

Author's response to reviews

Title: Self-rated Health among Mayan Women participating in a Randomised Intervention Trial reducing Indoor Air Pollution in Guatemala

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Author's response to reviews: see over

Dear Editor:

Thank you very much for the valuable comments made by the reviewers on our paper Self-rated Health among Mayan Women participating in a Randomised Intervention Trial reducing Indoor Air Pollution in Guatemala.

We hereby resubmit the manuscript having made alterations based on the reviewer's queries. We hope the paper will be now suitable for publication on BMC. Our response to the reviewers' follows below.

REFEREE 1 (Surinder K Jindal)

The first reviewer writes: *“It is not clear to what factors the improvement in health status of 24.2% of control women who did not use Plancha can be attributed. This also raises the issue of assessment of other confounding variables responsible for change in health which might have been there. For example, the education provided by the interviewer/others who provided Plancha could have influenced the results in favour of Plancha”*

As the reviewer points out it is not obvious why women in the control group report an improvement in health. What we can say, given that we conducted a randomised controlled trial, is that the difference in improvement between intervention and control women is most likely due to the effect of the intervention. We can speculate that changes in ventilation practices, awareness of the smoke as a problem or desire to please the investigators can account for improvement in self-rated health in both intervention and control groups. But, as both groups received the same information, the differences in outcomes should be due to the intervention, which is the object of investigation in our article. The fact that the information given to both groups was the same is now better explained in the text: “Intervention women were carefully instructed in the use and maintenance of the stoves but did not receive any other health advice that differed from controls.”

REFEREE 2 (Sean Semple)

We thank the reviewer for his positive comments on the manuscript as a whole. The reviewer proposes the revision of two major points.

1. *“There is no discussion about the possible influence of pregnancy on self-reported health status.... There is no data presented to demonstrate the difference in the numbers pregnant in intervention/control groups at follow-up. The possible role of pregnancy on the improvement in health of both groups should be discussed. The Leinonen study cited on page 12 looks at changes in health over five years in an elderly population- this study was among young women in their late 20's- it would be useful to cite data on self-reported changes in health in women of a comparable age.”*

We recognise that this is an important point to be explained more thoroughly in the text. Accordingly, we have now included:

- a) The percentage of pregnant women in the intervention and control groups 18 months after the start of the study: “A high percentage (68.9%) of the women were pregnant at baseline, but only 12.8 percent (12.6% in the intervention group and 13.6% in the control group) were pregnant at the time ...”
- b) A discussion of the eventual importance of pregnancy in our study: “The percentage of women that were pregnant decreased substantially during the study period, from approximately 70 percent to 13 percent. Although it is known that women experience substantial changes in health status during and after pregnancy, it seems like self-rated health status exhibits smaller changes over the course of pregnancy than other types of health assessment.¹ Also, as the percentages of pregnant women in the control and intervention groups were balanced, this issue is not likely to alter our results although it should be taken in consideration when comparing our results with other populations.”
- c) A reference on self-reported changes in health in women of a comparable age: “Hass et al reported smaller changes in self-reported health status during the course of pregnancy than they did in physical function and vitality.”¹

2. *“While I agree that the plancha is likely to be the cause of the improved health it may be argued that it is the indirect effect of factors such as ‘increased social status’ and the intervention groups desire to ‘please the study team’. The advantage of this study is that it builds upon previous work and findings showing reduced exposure levels and so the weight of evidence begins to indicate that the health benefits are a real result of the plancha reducing personal exposures among these women. However I think the final sentence of para 2 on page 12 should be moderated accordingly to introduce the possibility of bias- discussed very clearly and well- at the beginning of page 13. The authors may wish to cite or examine the discussions (see HEDON website- find address) on the introduction of bias by participants who wish to provide the study team with the answers they believe they want.”*

As suggested in the HEDON website, we now discuss the possibility of bias introduced by fieldworker: “Thirdly, there were two different fieldworkers that interviewed the women. Although these fieldworkers were local women who shared the ethnic background with the participants, the possibility of a bias in the interpretation of the participants’ responses exists. However, the fieldworkers were randomly assigned to the intervention and control group, which reduces the likelihood of this kind of bias.”

