Reviewer's report

Title: Misconduct in research: A descriptive survey of attitudes, perceptions and associated factors in a developing country

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Reviewer: Daniele Fanelli

Reviewer's report:

In my previous review, the main recommendations made to the authors were to provide more information about the methods used, and dedicate more attention to presenting and discussing their results against the background of existing literature. The authors have revised the manuscript following these recommendations, but I feel that major revisions are still required to make the paper a useful contribution to the field.

Major Compulsory Revisions

The Methods section has substantially improved, although I join another reviewers’ request to provide, in an appendix to the paper, the actual questionnaire used. In order to evaluate the results presented in the study, interested scholars will need to read the actual questionnaire straight away, so including it in the publication, as an appendix, is compulsory.

In the Results and discussion section, readers risk getting confused about what exactly each question was asking – in particular, if respondents were reporting on their own beliefs or on their actual knowledge, and which definition of misconduct was being used. Indeed, I think that in my previous review I was confused about this, too (see also comments on the discussion).

Presenting the actual questionnaire along with the paper, as suggested above, should help clarify any doubts, but the paper itself needs to make these distinctions clearer. In the subs-section entitled “Perception of frequency of occurrence…”, the first two paragraphs are, if I am understanding correctly, about beliefs (with data reported in a table). The last paragraph, however, seems to be about actual perception of prevalence, gained either through personal experience or through second-hand knowledge, of “misconduct” as defined in this particular study (i.e. using a very broad definition). If I got this right, then the authors could consider a separate subheading for this data, which is of a different kind to the above, and relevant for the purposes of estimating actual prevalence.

More importantly, the authors should specify the definition of misconduct that was used for these questions, even if by doing so they have to repeat information given elsewhere. I wonder, however, if by any chance they have such data in a disaggregated fashion, in which case they should definitely report this information in a separate table.
In the Discussion section, the study seems to perpetuate a confusion between results about beliefs and results about actual knowledge, and about definitions of misconduct used. It does so in the way it re-presents its own results and compares them to those of previous studies. For example, in the first paragraph of the Discussion, it states that “only 9.1% of our researchers said that falsifying data had never occurred at their workplace” and then compares this datum to a previous study on non-self reporting, even though these results were presented in the results section as being about “perceived occurrence”. Were the respondents asked about what they believed or about what they knew to be the case? The two kinds of results have very different implications. This is just an example. Below I will make a few more observations about the Discussion section, and then make a suggestion on how to revise it.

In the first paragraph of the Discussion section, the authors cite my meta-analysis as saying that “studies are difficult to compare” but that’s not at all why I was suggesting them to use it. This is not why I suggested referring to the meta-analysis, however. That statement appeared in the introduction to the meta-analysis, and was not one of its conclusions – indeed, by the very fact that a meta-analysis was done, my study partially overturned that statement. Most importantly, however, the real point about citing the meta-analysis in the discussion was because it had showed that method of questionnaire delivery and, more importantly, the way questions are used make a large difference on the outcome of surveys.

Admission rates, in particular, turned out to increase in proportion to how generic and comprehensive the questions (i.e. definitions of misconduct) are. Indeed, the finding that 30-50% of respondents had personal knowledge of one case of misconduct - broadly defined - in their own institution would be rather in line with results of other surveys, in which studies asking about very generic behaviours had up to over 70% admission rates.

The above datum is the only one from the meta-analysis that can be compared to those of this study. All other data, which is about perception, can, and should, be compared to other studies on perception. In the discussion, instead, results of this study are ambiguously compared to a few previous surveys about prevalence, as in the example given previously. There are plenty of studies assessing the beliefs of researchers around issues of misconduct. The authors may find a preliminary list in the appendix to the meta-analysis, where studies on perception where excluded because they did not report data on occurrence of misconduct.

In sum, to avoid any confusion in the reader, and generally improve the usability of the study by other researchers, I suggest that the Discussion section should be revised as the following:

- first, after having quickly summarised the scope and findings of the survey, the Discussion should acknowledge general limitations: explain that this is a small, preliminary study, that the definition of misconduct was broad, and all other flaws that are now mentioned in the discussion section.
- then, discuss the results one by one (or rather group by group), in the order in which these are presented in the results section. Indeed, the authors could even consider subheadings that mirror those in the results section.

- in each of these sub-sections of the discussion, the authors will then be able to compare results to the appropriate previous studies, and discuss the specific limitations (for example, the use of a broader definition of misconduct in some questions)

- the authors can then conclude the discussion with their conclusions and recommendations.

Minor Essential Revisions

In the methods section, the authors should indicate the academic field of the conference. Answering to my previous request, the authors pointed out the need to preserve the anonymity of respondents, which is understandable. However, later in the discussion they mention that the conference was about a “medical specialty”. Therefore, at least this information (and perhaps something a bit more specific) can be provided in the methods, too.

It is not entirely clear, in the methods section, which parts of the SMQ-R are presented in this study. Perhaps this point could be clarified.

Discretionary Revisions

I found it extremely interesting that an independent study, now cited in the introduction, on misconduct in Nigeria yielded very similar results to this survey. High admission rates and findings that egregious forms of misconduct are reported/perceived more frequently than QRPs were common to both studies. Such replication reinforces (as replications always do) the credibility of this study, and suggests that patterns of scientific misconduct might indeed be different in Nigeria compared to the US or other countries. I would encourage the authors to elaborate the comparison with this independent study, and emphasize the similarities and differences.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests