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
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Adrian Aldcroft
Executive Editor
BMC series journals
BioMed Central

Sir,

COVER LETTER

Thank you for your review of our manuscripts. The manuscript has been extensively revised in line with suggestions from the reviewers. Below are responses and details of the changes made according to the recommendation of each reviewer.

Thank you

Patrick Okonta
RESPONSE TO REVIEWER **ELIZABETH WAGER**

Thank you for your constructive review, our response is as follows

1. Statistical query
   I am not an expert in statistics, but I thought the Fisher exact test was only suitable for very small samples (<5) so I was surprised to see that it was used for this study (N=133). Also, it is used for categorical data (usually 2x2 contingency tables) and I was not sure that data from Likert scales should be treated in this way. I suggest you get this point clarified by a statistician.

   **RESPONSE**
   We agree that the Fischer’s test is used for categorical data and not likert scale. In our analysis, we dichotomized the likert scale response into two, thereby converting it to a categorical data and thereafter performed the analysis. The Fischer’s exact test is used when any of the data in the cells is < 5, rather than the total number of respondents.

   Minor corrections
   2. p3, line 4, the usual phrase is ‘responsible’ (rather than credible) conduct of research

   **RESPONSE**
   The word ‘credible’ has been replaced with ‘responsible’.

   3. p3, line 6, the correct title of the ORI is the Office OF (not for) Research Integrity

   **RESPONSE**
   The word ‘of’ replaced with ‘for’

   Minor revisions (not for publication)
   4. On two occasions (p6) the term ‘majority’ is used without an article (‘Majority of the researchers’ or ‘Majority … disagreed’) I suggest that standard English would be to use a definite article (ie ‘The majority of researchers’, or ‘The majority disagreed’)

   **RESPONSE**
   The article ‘The’ has been inserted before the word ‘majority’.

   5. p7, 1st line under title, should be ‘had occurred’

   **RESPONSE**
   The word ‘had’ has been inserted before ‘occurred’.
RESPONSE TO REVIEWER  DANIELE FANELLI

Thank you for your constructive review, our response is as follows

1. Is the question posed by the authors well defined?
Yes. Although, as a minor observation, I think that the vision that the start of the Introduction conveys about developed countries is far too optimistic. I would say that these "are starting to" ... or "attempting to" protect the credibility of research. Moreover, different countries have different structures and initiatives in place. For most of these, it is still unclear whether they are anything other than formal, and whether they have any beneficial effects at all.

Response:

Your comment is quite appreciated and there is no reason to believe that developed countries have perfected measures to eliminate research misconduct. However, looking at it from our situation from a developing world with no structures in place, we are very impressed with what the developing countries have done so far. We have however modified our original statement to reflect the fact that interventions in the developed countries are ‘attempting’ to prevent misconduct.

2. Are the methods appropriate and well described?
No. The authors refer to a companion paper, which reported another part of the same survey. I could suppose that many details might have been provided in the cited study, but I would repeat them in this study, too, for the benefits of readers. Examples of important details would include: the title and/or characteristic (topic) of the conference, details on how the questionnaire was handed out, and retrieved (drop box?, taken back by hand?), etc.

Response:

We have extensively revised the method giving more details including how the questionnaires were collected after completion. More details have been given about the SMQ-R questionnaire, its adaptation and contents. However, we have refrained from giving more information about the characteristics of the conference and the participants as this may identify the particular group of researchers who participated in the research and conference. There is a potential for stigmatization as a result of the findings of the study and we would like to protect the research participants from that risk. This reasoning has also been stated in the methods (last paragraph, page 6)

3. Are the data sound?
Yes (?) I have no reason to doubt that the data presented is genuine, although I have noted some peculiarities, which suggest that the sample and results of this study are rather unusual, if compared to other studies.
For example, Table II shows that plagiarism and data falsification are perceived to be more common than the falsification of a resume, falsification of a reference list, or disagreements about authorship. This is in stark contrast with the majority of surveys done to date, which generally show such questionable research practices to be more common than FFP (and it would be logical to believe so). I don't doubt the data itself, but this would suggest that an important variable is skewing the results. It could be the particular population at this conference, or it could be something in the procedure, including the fact that these respondents had already been "stimulated" with questions about misconduct not reported here. As suggested above, it is crucial that authors provide as many details on the methods as possible.