Regarding the sentence “Since women were randomised to either intervention or control groups, this is most likely an effect of the Plancha” we still think, as the reviewer points out, that the most likely reason to explain the differences between intervention and controls in this randomised controlled study is the intervention. We agree that indirect effects of the intervention, as the social status, could eventually be the reason that explained differences. But the probability for this is considerably smaller as the social status is not likely to reduce the prevalence of symptoms like headache or sore eyes.

Thus, we maintain the sentence as it was, discussing the possibility of different bias in the text

The reviewer had some minor points that we address under:

1. *“Page 9- last para. “nearly all”- 84/98? Actually should this be 84 of 89- is there a typo at the top of table 4 where it states 98 women using the plancha. If it is 84/89 then I can agree with “nearly all”- I prefer percentages in the text rather than having to guess at what terms like “nearly” mean.”*

Thanks for this comment. There was a typographic error in table 4, which is now corrected. It is actually 89 women that used the Plancha. The reason why we do not give percentages is that these are open (unprompted) questions. This means that also women who do not answer a specific question might have said “yes” if we asked about the same issue using a close-ended question. For that reason it is not recommended to use percentages when reporting data coming from open questions.

2. *Page 9 –last para. “Unprompted”- what information was provided to participants in the recruitment process. Were they told that the plancha would reduce smoke levels or was likely to reduce smoke levels in their kitchen?*

See the comment above. As little information as possible was given to the participants regarding the *planchas*. As they were well known in the area, women had their own ideas about why they wanted to have one. The team was especially careful not to talk about smoke or respiratory or other symptoms to the participants after consent was given.

3. *Page 10 para 2 mentions that some women described reduced light from the fire. Did this lead them to use additional lanterns/lamps that perhaps emitted more particulate matter?*

Although do not have this information at the time of the self-rated health assessment, we know, from a questionnaire performed in May 2004 among the same women, that the main lighting source for both the intervention (70.8%) and the control (67%) group was electricity. At that time, only four women (4.5%) in the intervention group reported building a fire in the house or near it on a weekly basis for some other reason than cooking. We include a general sentence on lighting source in the results section now, as no important increase in the use of electricity happened from baseline to this assessment. The reason why we have not included more information in the paper is that it was neither assessed at baseline nor at 18 months, and we thought it could complicate the paper if we had to explain even more different health assessments.

4. *Page 10, para 3. “very few”- how many? The paper would benefit from a table of the results described in this paragraph.*

We have now written the numbers in the text. “Self-reported improvements in health included a reduction of eye discomfort (52 women), headache (15 women) and throat discomfort (9 women). Most (45) of the 48 women who stated that the smoke reduction had an effect on their children’s health, explained this in terms of reduced eye discomfort. By contrast, very few women mentioned a reduction in respiratory symptoms as the explanation for improved health, either for themselves (4 women) or for their children (1 women).”

5. Page 10, last line. (difference non significant)- give the $p=0.141$ and then at the start of the following page give $p<0.0001$.

The text is changed according to the reviewer’s suggestion.

6. Page 11- para 1. “was seen as far more important” - looking at the description of the questions in section C on page 7 it is difficult to see how this conclusion can be reached. The participants may have been reporting the symptoms they most associated with improved health but importance was not rated.

We thanks also for this comment. The text is now changed to “reported more often”.

REFEREE 3 (Brendon Barnes)

We want to thank the reviewer for his comments on the paper. The reviewer has several minor queries that we address under.

Abstract: page 2, methodology section, combine the two paragraphs or clearly separate them.

Abstract: page 2, results section, change (e.g. eye discomfort, headache) to (for example, discomfort, headache and so forth).

These sections are now unified and changed as the reviewer suggests.

Background: page 5, third line from the top, change ‘heath of the women’ to ‘health of the women’.

Methods: page 5, second paragraph, it should be made clear that the planchas were offered free of charge.

Changes according to the comments made.

Methods: the authors need to explain why there were two recruitment groups/waves?

The reviewer points a vital issue for the understanding of the whole RESPIRE study. This is being explained in depth in a previous article.² We have now explained it briefly in the text. “To avoid extending the geographical study area, participants were recruited over

two periods: the first between October and November 2002 and the second between April and May 2003.”

