Reviewer’s report

Title: Quantifying Professionalism in Peer Review

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Reviewer: Bahar Mehmani

Reviewer's report:

This is a nice study and provides yet another reason for journals to measure the quality of their referees' performance. It is clear that authors have spent a great deal of time reading almost 1500 peer review reports. However, in order to make it more efficient and clear authors need to reconsider parts of their study:

- differences between external and internal peer reviewers: It is not clear which percentage of the studied peer review reports are written by internal peer reviewer such as editorial boards or editorial office members. The handling editor might have a more relaxed approach toward such groups of referees and not necessarily read the entire body of the reviewer comment.

- differences between the peer review policies of journals: It is not clear which peer review policies (single/double/tripple blind or open peer review) have been considered. This impacts a particular category about missing link to a required article. In case of double-blind policy, it might be that the reviewer originally requested authors for a self-citation which breaches the peer review policy of the journal. Editors/editorial offices of double blind journals usually scan the content to remove reviewer's self-citations.

- subject area limitations: While it is understandable that authors chose subject areas closer to their field, the lack of study on other fields limits the presented results. To put this in perspective, it is important to mention how big the size of these specific communities are since peer review is a human interaction and people behave differently in closer/small communities than in wider groups.

- anonymity: Although it is stated that to respect anonymity authors did not control for reviewer/author gender, ethnicity, academic ranking, I don't think running such detailed studies provided having a properly defined categories, would harm anonymity. It would be quite a complementary and insightful result to know if younger/women/global-south affiliated authors receive harsher reviews for example.

- Correlation with recommendation type, editor decision, and resubmissions: The study does not address which percentage of the referee reports are correlated with ultimate recommendation, i.e., accept, minor/major revision, reject. It would be insightful to see the correlation of 'unprofessional' reviews with negative recommendation types. It is also not clear if these authors have received their 'unprofessional peer review' for the first time submission or in follow up submissions after being rejected from a different journal.
- article types: it is not clear for which article types these collected peer reviews were submitted, i.e., researcher article, review article, case study, etc. This needs to be clearly mentioned.

- non-exclusive attributions: none of the 7 listed categories are truly defined and considered as exclusive. In fact the first group, 'unprofessional' covers almost the rest of the group. There is another study very similar to what is presented here that is specifically classifying such attributions in a non-exclusive manner. It would be beneficial if authors check that study and try to replicate using the same method (reference: https://doi.org/10.1016/j.jeap.2020.100867)

- inter-rater agreement: it is not clear if one or all authors read the entire body of the collected referee reports and mapped to these categories, or there were some inter-rating mechanism in place.

- Comparison between two samples: It is not clear what the subject areas of the 6 PIs who voluntarily shared their history of peer reviews are. Also, it is not mentioned which proportion of shared peer reviews by this group is related to what recommendation term. As a result in conclusion we miss a clear reason why this group has received more 'unprofessional' referee report and which period of their career. Where these reports received from the same group of journals or they changed in the course of time?

In light of these items, I suggest authors to reconsider their study and address these points.

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