Response

We have including more details in our methods but as stated earlier refrained from providing identifiably information about the conference and attendees. The findings of a different pattern of misconduct distinct from what has been documented in literatures from developed countries although surprising, lends credence to the relevance of the study. We cannot assume that findings from the developed countries can be extrapolated to other regions of the world. This notwithstanding, it is hoped that our study will stimulate more research on this subject from developing countries. Thereafter, perhaps our findings may be further validated.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
I am not aware of specific standards of data deposition, but the presentation of data seems sufficient for this kind of study.

No response needed.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
No. In the first paragraph of the discussion, the authors seem to acknowledge somewhat the deviant nature of some of their results, but go little in the way of discussing these discrepancies. They cite one old survey, ignoring the many others that have been done.
I hate to have to suggest a citation to my own work, but all means and purposes such comparisons need it. The authors should start by comparing their results to the general patterns that emerged in my systematic review (which is quite relevant also for the methodological issues in interpreting these results, Fanelli D (2009) How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data. PLoS ONE 4(5): e5738. doi:10.1371/journal.pone.0005738) and perhaps include later surveys that were not included there.
The second paragraph in the discussion is rather unclear. It proposes as alternatives two sentences that seem to express the same idea: that misconduct is higher in this study because the country in question has no structure/activities
to prevent misconduct.
A significant part of the discussion is dedicated to discussing solutions to the problem of misconduct in their country. I find this rather unnecessary, and beyond the scope of the study. This is an empirical study, and the discussion should be dedicated to evaluating the validity of the evidence and its implications. What the authors believe to be the best solutions might be mentioned in one paragraph, but should eventually be elaborated in a separate paper.

Response

1. The paragraph has been expanded to include the systematic review by Fanelli and a very recent 2013 study.
2. The paragraph has been rephrased and made clearer. The latter part of the 2 paragraph comments on the possible implication of a perceived high prevalence of misconduct.
3. We have expanded our discussions of the results and agree that a very significant portion of our discussion should be on our results and the implications. Only in 2 paragraphs out of 10 paragraphs in the discussion did we make reference to possible interventions to reduce misconduct. We believe these 2 paragraphs complements and add some value to our discussion and would like to retain it.

6. Are limitations of the work clearly stated?
No. As stated above, much of the discussion is spent on proposing solutions, yet only one short paragraph is dedicated to explicitly discussing the study’s limitations.
And the (few) limitations mentioned are far from negligible. In particular, the authors point out that their definition of misconduct is broader than that of ORI. This is a crucial limitation, and the obvious first explanation for the discrepancies observed between this survey and others. The aforementioned meta-analysis provided direct evidence that the particular wording of questions, how generically the behaviours are described, and other methodological factors make a huge difference on the outcome.
A very important point, largely overlooked by the authors, concerns the results presented in Table IV, which show no association between prevalence and environment. The authors have no clear explanation for this strange result, even though it follows quite logically from the fact that they had a broad definition. As shown in the table, almost all cases fall into the “Present” column, which, given the very broad definition of “scientific misconduct”, is unsurprising. With such huge imbalance of cases, statistical associations between variables would be almost impossible to find in the first place.

Response
Identified limitations to our study has been increased from 2 to 5 and the implications of these limitations to our study has been clearly highlighted. Particularly highlighted is the limitation of our broader definition of scientific misconduct. Also the limitation of the ability of our study to show any associations has also been clearly stated.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
No. As mentioned above, they make little mention of previous work, both in laying down the background to their work and, perhaps even more crucially, in discussing the differences and limitations of their work.

Response:

The background section has been extensively revised and expanded from one page to 2 pages. Works of other authors have been sited, particularly the seminal works of D Fanelli. Two very recent publications (Onah, and Adeleye) have been incorporated into the background.

Again in discussing our results we have highlighted the limitations of comparing data on scientific misconduct as earlier identified by Fanelli. The work of Cohen in explaining the relationship between deviant peers and development of deviant behaviours has been cited. Furthermore, a comparison of our study result with that of a 2013 study by Hofmann in Norway has been included.

