Author's response to reviews

Title: A qualitative study on caretakers' perceived need of bed-nets after reduced malaria transmission in Zanzibar, Tanzania

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Author's response to reviews: see over
The editorial board
BMC Public Health

Dear Editors,

Please find enclosed a revised version of the manuscript entitled "A qualitative study on caretakers' perceived need of bed-nets after reduced malaria transmission in Zanzibar, Tanzania" co-authored by Netta Beer, Abdullah S Ali, Helena Eskilsson, Andreas Jansson, Faiza M Abdul-Kadir, Guida Rotllant-Estelrich, Ali K Abass, Fred Wabwire-Mangen, Anders Björkman and Karin Källander. Revisions were made according to the reviewer's comments, as described below:

Reviewer's report
General comments
The authors' revisions have improved the manuscript and facilitated a more constructive review - particularly through the clarification of the use of the HBM.

Major compulsory revisions
1. On the whole, the methods section is detailed but two areas require clarifications:
   a. Respondent selection. More detail regarding the previous household survey could be provided as this was used as a basis to select respondents: what was the sample size? How was the questionnaire administered? Also, was one caretaker selected from each household or were multiple caretakers interviewed at a household?

   Author response: We have added some information on the household survey as well as a reference to the article in the Methods section, under sampling and study participants (pg.8, first paragraph).

   b. Sample size. No reference is made to point of theoretical saturation as limiting the sample size, yet it seems that the interviews with male respondents were originally excluded for that reason. Nineteen individual interviews with male and female caretakers: is this a sufficient number of interviews to produce sufficient data to draw conclusions? The answer depends somewhat on the response to the previous question: were interviews conducted to the point of theoretical saturation? (i.e. no novel insights emerged from the interviews) Why therefore did you stop at 10 or 19. Please clarify.

   Author response: We have added a section on sample size considerations which includes the fact that we ensured that the interviews were conducted to the point of saturation in the Methods section, under sampling and study participants (pg.8, second paragraph).

2. Discussion – “women’s decision-making power”: The authors state that women "have high self-efficacy and decision-making power”. I would suggest that given the methods used, without observations (or other methods) to confirm or refute this (see also issue of triangulation in the previous review), the authors can only
state that women described having decision-making power (unless of course they have other data on which to base this statement)

Author response: This was corrected, and it is now clear that we are only reporting on perceived or described decision-making power and self-efficacy in the results section, under self-efficacy and cues to action (page 15), and in the discussion section (page 20, 2nd paragraph).

3. Disease categories (Results and Discussion). The discussion of respondents’ ideas about other causes for malaria highlights one key assumption that is made throughout the paper: that the respondents, translators and researchers shared the same understanding of malaria as a disease category. It would be useful to include more data on this: I assume that it came up during the interviews (whether the categories are one to one or not). It would also be beneficial to discuss this with reference to the significant literature on this topic in Africa (and Tanzania see e.g. Winch et al 1996).

Author response: This point is well taken. Since we see the issue of using the same terminology as a methodological issue, we added a section on this in the Methods section, under data collection and the proposed reference was added (page 10, second paragraph). The proposed reference is now used in the discussion section (page 17, second paragraph).

4. Limitations. The limitations of the HBM are stated, however, these limitations are inevitably tied into the study itself (see previous comment on decision making power) - the results are relate to perceptions and not behaviour. This should be more clearly articulated in the final paragraphs of the discussion.

Author response: We have now highlighted the fact that this study did not link caretaker's perceptions to actual use in the Discussion (page 21, last paragraph)

Minor compulsory revisions

1. Excessive use of “was done” remains: conducted, undertaken, carried out are far more formal terms.

Author response: We have replaced this term in several places.

2. I would consider rewriting the 5th para of the discussion re comments above on disease categories and to improve readability.

Author response: We have made some revisions in this paragraph; however we addressed the issue of disease categories in the methods section, as mentioned above.

3. A few typos: effective or efficacious (?) rather than efficient, 1st para background belief rather than believe 6th para, discussion; economic rather than economical, penultimate para, susceptibility and severity of malaria (findings); household
rather than house, 1st sentence, self-efficacy and cues to action (findings); write out numbers <ten

Author response: We have made these corrections.

Discretionary Revisions

1. Changes in LLIN usage. This is mentioned but did the data give any insight into why there were difference between the survey and the interview data?

Author response: This might be due to the fact that the survey was done several months after distribution and they were still using other bed-nets at that time, before starting to use the LLINs. However, since we did not ask or probe about this issue, we would rather not make these assumptions. This is now mentioned in the methods section under sampling and study participants (Page 9, first paragraph)

2. Reconsider the inclusion of the paragraphs on seasonality and figure in the results - the figure would seem far better placed in the discussion.

Author response: After many discussions we decided to leave the seasonality paragraph and figure in the results section.

We hope our revisions are satisfactory.

Additionally, the manuscript has undergone English review by a native English speaking colleague, Mrs. Witney Åhs, and language revisions were made.

Sincerely yours,
Netta Beer, PhD student