Author’s response to reviews

Title: Self-reported data in environmental health studies: Mail vs. Web-based surveys

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Author’s response to reviews:

Dear Editor Jennifer Dykema,

We would like to thank you and the reviewers for a second revision of the manuscript “Self-reported data in environmental health studies: Mail vs. Web-based surveys”.

We are therefore submitting a new version of the manuscript where all the concerns pointed out by the reviewer have been addressed. We enclose to this letter a point-by-point notes text describing the amendments that have been made in the manuscript and where they can be found.

Furthermore, we would like to point out that the only figure presented in this manuscript (Figure 1), which displays a map with the study regions, was created by ourselves.

Best regards,

Manuella Lech Cantuaria, PhD
Prof. Victoria Blanes-Vidal, PhD

The Maersk Mc-Kinney Moller Institute, University of Southern Denmark, Campusvej 55, 5230, Odense M, Denmark
Dear Dr. Cantuaria,

Your manuscript "Self-reported data in environmental health studies: Mail vs. Web-based surveys" (BMRM-D-19-00288R1) has been assessed by our reviewers. Based on these reports, and my own assessment as Editor, I am pleased to inform you that it is potentially acceptable for publication in BMC Medical Research Methodology, once you have carried out some essential revisions suggested by our reviewers.

Their reports, together with any other comments, are below. Please also take a moment to check our website at https://www.editorialmanager.com/bmrm/ for any additional comments that were saved as attachments.

Once you have made the necessary corrections, please submit a revised manuscript online at:

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If you have forgotten your password, please use the 'Send Login Details' link on the login page at https://www.editorialmanager.com/bmrm/. For security reasons, your password will be reset.

We request that a point-by-point response letter accompanies your revised manuscript. This letter must provide a detailed response to each reviewer/editorial point raised, describing what amendments have been made to the manuscript text and where these can be found (e.g. Methods section, line 12, page 5). If you disagree with any comments raised, please provide a detailed rebuttal to help explain and justify your decision.

Please also ensure that your revised manuscript conforms to the journal style, which can be found at the Submission Guidelines on the journal homepage.
A decision will be made once we have received your revised manuscript, which we expect by 29 Nov 2019.

Please note that you will not be able to add, remove, or change the order of authors once the editor has accepted your manuscript for publication. Any proposed changes to the authorship must be requested during peer-review, and adhere to our criteria for authorship as outlined in BioMed Central's policies. To request a change in authorship, please download the 'Request for change in authorship form' which can be found here - http://www.biomedcentral.com/about/editorialpolicies#authorship. Please note that incomplete forms will be rejected. Your request will be taken into consideration by the editor, and you will be advised whether any changes will be permitted. Please be aware that we may investigate, or ask your institute to investigate, any unauthorized attempts to change authorship or discrepancies in authorship between the submitted and revised versions of your manuscript.

We look forward to receiving your revised manuscript and please do not hesitate to contact us if you have any questions.

Best wishes,

Jennifer Dykema, Ph.D.

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Reviewer reports:

Morgan M Millar, PhD (Reviewer 1): Thank you to the authors for the detailed and thorough responses to the reviewer and editor comments. The revised manuscript is improved considerably.

A couple of minor points:

1) Page 1, line 9: surveys are generally not a qualitative research method.
The authors agree with the reviewer and this was mostly a typing mistake. The work “qualitative” was now replaced by “quantitative”.

2) Page 3, line 12: typo of "last" should be "least"

The word was changed accordingly.

3) Page 4, line 16: the letters were addressed to the "householder". It would be helpful to specify if there were any instructions as to which adult household member should complete the questionnaire

In the informative letter, we also included simple instructions specifying that the answers should be given by a single participant and that she/he should be older than 18 years. This information was added in the manuscript (page 4, line 17-18).

4) Page 6, line 23: Why is sickness classified as a reason for ineligibility, rather than a refusal? It is rather unusual to exclude refusals from the denominator. Furthermore, the phrase "the person does not live in the address" does not make sense given the letters were not addressed to a specific person. Relatedly, in Table 1: it seems like the refusals should be included in the denominator. How many households were classified as ineligible and subtracted out of the denominator?