8. Do the title and abstract accurately convey what has been found?
Yes, although the conclusion section of the Abstract could perhaps avoid jumping to statements about what needs to be done, unless such statements clearly follow from the survey. And it is not obvious that they do.

Response:

We would like to retain the part of our conclusion that made a statement on what needs to be done although we realize it does not flow directly and explicitly from the results of our study. However, we have rephrased it to clearly show that it is a recommendation.

9. Is the writing acceptable?
No, not entirely. The paper reads relatively well, but there are occasional linguistic flaws. For example, in the abstract, “Over 88% of researchers were concerned about the perceived amount of misconduct prevalent”; or “Majority (84.8%)” instead of “The majority” on page 6; or “any statistical significant relationship” instead of “statistically significant” on page 8; etc. The lack of clarity noted above in at least one passage might also be due to language, so I suggest the authors get a mother tongue colleague to review the final text.
Response
The noted errors have been corrected and the final revised version was copyedited
RESPONSE TO REVIEWER  BC MARTINSON

Thank you for your constructive review, our response is as follows

2) While it is true that the SMQ-R tool has been validated, this was done in a sample of research coordinators in the U.S. And while it is one of the only such measures available, its suitability for use in other samples of researchers and in other countries and cultures remains unknown. Was the original translated from English for the study? While limitations of the SMQ-R are not directly the responsibility of the current authors, I still need to take issue with a couple of aspects of the SMQ-R that likely limit its value for their purposes. First, it’s concerning that the SMQ-R asks for responses to many of its question items in terms of the frequency of, for instance, observed misconduct. This seems a less than ideal metric for assessing misconduct, which in most discussions of the topic historically has been presumed to be a fairly rare event, and one that is typically hidden. Due to this somewhat odd framing of the metric, no time-frame is specified for the observation period. So those with longer exposures to either observe or to have engaged in undesirable behavior would presumably report higher levels, just due to differential exposure time. In some cases, the individual question items included are also problematic. In particular, one item highlighted in this manuscript asked respondents to indicate their level of agreement with the statement, “Dishonesty and misrepresentation of data is common in society and doesn’t really hurt any body.” This statement has three quite distinct components; 1) dishonesty, which is distinct from and much broader than 2) misrepresentation of data, both of which are distinct from the issue of 3) whether either of these is really harmful. That’s a triple-barreled question, making it impossible to know which part of the statement respondents are agreeing or disagreeing with. It’s a poorly framed question item that likely should never have found its way into the SMQ.

Response

1. The original SMQ-R was not translated from English. English is the lingua franca in Nigeria.

2. Limitations of the SMQ-R questionnaire: We agree with your observations on the limitations of the questionnaire especially on the absence of time frame for the information requested. However we note that in a certain part of the questionnaire we asked respondents if they had been aware of any investigator that had committed misconduct in the last five years. The result of that question is reported on the second paragraph, page 10.
3) I am also somewhat skeptical of the reality behind such reports of observing or perceiving undesirable behavior on the part of others in one's environment. There are well-established psychological principles that lead us to believe that most people will generally see the behaviors of “others” in a less flattering light than they will view their own behavior. So we naturally expect reports of others’ behaviors to suggest higher levels of wrongdoing or undesirable behavior. The problem is, we don’t really know how much of what is being reported is based in reality versus that which is based on these psychological biases. So interpreting responses to such questions must always be done with a great deal of caution and skepticism.

Response

We also agree with the reviewer that perception may differ from reality. We made reference to this fact in paragraph 2, page 13 of our discussion in which we made reference to our earlier report of a self-reported prevalence of committing misconduct of 69%. Furthermore we have expanded this paragraph to reflect clearly this opinion.

4) I have a larger concern, however, about the sample of respondents themselves. The 2012 results published by these authors from this same sample suggest that these respondents may have an extremely high level of self-reported undesirable research-related behavior. Their 2012 article suggests that more than two-thirds of these respondents self-reported having engaged in one of eight types of undesirable research-related behavior about which they were queried (although, again, we don’t really know over what time-frame nor how frequently they may have done so), and more than 40% self-reported having engaged in misconduct that meets the U.S. federal definition promulgated by ORI. These are far higher prevalence estimates than others have observed and reported, and it’s not clear to what extent this is a function of how the respondents were asked to report about their own behaviors, and to what extent it’s a function of the respondent sample itself. For the sake of argument, however, let’s say that misconduct is truly this rampant among the researchers in this sample.

