Reviewer’s report

Title: Uptake and outcome of manuscripts in Nature journals by review model and author characteristics

Version: 0 Date: 06 Mar 2018

Reviewer: Victoria Wong

Reviewer's report:

With the current trend towards full transparency in peer review (e.g., https://scholarlykitchen.sspnet.org/2017/08/03/transparent-peer-review-mean-important), research on double-blind peer review seems less relevant. I think the Background needs to acknowledge this fact and make a stronger argument for the continued relevance of double-blind peer review to justify this study. Some arguments may include the fact that reviewers are more likely to accept an invitation for review in a double-blind model (https://authorservices.wiley.com/asset/photos/reviewers.html/PeerReviewStudyReport_summary.pdf) or that many researchers have confidence in double-blind peer review (http://blogs.nature.com/peer-to-peer/2008/02/working_doubleblind.html). It appears that Line 95 of the manuscript was supposed to have a reference supporting the introduction of double-blind peer review into Nature-branded journals, but does not appear to have been cited properly (is this reference #19?). This reference may be a good springboard for the discussion in support of double-blind peer review as a plausible option.

The Abstract's Results section should contain both absolute numbers as well as percentages, and also p-values when relevant.

In the Background section, particularly in the first paragraph, reference citations should be consistently formatted (numbered consecutively, in square brackets). It looks like some are not formatted this way such as "(Brown, 2006)" and "Falagas et al. (2006)."

The Background is a bit lengthy with redundancy. It would be reasonable to summarize and group study findings, then group references together, particularly in the paragraph on gender and institutional biases.

In the Methods section, the number of records excluded need percentages attached (e.g., 5,011 records excluded for review type, 22,081 transferred manuscripts, 27,245 records with unknown gender.) Perhaps an additional figure would make the dataset more clear (depicting how the dataset went from 133,465 unique records down to 128,454, then down to 106,373 direct submissions, and the further breakdown to three different journal categories). This figure should be comparable to a classic CONSORT 2010 flow diagram.
On Line 157, what is a non-ranked institution and how were these handled in statistical analysis? Why were they non-ranked? Were they excluded from analysis? How many institutions were in that group? It seems reasonable to keep them in the analysis just as another category (non-ranked institution, category 4) for descriptive statistics.

It looks like on Line 208, the acronym "DB" is introduced without a definition then used occasionally thereafter. All incidences of "DB" can probably be changed back to "DBPR" since the terms seem interchangeable.

One major weakness of the manuscript was the large number of excluded manuscripts. For example, 17,379 manuscripts had a female corresponding author, 83,830 had a male corresponding author, and 27,245 had a corresponding author of unknown gender and were excluded from analysis. The large number of exclusions here could skew the results significantly, particularly if there was some kind of confounder such as if one gender had more names that could not be identified using the Gender API service.

In the Results section, many of the tables can be grouped and merged such as Tables 1, 2, and 3; Tables 5, 6, and 7; Tables 10, 11, and 12, etc. It's unclear to me, however, why the numbers don't add up for these latter two "triads" of tables. For example, in Table 10, the number of accepted DBPR papers is 242. In Table 12, 35 + 137 = 172, not 242. Likewise, in Table 10, the number of accepted SBPR papers is 8,692. In Table 12, 1,187 + 6,297 = 7,484, not 8,692. It seems like these inconsistencies are due to all the aforementioned manuscript exclusions including those that were transferred to another journal and those where the gender of the corresponding author was unknown. I will defer to a statistical reviewer to determine how to best present this data with all the exclusions, but it seems to me that combining these tables allows for the most clarity.

Weaknesses of the study should be discussed in the Discussion section, in particular, all the missing data (e.g., unidentified gender of corresponding authors, records without review type, transferred manuscripts, non-ranked institutions).

One way to answer some of the questions speculated upon in the Discussion section (for future study) would be to survey corresponding authors about their motivations for choosing double-blind peer review.

I think the Discussion section is a fair assessment of the study results, without overreaching or implying causation over correlation.

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