Author's response to reviews

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

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Author's response to reviews: see over
23rd December 2013

Adrian Aldcroft
Executive Editor
BMC series journals
BioMed Central

Sir,

**COVER LETTER**

We are very grateful for the invaluable comments of the reviewers. We have revised the manuscript in line with the suggestions. Details of the revisions made are inserted in the reviewers’ comments and highlighted in red. The questionnaire has been included.

Thank you

Patrick Okonta
Reviewer's report
Title: Misconduct in research: A descriptive survey of attitudes, perceptions and associated factors in a developing country
Version: 2 Date: 25 September 2013
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.

RESPONSE
The questionnaire has been provided

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.
RESPONSE
The questionnaire provided would help in making this clearer. The thrust of this manuscript was on the researchers’ perception of the prevalence of scientific misconduct in their workplace. However, there was a part of the questionnaire included in this manuscript that asked about their awareness of acts of misconduct in their workplace (see questions 22 and 23). Data about self reported acts of misconduct (questions 43-50) had been reported in our earlier article. The words ‘perceive’ and ‘belief’ has been used interchangeably in the manuscript and questionnaire. In addition the following revisions have been made to the manuscript to make it clearer.

1. The last paragraph under the subheading ‘perception of frequency of occurrence of scientific misconduct in the workplace’ deals with researcher’s awareness of actual cases of misconduct. A new subheading – Researcher’s awareness of acts of scientific misconduct in their workplace- has been inserted (page 10).

2. The following sentences on page 10 have been revised
‘A further analysis showed that 88 researchers out of the 91 (96.7%) that personally admitted committing misconduct believed that scientific misconduct had occurred at their workplace, while 40 out of 42 researchers (95.2%) who had not committed any acts of scientific misconduct believed that scientific misconduct had occurred at their workplace. There was no statistically significant relationship between having committed misconduct and belief that misconduct occurred at the workplace, (Fischer exact p-value 0.65).’

Revised to
‘We further looked at whether the researcher’s perception of presence of misconduct at their workplace was associated with the researcher’s having committed acts of scientific misconduct (data on researcher’s self reported acts of scientific misconduct was presented in our earlier paper). The analysis showed that 88 researchers out of the 91 (96.7%) that personally admitted committing misconduct believed that scientific misconduct had occurred at their workplace, while 40 out of 42 researchers (95.2%) who had not committed any acts of scientific misconduct believed that scientific misconduct had occurred at their workplace. There was no statistically significant relationship between having committed misconduct and belief that misconduct occurred at the workplace, (Fischer exact p-value 0.65).’

3. The heading of table IV has been revised to – Association between perceived presence of scientific misconduct in the work place and work environment.

4. The definition of scientific misconduct used in this study was previously stated in the methods section. This broad definition was stated in the questionnaire.

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.

RESPONSE

1. As stated earlier, the manuscript is mainly about perception. Seeing the way the questions were asked in the questionnaire would make it a lot less confusion. In particular the first paragraph in the discussion refers to perception. This same data is presented in table II with the heading PERCEIVED OCCURRENCE OF VARIOUS ASPECTS OF SCIENTIFIC MISCONDUCT IN THE WORKPLACE.

2. Comparison was made with the study by Pryor et al because they used the same survey instrument as we did. In fact they actually developed the SMQ-R instrument. However, we modified the instrument by inserting questions on self-reporting of misconduct (see methods).

3. The statements about difficulty in comparing data has been revised to read
   ‘Fanelli, had remarked that the method of questionnaire delivery and in particular how the questions are asked could impact on the results from surveys on scientific misconduct and this must be considered when making comparisons of results from surveys.’ (page 12)

4. The layout of the discussion has been revised
   a. The limitation of the study has been presented immediately after the first paragraph as instructed.
   b. More comparisons with similar studies have been made eg study by Adeleye from Nigeria (page 14)
   c. Important differences have also been highlighted between studies
   d. However, we would prefer not to use subheadings in the discussion. We believe significant revision has been done to make the discussion comprehensible and free flowing without the use of subheadings

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.

RESPONSE

1. The conference organizers and the ethics committees that reviewed these protocols were assured that the name of the specialty would not be mentioned in any journal publication arising from the study. We would like to keep to that promise, more over we are of the opinion that mentioning the specialty would not have any added value to the manuscript.
2. The part of the SMQ-R presented in this report are indicated in lines 13 and 14 of page 6 in the Methods. It is also repeated in the first paragraph of the Result section on page 8

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.

RESPONSE

Some limited comparisons has been made (see page 14), however the article by Adeleye focused on self reported prevalence of misconduct while this manuscript focused on perceived prevalence. At the time we reported our data on self reported prevalence of misconduct, the Adeleye article had not been published.
Reviewer's report
Title: Misconduct in research: A descriptive survey of attitudes, perceptions and associated factors in a developing country
Version: 2 Date: 26 September 2013
Reviewer: BC Martinson

Reviewer's report:
1) My original concerns remain about the nature of the sample employed (a convenience sample with extremely high prevalence of self-reported misconduct themselves), and about the limitations of reports of having observed others’ misbehaviors. Yet, I acknowledge that the authors are not in a position to correct or address these matters in this manuscript. Setting those concerns aside, the authors have been quite responsive to the full range of reviewer concerns that were expressed in the initial review. As a result, the updated manuscript is stronger, and more balanced with more explicit recognition of the limitations of the study and data being employed.

RESPONSE
Thank you very much Sir

2) Additional pertinent literature review has been incorporated both in the introduction and discussion sections. I suggest the authors go a bit further in this regard, specifically adding some discussion of other studies that have reported on similar data using reports of the behavior of others as primary evidence. Work from a publication by Titus, Wells and Rhoades (Nature. 2008 June 19; 453(7198):980-2) presents an exemplary way of collecting such “report of others’ behaviors” and yielded figures against which to contrast the data in this manuscript. Much earlier work by Swazey, Seashore-Lewis and Anderson, which used a querying process more similar to that used in the present study could also be cited and contrasted with the current results. Specifically, consider the following publications:

RESPONSE
Thank you for suggesting some relevant publications. We have included the article by Swazey (see page13). Indeed there are a lot of publications from the developed countries on scientific misconduct, however comparisons is often difficult due to differences in methodology. One of the strong criticisms of some other reviewers of this manuscript is that certain comparisons made have been inappropriate.
3) In the first paragraph of the Background section, the authors enumerate the various mechanisms and structures that have been put in place in some developed countries to foster research integrity and minimize research misconduct. The introduction leaves the impression that these efforts are and have obviously been highly successful in attaining the desired outcomes in those countries, but this is misleading. We do not yet have good evidence for the efficacy or effectiveness of most such efforts. This should be explicitly noted in that paragraph.

RESPONSE

This has been revised and the sentence below added

‘However, the extent to which these interventions have been effective in reducing misconduct have not been documented.’