Response

The authors were equally surprised by this finding. We have no reason to doubt the validity of this figure. However, subsequent future studies would either validate it or refute it.

5) The authors note in their discussion “it might be argued that individuals might have a tendency to lower their moral threshold for committing an offence if it is perceived that everybody else is committing similar offences.” Indeed, there is a
line of theory in criminology that predicts just this sort of outcome, with exposure to “deviant peers” and the development of deviant sub-cultures as being pre-conditions of deviant behavior on the part of an individual (see, e.g. Ben-Yehuda, N., 1986. DEVIANCE IN SCIENCE Towards the Criminology of Science. British Journal of Criminology, 26(1), pp.1–27. Also - Cloward, R.A. & Ohlin, L.E., 1960. Delinquency and opportunity, New York: Free Press. Also Cohen, A.K., 1965. The sociology of the deviant act: Anomie theory and beyond. American Sociological Review, 30, pp.5–14.) This raises, however, some tricky questions about the direction of causality in the observed descriptive data. Are the reported perceptions of the prevalence of undesirable behavior due to the fact that it is, in fact, so prevalent in the population? Or is it because such a high proportion of the respondents may already be participants in a deviant sub-culture in which such behavior IS in fact more common, but may NOT be outside of that sub-culture? Difficult to know with cross-sectional data of the sort available..

Response

We very much appreciate your exposition on the link between exposure to deviant peers and development of deviant behavior, this has been included in the discussion.(first paragraph, page 13)

6) Currently, the descriptive results do not distinguish the perceptions of those who also reported engaging in undesirable behavior themselves from those who did not. I think that, at minimum, for the descriptive data to be at all useful here, the authors must stratify their analyses such that one can distinguish the perceptions of those who self-reported having engaged in misconduct from those who did not. It’s not perfect, but it would at least give a chance of partially sorting the extent to which the reported perceptions are being colored by one’s own participation in a deviant sub-culture.

Response

A further analysis has been done to determine if there is any relationship between committing misconduct and perception of the prevalence of misconduct. There was no statistical significance (p value 0.65). See last paragraph page 9 continued on page 10.

7) Finally – since these analyses are predominantly descriptive anyway, why not present the results in Table 1 using the full original metric of the Likert items?
Response

Table 1 as presented in the manuscript is actually as in the questionnaire. It is a 3 response option: Agree/Disagree/Don’t know
RESPONSE TO REVIEWER  ZUBIN MASTERS

Thank you for your very constructive review

We respond as follows

1) Background section: As this paper will be open access and I suspect it will be highly accessed especially by researchers in developing countries, it would be good if the authors include a bit more information about research misbehaviors/misconduct and the Nigerian governance system of research integrity in the Background section. The authors quickly jump to the problem statement of their research study and why it is important to understand what is going on and what are the perceptions of researchers in developing nations. I will use the term “research misconduct” broadly here as the authors have defined it and not restrict it to falsification, fabrication and plagiarism. I provide two suggestions where the Background section can be expanded. First, could the authors discuss some of the data we have on developing countries as it pertains to the prevalence of misconduct? The authors briefly discuss one main result from their Developing World Bioethics (DWB) paper, but perhaps more can be discussed from this study e.g., 68.9% of Nigerian researchers admitted to having committed which of the 8 listed forms of misconduct. In addition, a recent article published by Ana et al. titled “Research Misconduct in Low and Middle Income Countries” in PLOS Medicine may be a good place to add some prevalence data from low and middle income countries. Second, if the authors could briefly describe the research integrity governance system in Nigeria (or the lack thereof) and situate how this fairs with developed nations i.e., the ORI or other systems in developed nations, this might provide the reader with a better understanding of how research integrity is governed in Nigeria. I think a few brief sentences should provide a greater understanding of the prevalence of research misconduct in developing countries and the system of governance of research integrity in Nigeria compared to other developed nations.

