PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Authorship, plagiarism and conflict of interest: views and practices from low and middle income country health researchers
AUTHORS	Rohwer, Anke; Young, Taryn; Wager, Elizabeth; Garner, Paul

VERSION 1 – REVIEW

REVIEWER	Lex Bouter Department of Epidemiology and Biostatistics VU University Medical Centre Amsterdam The Netherlands
REVIEW RETURNED	16-Jul-2017

GENERAL COMMENTS	This is interesting and saddening – although not surprising - report on the views on reporting practices of corresponding authors of Cochrane reviews from low and middle income countries. The data were collected by a web-based survey and in-depth interviews and concern attitudes, self-reported behaviour and perceived behaviour by others. Major remarks 1. The title is insufficiently precise. This is not on the views of researchers from any discipline in general, but on the views of corresponding authors of Cochrane reviews from LMIC. And 'views on authorship, plagiarism and conflict of interest' may be substituted by 'views on health research reporting practices', thus making it more clear which discipline is at issue. 2. It is not made clear HOW the identity of participants is protected. Using a Google product does not exactly sound assuring. Furthermore, how do you know that the participants believed their identity to be well protected? Doubts about this might have influenced the decision to participate and the answers given by those who did. 3. Data are available on request from the first author. That is not best practice. Why not just add the anonymized dataset as supplement to the publication? And please also consider publication of the full results to the survey questions (not dichotomized) as supplement to the publication. 4. You need to explain better how strongly self-selected the interviewees were: a) they decided to participate, b) they indicated to be willing to be interviewed, and 3) they accepted the later invitation. In essence the response rate for the interviews is 2.8% (15/583). Please also discuss this in the discussion paragraph.

	5. Using cases or vignets has the advantage that it's quite clear to which situations the questions refer. But the downside is that the interpretation in a wider and more abstract sense is difficult to justify. Look for instance at the item on conflict of interest where in the results section it is assumed that this items deals with 'known conflicts of interest in the past'. Please discuss this as another study limitation. 6. I'm puzzled about the combination of what participants declared to have done themselves and what they perceived others to have done. These are two very different things that ought to be reported separately. Additionally, reported behaviour by others – even if true – is very difficult to interpret as different participants may refer to behaviour by the same colleague. 7. I missed in the discussion section the following relevant issues. a. Is norm subscription less good and research misbehaviour more frequent in LMIC than in the 'Western world'? b. Self-reported behaviour and perceived behaviour in others are not the 'real thing' (independent measurement of the occurrence of the behaviour itself). c. Socially desirable answers may have been given to the questions on norm subscription and those concerning research misbehavior of the participants. Minor remarks 1. The second bullet under strengths and limitations suggests that the choice for Cochrane reviewers '..may improve the reliability of the responses.' You probably mean validity as reliability is the technical term for precision or lack of random error. 2. The introduction should make clear that this is about biomedical research, or more exactly: about authors of systematic reviews of health care interventions. 3. You state in the methods section that you reminded non-responders twice, but elsewhere you explain that you were unable to do a non-response analysis because you didn't know who of the invitees responded. Possibly you sent all invitees two reminders. 4. You explain correctly that the convictions of the author team may influence the results and interpretation of the interviews. But you do not explain how well trained and how experienced AK is in this form of qualitative research. Please do so in the revised manuscript. 5. You might want to provide 95% confidence intervals for the frequency point estimates (table 3, supplementary file 4 and the frequency estimates mentioned in the text). That might provide readers with a better feeling for the precision of the data you present. 6. While reporting the results of the interview you describe them as coming from students, junior investigators and 'more senior people'. Please make it clear what you mean by these categories in terms of the characteristics listed in table 2. 7. Please explain how long it took on average to complete the web-based survey.

REVIEWER	Dr. Bernard Gallagher Reader in Social Work University of Huddersfield, England
REVIEW RETURNED	24-Jul-2017