We are in total agreement with the reviewer and we thank you for pointing that out. In fact, the sentence should have been written in another way, as it does not reflect the real situation. In that case “sickness” refers to few cases of letters sent back to us, where a health care assistant has sent us the questionnaire back saying that the householder was unable to reply the survey due to mental sickness e.g. dementia. When we state that "the person does not live in the address", it is because, as all the addresses reached were in the countryside, a considerable amount of them are actually summer/vacation houses, and therefore, nobody leaves there on a daily basis. In that way, both cases were considered ineligible, as none of them have conditions to properly answer the questions provided. Therefore, we have now rewritten the sentence in a better way and it now reads as “Known ineligible cases (i.e. returned letters due to invalid address and refusals due to e.g. the fact that the house is not used on a daily basis (i.e. vacation house) and rare cases of mental sickness informed by a health care assistant)” (Page 6, line 24-25 and page 7, line 1).

The text of Table 1 was also rewritten accordingly. We also included there the number of households classified as ineligible cases (i.e. 75).
5) Page 15, line 1: isn't it more commonly found that older ages are more likely to respond than younger ages?

According to Green (1996), there is a belief that, as age increases, the response rate and quality of the responses decrease. However, she also cites a wide range of studies that have obtained different results in terms of the correlation between age and response rate, leading us to conclude that there is no previous hypothesis in relation to this relationship. Due to that, and in order to avoid misunderstanding, this sentence was rewritten. It now reads as: “There was no previous hypothesis in relation to this fact, as there is a general lack of consistency in the literature regarding the relationship between age and response rate [47].” (page X)

Green KE. Sociodemographic factors and mail survey response. Psychol Mark. 1996;13:171–84

6) It is mentioned that the estimated time to complete the questionnaire was 10-12 minutes, but in the discussion of limitations seems to suggest it was a long survey. 10-12 minutes seems rather short. Can the authors specify the number of items or page numbers, and perhaps include the questionnaire as a supplementary file?

The questionnaire contained 4 pages full of questions. Our validation procedure indicated 10-12 minutes for a fast respondent. It is true that it can take longer for respondents that prefer to read the questionnaire carefully. A non-original version of the questionnaire (i.e. being this one translated from Danish to English) was included as a supplementary material and it can be further judged whether it is relevant to have it as a supplementary material or not.

7) Were any incentives offered for participation? If so, this should be specified in the study design. If not, this may be added to the discussion of limitations regarding the low response rates in the discussion.

No incentive was offered for participation in the study. We have therefore included this as a limitation of the study (page 14, line 5-6).

8) Tables: The p-values associated with Tables 1 and 2 (as reported in the text) should be added to their respective tables

The p-values were added to the corresponding tables, as suggested.
9) Table 1: notes have typo of "diving" rather than "dividing"

The typo was corrected. We again would like to thank the reviewer for pointing out all these typos.

Jeanette Ziegenfuss (Reviewer 2):

1) The authors successfully addressed earlier comments. However, with more clarity in the objective of the manuscript and the methods, new issues emerge. Most importantly, the self-selection analysis, while more clearly articulated in earlier response to reviewer 1, does not achieve what is claimed by the authors. On page 5 the authors state that random selection into the sample agnostic of geography minimizes selection bias into the study. Is is unclear how this is the case. What you are describing in this paper is an observational study of mode selection at multiple contact attempts to a simultaneous mixed-mode survey. Differences in those that opt into paper or web is one of the outcomes of interest. The paper would be more clear if the attempt to deal with self-selection bias was omitted and instead self-selection into mode was noted as a potential partial explanation of measured differences of demographic and self-reported across modes.

The authors appreciate this comment and believe that, due to the terminology, this part of the manuscript should be improved. The first selection bias cited in the manuscript (page 5, line 7-9) refers to the selection of households invited to participate in the study. Therefore, the “selection bias” term refers only to the way in which the households were included in the study and do not relate to the differences in terms of the survey-mode choices. Differently to that (and more importantly), we have carried out a self-selection analysis to investigate whether the selected mode can be associated to the characteristics of the region where the person lives. These results are shown in Table 3. To avoid the confusion between these two terms, the sentence “This fact minimizes the selection bias in this study”, in page 15, was deleted from the manuscript.

2) The conclusion of the paper does not directly address the first research question.

We have now included a sentence in the conclusion section in relation to the first research question (page 15, line 15-18).