Response

1. The background has been extensively revised and has increased from 1 page to 2 pages.
2. We have included more background information on Nigerian governance system of research and lack of a governance system for research misconduct.
3. We have included 2 very recent studies from developing countries (Ana et al, and Adeleye) to give more background information
4. The 8 forms of misconduct have been listed in paragraph 3, page 3.
2) Background section: The paper is a continuation of the research performed based on the SMQ-R-based survey first published in the author’s DVB paper. In the last paragraph of the Background section, the authors state that the current study is the “concluding part of a larger study [5]”. Stating that the current study is a concluding part of a larger study does not clearly explain what parts of the SMQ-R-based survey were covered in the DWB paper and what is covered in the current paper. I think it would be clearer to the reader if in this last paragraph of the Background section the authors could clearly state the method used (SMQ-R survey) and what aspects (areas) of the survey were covered in the DWB paper and what will be covered here.

Response

The last paragraph of the background has been modified as suggested. What was reported in the first study has been mentioned and what is to be reported in the concluding part has been mentioned. (page 4)

3) Methods section: Several parts of the methods need to be made clearer. A rationale for using the SMQ-R-based should be explained. The authors explain that their survey was adapted from the SMQ-R. What modifications were made and why? Lastly, if possible, the authors should place a copy of their survey as supplemental material. This last part is essential if anyone wishes to employ this instrument to survey other nations or to reproduce this work at a later date.

Response

1. The details of the adaptation has been included in the first paragraph, page 6
2. A copy of the study questionnaire would be sent to the editor if it is requested.

4) Methods section: The authors should indicate the number of questions per area that were in the survey.

Response

This has been done (page 6)

5) Methods section: The authors explain in their DWB paper that performing a survey at the 2010 conference was a convenience sample. Yet other than in the Abstract, there is no mention of how the survey was disseminated. Moreover, what is the title of the 2010 conference and why was this conference chosen as a convenience sample should be stated in the Methods. Lastly, I presume the authors did receive permission from the conference organizers, as such I would explain this in the Methods.
Response

1. We have refrained from giving details about the conference as we wish to avoid the potential of possible stigmatization of the group of researchers if identified. The possibility of stigmatization is real in a study like this which is the first of its kind in the region. This explanation has been incorporated in the methods (paragraph 2, page 6).

2. Details of how the questionnaires were administered and collected have been included (paragraph 2, page 6).

3. The conference was chosen because one of the researchers belonged to that association; however we wish not to include it for reasons already stated above.

4. Permission was obtained from the conference organizers and it has been so stated (first paragraph, page 7).

6) Methods section: There are inconsistencies in the order and naming of descriptions in the Methods and the subsequent Results. (i) The authors explain that the questionnaire elicited information in four areas. But the order of the topics 1 through 4 is not in the same order in the Results section. For example, the fourth subsection on ethical climate of the work environment in the Results is listed as #3 in the Methods. (ii) The description in the Methods section should ideally have the same (or very similar) titles of the subsections in the Results section. (iii) Somewhere in the Methods section, the authors should discern what sections of the modified SMQ-R were covered in the DWB paper and what was covered in the current manuscript.

Response

1. The order in which the group of questions appeared in the questionnaire has been stated in the methodology.

2. The part of the questionnaire result reported in the DWB article and what will be reported in this manuscript has been mentioned in the last paragraph of the background and also in the first paragraph of the result (page 8). We are of the opinion that it should not be mentioned again in the methods.

3. We agree that ideally the reporting of the result should follow the same sequence as that in the questionnaire. However, we decided to alter the sequence in our reporting of results because we felt it will provide a better flow of thought and understanding. Remember we are using an already existing questionnaire.

7) Results section: I have two suggestions: (i) If the authors could move the second paragraph making it the first paragraph of the Results section, this will clearly explain to readers what results are being presented in the current manuscript and what was presented in the DWB paper. (ii) The first paragraph of the results explaining the demographics has been previously published in DWB and reiterated here. I agree with the authors that it is necessary to reiterate the demographic information in the current manuscript. In order to be transparent, my suggestion is for the authors to state clearly that the demographic and respondent information was previously obtained and published in DWB and is...
presented again here for ease for the reader. I think this is necessary to explain because the authors don’t want to make it appear as if these are original results presented for the first time.

Response

1. The second paragraph has been moved to the first paragraph as suggested.
2. The statement indicating that the demographics of the respondents earlier presented in the DWB article, are being represented here has been included in the last statement of paragraph 1, page 8.