GENERAL COMMENTS	This is a very good manuscript (M/S). To begin with, it is very well written and structured. In terms of substantive issues, the M/S covers a very important subject. There is little empirical work in general on research ethics, even though ethics should underpin research. There is even less such research in LMICs. On top of this, the authors have uncovered some very important - but also very troubling - data. It is not a total surprise but it is shocking, and one would hope this work would contribute to changes in policies and practices in this area. As I have said, this M/S is very well presented and could almost be published as it is. There is a need, though, for a few amendments. I have uploaded a document that lists some specific issues with the text. I also have a few more substantive issues to raise and these are as follows: 1. It would be good if the authors could say a bit more about how/why they chose the particular "scenarios" they utilised e.g. plagiarism. There is a mention of the selected scenarios being "common" ones but this sounds a bit ad hoc. There is not a major problem if this was the case but it would be worth clarifying and explaining whatever policy was used. 2. The survey asked separately about what a participant had done and what a participant knew that others had done. These are, of course, completely different situations and they should be reported upon as such. I thought this was not done sufficiently systematically in the M/S. 3. It is not altogether clear why this study focused on LMICs. One has to be very careful not to stigmatise these countries by implying that these problems are worse there. I think this focus needs to be more fully explained/justified 4. Similarly, there needs to be a more explicit comparison between practice in LMICS and HICs (in the Discussion). I could imagine that similar results would have been found in HICs. If the roughly comparative data does not exist, then this needs to be said. The authors refer to the "drivers" of ethical malpractice being similar in the two sets of countries but I think some reference needs to be made to the existence of this malpractice in both sets of countries or better still the levels of malpractice in both sets of countries. Again, one needs to be mindful of not stigmatising LMICs unfairly. The reviewer also provided a marked copy with additional comments. Please contact the publisher for full details.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

Major remarks Responses:

1. Thank you for the comment. We have worked on numerous iterations of the title. We think that "Authorship, plagiarism and conflict of interest" is more specific than "health research reporting practices", since the latter could also refer to general reporting guidelines.
Cochrane was simply a sampling frame: we have now added potential biases linked to the sample in the discussion. Our view is that this group would be more aware of these practices as they are discussed in Cochrane and thus the estimate may be more accurate.
We have adapted the title to: "Authorship, plagiarism and conflict of interest: reported views and practices from low and middle income country health researchers".

2. To clarify: We invited the participants via email and highlighted that responses were anonymous. Participants were not asked to report their name or the name of their institution. Those participants that indicated willingness to be interviewed were directed to another, totally separate survey to let us know their contact details.

3. We added detailed results of the survey responses as a supplementary file 4. We would prefer not to add the full dataset as a supplementary file, to maintain anonymity. Even though it is unlikely that participants could be identified, there are very few Cochrane authors in some countries.

4. This is reasonable. For a survey of this kind this is a fairly good response rate, but we have now commented in the methods and in the discussion about the potential biases related to respondents in the survey. We have clarified this in the methods and results section. All survey respondents who were willing to be contacted for interviews and provided contact details, were contacted via email. Of these, we were able to set up interviews for 15. Although this is a small, self-selected sample, we were staggered about how consistent the responses were. We have now commented on this in the discussion.

5. This point is reasonable. We have added a paragraph to the discussion. "We chose scenarios that included nuanced decisions but still had fairly clear correct answers and designed them to elicit responses that dichotomise these as right or wrong. However, we could not measure "overall" knowledge and behaviour in relation to all aspects of authorship practices, plagiarism and conflicts of interest, so the findings should be interpreted within the specific focus and examples of research reporting we examined."

6. Thank you for your comment. We were mostly interested in whether respondents were aware or irresponsible practices happening, but your point is well taken and we now report only on the number of respondents that admitted to misconducts themselves in table 3. Comprehensive responses have been added as supplementary file 4.

7. Thanks for these suggestions. We added data from studies conducted in high income countries to the discussion. We now also address interpretation of survey responses and social desirability bias in the discussion.

Minor remarks Responses:
1. Thanks for pointing this out. We have changed this bullet to: “Respondents were part of Cochrane which has strong ethical values and thus may improve the sensitivity of reporting practices.”

2. Thanks for the comment. We have amended the introduction to indicate that we are referring to health researchers. We asked Cochrane authors about researchers at their institution in general. Therefore, even though our sample were Cochrane authors, the findings relate to researchers in general.

3. Thanks for pointing this out. We edited the sentence to indicate that we “sent two reminders after the original invitation”.

4. Thanks for pointing this out. We have added the following sentence: “AR completed formal training in qualitative interview and data analysis methods and has some experience in doing qualitative research.”

5. Thanks for the comment. We added 95%CI to table 3, supplementary file 4 and the text.

6. We indicated that “The interview group comprised junior researchers (PhD students or those who had recently obtained their PhD; seven respondents) and senior researchers (professors who had supervised PhD students; eight respondents).” We did not want to report the characteristics according to table 1, as this was a small sample and we would like to maintain anonymity.

