment and tenure, and the need for women dramatically to out-compete their male counterparts to be perceived as equal [3], any such publication bias impedes the progress of women to more advanced professional stages.

It is worth noting, however, that because there are fewer women in more senior positions [15] (<http://www.nsf.gov/statistics/wmpd/employ.htm>), increased acceptance of manuscripts by less established researchers (who might be hypothesized to benefit more from a double-blind review policy) would result in increased representation of females.

Double-blind review is frequently criticized on the grounds that it involves an increased administrative load

and that authors can be readily identified. However, the more compelling issue is whether double-blind review makes a difference. In light of our study, and evidence that the ecology and evolutionary biology community support double-blind review [12], now might be the time for journals to revisit this issue.

### Acknowledgements

This work was conducted as part of the 'Role of publication-related biases in ecology' Working Group supported by the National Center for Ecological Analysis and Synthesis, a Center funded by the NSF (Grant no. DEB-0072909), the University of California, Santa Barbara, and the State of California. We are grateful to Letitia Grenier, Graeme Ruxton and two anonymous referees for comments on this manuscript, and to Katrina Lythgoe for recommendation of gender-appropriate figure colours.

### References

- Lortie, C. *et al.* (2007) Publication bias and merit in ecology. *Oikos* 116, 1247–1253
- Link, A.M. (1998) US and non-US submissions – an analysis of reviewer bias. *JAMA* 280, 246–247
- Wenneras, C. and Wold, A. (1997) Nepotism and sexism in peer-review. *Nature* 387, 341–343
- Mainguy, G. *et al.* (2005) Peer review – the newcomers' perspective. *PLoS Biol.* 3, 1534–1535
- Møller, A.P. and Jennions, M.D. (2001) Testing and adjusting for publication bias. *Trends Ecol. Evol.* 16, 580–586
- Yankauer, A. (1991) How blind is blind review. *Am. J. Public Health* 81, 843–845
- van Rooyen, S. *et al.* (1998) Effect of blinding and unmasking on the quality of peer review – a randomized trial. *JAMA* 280, 234–237
- Katz, D.S. *et al.* (2002) Incidence and nature of unblinding by authors: our experience at two radiology journals with double-blinded peer review policies. *Am. J. Roentgenol.* 179, 1415–1417
- Pitkin, R.M. (1995) Blinded manuscript review – an idea whose time has come. *Obstet. Gynecol.* 85, 781–782
- Stensrud, D.J. and Brooks, H.E. (2005) The future of peer review? *Weather and Forecasting* 20, 825–826
- Regehr, G. and Bordage, G. (2006) To blind or not to blind? What authors and reviewers prefer. *Med. Educ.* 40, 832–839
- Smit, C. (2006) Peer review: time for a change? *Bioscience* 56, 712–713
- Cuthill, I. *et al.* (2000) Editor's report. In *ISBE Newsletter* 12, 6–10
- Tregenza, T. (2002) Gender bias in the refereeing process? *Trends Ecol. Evol.* 17, 349–350
- Neugebauer, K.M. (2006) Keeping tabs on the women: life scientists in Europe. *PLoS Biol.* 4, 494–496

0169-5347/\$ – see front matter © 2007 Elsevier Ltd. All rights reserved.  
doi:10.1016/j.tree.2007.07.008 Available online 25 October 2007

### Letters

## Island rules cannot be broken

Meike Köhler<sup>1</sup>, Salvador Moyà-Solà<sup>1</sup> and Richard W. Wrangham<sup>2</sup>

<sup>1</sup>ICREA and Institut Català de Paleontologia, Department of Biologia Animal, Vegetal, i d'Ecologia (BAVE), Universitat Autònoma de Barcelona, 08193 Bellaterra, Spain

<sup>2</sup>Department of Anthropology, Peabody Museum, Harvard University, 11 Divinity Avenue, Cambridge, MA 02138, USA

In a recent article in *TREE* [1], Niven reviewed the controversy over *Homo floresiensis* [2], a new fossil homi-

nine species from the island of Flores. To our knowledge this was the first attempt to explain how small brain size, short stature and certain morphological oddities in *H. floresiensis* conform to island life. We applaud his

Corresponding author: Köhler, M. ([meike.kohler@icrea.es](mailto:meike.kohler@icrea.es)).