8) Results section: I am not a statistical expert, but was there no statistical analysis performed for the nominal data showing differences in what participants reported? From my understanding, some form of statistics i.e., a Chi Square analysis would be appropriate to determine whether the observed counts fits expected ratios. If the Fisher exact test is meant to do the job I am explaining, why are the Fisher scores and p values not reported throughout the results and only the frequencies are reported. Does this mean other than what is reported in Table IV everything is statistically significant. If so my suggestion is to report these values alongside reporting the frequency data.

Response

This is simply a descriptive study with little intention to make inferential statements; as such very little statistical analysis is required. In the few areas where possible associations of findings have been explored, a Fischer exact test has been employed.

9) Results section: I would try and be consistent in explaining data throughout the Results section. (i) Within the text of the Results section, the authors began by reporting the number of participants followed by the percentage. Yet throughout the Results section, there are times when only the percentages are written, but the number of participants is not written. I would maintain consistency when reporting the frequency data to the best the authors can do. (ii) If the authors are explaining percentages with one decimal value, I would also keep this consistent throughout the text and tables. (iii) The language used in the text and in the tables is inconsistent. E.g., in the subsection “Attitudes and beliefs about scientific misconduct,” the authors explain the first result where researchers disagreed with the statement “dishonesty and misrepresentation of data are common in society and do not really hurt anybody” whereas in Table I this is written as “Dishonesty and misrepresentation of data is common in society and doesn’t really hurt anybody”. Just tiny word changes like “is” to “are” and a space between “anybody” should be made consistent between what is reported in the text and in Tables. (iv) Table I doesn’t have an explanation of the * and the column is labeled “Total no of resp.” whereas Tables II and III have the column
labeled “*No of resp. to question”. Other column name differences exist in the Non-Respondent column. Again, I would make these titles consistent between columns in the tables where appropriate.

Response

1. The relevant consistent checks have been made
2. The labelling of the columns of the tables have been made consistent
3. The legend for all tables are the same
4. The figures are reported to one decimal point, (except with the statistical analysis with 2 decimal places), however the figure .0 is omitted where it is a whole number.
5. In reporting the results we have indicated figures and corresponding percentage where we deemed necessary, however in some cases just expressing the percentage covers a clear meaning and provides better ease of reading. Moreover, the actual figures and their corresponding percentage is reported in the tables for readers who may want to know the actual figures

10) Results section: In the last paragraph of the Results for Table IV, it would be helpful if the authors could include a more substantive explanation of the results in the text despite the lack of statistical significance.

Response

We have included explanation and interpretation of that result in our discussion.(paragraph 3 page 14) We believe the statement as it is and the corresponding table provides sufficient information without having to make further repetition.

11) Results section: First sentence, first paragraph, it would be helpful if the authors could write “completed the survey” in “(133 out of 15 researchers attending the conference completed the survey)”.

Response

This has been included.

12) Discussion section: First sentence, first paragraph, the word “his” should be “this” to read “The most pertinent finding from this study…”

Response

Corrected

13) Discussion section: In the last paragraph of the Discussion on the limitations of the study, I would like the authors to clarify their first limitation. The authors explain that their definition of scientific misconduct is wider than FFP used in US
regulations. The definition of scientific misconduct as stated in the Methods section is “The non-adherence to rules, regulations, guidelines, and commonly accepted professional codes or norms”. This definition I think is so broad, it can capture anything. E.g., even verbal or sexual harassment can be captured which could be part and parcel of scientific misconduct, but it has more to do with professional misconduct than scientific misconduct. Because of this very broad definition, I think the authors might wish to explain that the high rate of prevalence of misconduct seen in their study, especially around falsification and plagiarism, might be partially explained by this very broad definition.

Response

This has been included in our limitation (Page 16)

14) Discussion section: Also in the limitations paragraph, can the authors briefly mention that there is a self-reporting bias as it relates to misconduct in the workplace. People are likely to point fingers at others, but underreport their own unethical acts. This too I believe can partially explain the high frequencies of misconduct reported in the study.

Response

In our earlier publication in which we documented the prevalence of self reported misconduct, we noted that there may be an underreporting of the actual prevalence. However, in this report that dwells on their perception of the prevalence, we are of the opinion that this may not be a factor and so no need to mention it.