7. The survey took 15-20 minutes to complete. We added this to the section on data collection.

Reviewer 2
Responses:
1. We have revised the introduction and hope that it is now clearer why we chose the specific practices. We have also added a section to the discussion on why we chose specific scenarios (also see response to comment 5 of Reviewer 1)

2. Thanks for the comment. As per our response to reviewer 1, we now report on self-reported behaviour in table 3.

3. We have revised the introduction and hope the rationale for focusing on LMICs is clearer. The aim was to understand how researchers in LMICs perceive and experience research integrity/misconduct as there are very few empirical studies on this topic from LMICs. In addition, researchers from LMICs face the same pressures as those from other countries, but structures, processes and policies to prevent misconduct and promote integrity are lacking.

4. Thanks. We have added data to the discussion section, comparing our results to those of high income countries.

VERSION 2 – REVIEW

REVIEWER	Lex Bouter Department of Epidemiology and Biostatistics VU University Medical Centre Amsterdam The Netherlands
REVIEW RETURNED	04-Sep-2017

GENERAL COMMENTS	The revision adequately took into account the comments and suggestions made by the reviewers. The manuscript is now more balanced in the sense that the study limitations and the context of the study are more clear. There is one issue that attracted my attention when reading the revised manuscript that the authors may wish to remedy. In the abstract, introduction and methods the concept of 'redundant publication' is introduced. But it's not explained and not mentioned in the results and discussion sections. In table 3 and the supplementary files it becomes clear that it concerns 'text-recycling (using one's own work from a previous publication in another - presumably without proper reference). That's rather different from what I would have expected redundant publication to mean. My associations were republication (without reference to the original version) of spreading results over more articles that needed ('salami technique'). What redundant publication turns out to mean is 'self-plagiarism' (recycling your own words without proper reference). You might want to clarify this issue to your readers and relabel the phenomenon as one of the forms plagiarism can take. Maybe the discussion of the concept and the severity of self-plagiarism in 2014 memorandum on correct citation practiced by the Netherlands Royal Academy of Arts and Sciences can be helpful (https://www.knaw.nl/en/news/publications/correct-citation-practice).

REVIEWER	Dr. Bernard Gallagher University of Huddersfield, England
REVIEW RETURNED	12-Sep-2017

GENERAL COMMENTS	This is a very important, interesting and well presented manuscript, which should now be published.

VERSION 2 – AUTHOR RESPONSE

Comment: Redundant publication

Response: We agree that getting the terminology correct around plagiarism, redundant publication, and self-citation is important, as you point out in the comment below.

Comment:
There is one issue that attracted my attention when reading the revised manuscript that the authors may wish to remedy. In the abstract, introduction and methods the concept of 'redundant publication' is introduced. But it's not explained and not mentioned in the results and discussion sections. In table 3 and the supplementary files it becomes clear that it concerns 'text-recycling (using one's own work from a previous publication in another - presumably without proper reference). That's rather different from what I would have expected redundant publication to mean. My associations were republication (without reference to the original version) of spreading results over more articles that needed ('salami technique'). What redundant publication turns out to mean is 'self-plagiarism' (recycling your own words without proper reference). You might want to clarify this issue to your readers and relabel the phenomenon as one of the forms plagiarism can take. Maybe the discussion of the concept and the severity of self-plagiarism in 2014 memorandum on correct citation practiced by the Netherlands Royal Academy of Arts and Sciences can be helpful (https://www.knaw.nl/en/news/publications/correct-citation-practice).

Response:
We have addressed and clarified this by adding a table with definition of terms (new table 1) and have added a paragraph on the results of the scenario related to redundant publication in the results section. However, we would prefer to stick to the terms redundant publication and text-recycling, which are also preferred by the Committee on Publication Ethics (COPE). Redundant publication is an umbrella term and refers to: Republishing one's own work including copying of an entire manuscript (duplicate publication), publication of parts of the results in separate papers (salami publication) and re-using of text in several publications (text-recycling) (Table 1). Plagiarism, derives from a Greek word meaning to steal or kidnap, and while you can steal from others, you logically cannot do so from yourself, which is why the term self-plagiarism can become confusing.

Comment: Confidence intervals

Response:
The first reviewer, as a minor comment, suggested we add 95%CI to our results so that the readers would have a sense of the precision of our results. Consequently, we calculated and added 95%CI in our revised text. With further reflection, we actually do not believe this is helpful, and have confirmed this after consulting with our biostatistician and have removed them.
The reason is this: 95%CIs are only appropriate where probability sampling, such as simple random sampling has been used. We invited all corresponding authors of Cochrane reviews, living in LMICs to participate in the survey. Our sample can therefore be seen as a response rate sample of the entire population. It is likely to be quite biased and we cannot be 95% confident about the estimate because our participants were not a random subset of the population.

We hope you agree to this approach.