Methods: more contextual information is needed on baseline burning behaviors. Were open fires burned in a separate kitchen or room in the house? Do fires fulfill both a cooking and space heating function? Are children usually present when fires are burning?

See answer given to Reviewer 2. A sentence explaining that women usually carry their youngest child at their back while cooking for the family is included in the text. The percentage of houses that had a separate room for cooking is included in Table 1.

Methods: more detail is needed on the interview process. For example, how long were the interviews? Was anyone else present? Who conducted the interviews?

Unfortunately, we do not have records for the duration of the interviews. As explained in the text, the interviews were conducted in Mam, and administered by two bilingual (Spanish and Mam) fieldworkers, randomly assigned to intervention and control women. Nobody else from the study team was present. It is possible that some other family members were there, but we do not have records on that particular issue.

Methods: page 7, point B, did respondents give answers not covered by the pre-structured categories? If yes, how were these captured and managed?

For section B, based on previous experience during the study and on pilot work, a list of possible answers was developed, with space to record other responses. This is now better explained for section C in the sentence: "...and the fieldworker wrote down the symptoms mentioned in an open space with place for several answers."

Results: page 8, it would be useful to have the minimum and maximum age values as well as the mean and standard deviation.

Given in the text now: (range 15-44 years).

Table 1: change footnote reference from symbol to 'e'.

OK

Results: page 9 (reference to Table 3),
o A clearer explanation and/or a revision of the layout of Table 3 is needed. For example, does the table summarise the sample stratified by any, respiratory or other symptoms (with the same explanatory variables in each)? If the table is based on these three categories, it is misleading to claim that average/poor health ratings are related to increasing number of symptoms (page 9) as this is not evident in the table.

Thank you for this comment. We understand that this Table can be misleading. The results presented are not a result of stratification but of running the same analysis in the whole sample with different variables included in the model (either any symptom or respiratory symptom or other symptoms). Therefore we can claim that average/poor health ratings are related to increasing number of symptoms OR 1.35 (95%CI 1.02-1,78). We explain this more clearly in the Table now.

o What was the age cut-off used in the binary logistic regression? This is important to interpret the claim that average/poor health rating is associated with increased age.

Age was included as a continuous explanatory variable in the analysis. We specify this information in the text now

o Clarify why 'fieldworker' is included as a variable and not any others (for example, asset index).

See answer to Reviewer 2 regarding the reason why, although the probability is low, there still might be a reporter-bias.

o Change 'moths' to 'months'.

Table 4: change 'N' to 'n' in the table.

Discussion: page 12, third paragraph, replace "perceptions: Firstly" with "perceptions. Firstly".

Discussion: page 14, change 'should be due attention' to 'should be given due attention'.

Thanks.

Discussion: it would be very useful to outline recommendations for future studies.

In the last paragraph we write: "Thus, our results draw attention to the importance of educational programs run parallel to dissemination programs, to broaden the areas of perception of health benefits of improved stoves."

One of the more interesting findings is that planchas were associated with improved social status. Could the authors explain why and/or how this may operate?

We agree that this is an interesting issue that should be further studied, probably by using focus groups. Although we can speculate regarding this finding, we do not have data to explain it in this paper.

Given some of the disadvantages, did any of the participants attempt to adapt or modify the planchas in any way?

See answer given to Reviewer 2. We know that a few of the intervention women used open fires in addition to the plancha. But the RESPIRE study included weekly visits to the houses to make sure that the planchas were functioning adequately, so that it is unlikely that they were modified in any way.

We thank again the reviewers for their constructive comments.

1. Haas JS, Jackson RA, Fuentes-Afflick E, Stewart AL, Dean ML, Brawarsky P. Changes in the health Status of Women During and After Pregnancy. *J Gen Intern Med* 2004;20:45-51.
2. Díaz E, Bruce NG, Pope D, Lie RT, Díaz A, Arana B, et al. Lung function and symptoms among Indigenous Mayan women exposed to high levels of indoor air pollution. *International Journal for Tuberculosis and Lung Diseases* 2007;11(12):1372-1379.