15) A personal thing I like to do but of course it is up to the authors is to Acknowledge and thank the participants who took the time to complete the survey.

Response

We are particularly appreciative of this comment and have included it in our acknowledgement.
RESPONSE TO REVIEWER  SANDRA TITUS

Thank you for your constructive review, our response is as follows:

1. Is the question posed by the authors well defined?
They are reporting on some perceptions a convenience sample of researchers has about scientific misconduct in Nigeria. The question on perceptions of prevalence of research misconduct could be a useful question if it had been defined and studied appropriately. The paper is not appropriately designed to answer this question.

RESPONSE
We acknowledge the limitations in our study. We did not give a definition of ‘perception’, however, we believe that respondents answers to our research questions such as ‘How often do you believe the following occur in your work place: a) plagiarism b) falsifying date, etc; is a measure of their perception of the prevalence of scientific misconduct in their various work places.

2. Are the methods appropriate and well described?
The researchers have adapted a prior instrument to assess attitudes, work structure and views on the incidence of research misconduct. There are many instruments that have been developed and tested over time and have provided varying answers about prevalence. See Fanelli for a list of better defined studies. The authors need to examine the critiques that have been made about various instruments and what studies have data which are considered reliable and useful. Fanelli has done a meta analysis article in PLOS on research misconduct in which he only includes studies that have clear definitions of research misconduct. He has also done one that has examined questionable research practices in PLOS. These meta analysis list prior studies that show the questions and probes researchers have used that are considered to be reliable.

RESPONSE
We also acknowledge that there are various validated instruments on research misconduct. The SMQ-R which we have used is one of such instruments. We particularly recognize the work of Fanelli in this area. However considering that studies on research misconduct are virtually nonexistent in our country we decided to start with an exploratory and descriptive study. The SMQ-R instrument seemed appropriate for our study.

3. Are the data sound?
The descriptive data are sound however the solo chi square makes no sense to me. I have no idea how they think they could collapse down all of the scientific
misconduct measures and collapse all of the environmental issues as if all the components were the same. Maybe some correlations would be possible analyses for each item to demonstrate what was associated with work perceptions.

RESPONSE

The possible limitation of collapsing a likert scale response to dichotomous variable and subsequent conduct of a chi square analysis is well recognized. In our discussion we alluded to the possible effect this may have in blurring any association of misconduct with work environment.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
The design and analysis flaws are large. The best they could do would be to call it a Pilot study and critique their own flaws in conceptualizing it.

RESPONSE

We have acknowledged our limitations and consider our study as an exploratory and descriptive one. Definitely future research would build on it.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

6. Are limitations of the work clearly stated?
They are aware of the typical limitations of survey studies but they lack insight about their instruments deficits.

RESPONSE

We have stated the limitations of the study.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
The researchers have missed many other articles on research misconduct.

RESPONSE

It is important to note that studies on research misconduct from Nigeria or Africa were virtually nonexistent at the time of drafting this manuscript. However, a few articles have been published within the past 6 months and these have been incorporated in this revised manuscript. They include articles by Anah and another by Adeleye. Importantly, the work of Fanielli has been acknowledged and incorporated into the background and discussion sections of the manuscript.

8. Do the title and abstract accurately convey what has been found?

9. Is the writing acceptable?
I do not think the paper is strong enough to recommend a re-write. The design of
the questions limits what they can report. If they reported the descriptive findings it is a view of 130 Nigerian researchers in a convenience sample reporting vague perceptions that are poorly defined on various kinds of infractions in science and various kinds of work perceptions. While countries can have broader definitions of scientific misconduct, you cannot lump all of them together in a solo measure. Each item could be reported independently and associated with a perception of degree that their environment used punishment. A convenience sample has very limited value in determining overall prevalence in science. The sample alone would be very biased.

RESPONSE

We admit that the strength of this study is not on the robustness of the statistical analysis or the level of evidence it provides. Nonetheless, we believe that in the developing world where research misconduct has not been studied and remains a taboo to discuss, our simple descriptive study and data will not only be of interest to developing countries but also to developed countries. We believe it will stimulate further research and interest into this area. Future studies would undoubtedly build on this – a journey of a thousand miles starts with a small step.

Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being

Response

Language corrections have been made