Author's response to reviews

Title: Exploring the first delay: a qualitative study of home deliveries in Makwanpur district Nepal

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Version: 4
Date: 29 January 2014

Author's response to reviews: see over
Dear Editors,

We would like to thank the reviewers for their comments and have responded to their comments in the report below.

Yours sincerely,

Joanna Morrison

Reviewer: Joanna Raven

Reviewer's report:
Major compulsory revisions
This is a well written and interesting study. However there are a number of points that need to be considered:

1. In the results section, the findings are triangulated with the findings from the “Quality of Care” study. This raises several issues: there are no details about how this study was conducted and importantly any limitations; it makes it difficult to follow the findings from the submitted paper; the authors compare perceptions of women with perceptions of different stakeholders from the “Quality of Care” study – it is not clear whose views you are presenting when you present the findings from the Quality of Care study. I suggest that these findings are removed from the results section and then referred to in the discussion section – and refer to the report or publication of the “Quality of Care” study.

We have removed references to the quality of care study in the results, and made reference to these findings in the discussion section as suggested by the reviewer.

2. Rationale for selecting women from control and intervention sites should be clearly stated. In the findings, the differences between these two groups should be highlighted and then discussed in the discussion section.

We have added our rationale for sampling women from intervention and control areas. We detail where we have compared responses in the results section below:

“The status of women is a cross-cutting theme in every barrier to institutional delivery, and affect women of all ethnicities, and in intervention and control clusters.” P9

“Women from intervention and control areas who delivered at home were generally aware that it could be better for their health and the health of their child to deliver in a health institution” p10

“All those reporting concerns about cost were of Tamang or Rai ethnicity, from all types of health facility catchment area, but mainly in control areas.” P12

“Half of the women who reported bad experiences with the health services spoke about Health Posts, mostly from participants in intervention areas.” P12
3. It would be good to have more details about the women who were interviewed including who conducted their delivery – were there any women who delivered at home with a SBA?

**We have added details regarding delivery attendance.**

4. Was the aim of the study to help develop the intervention and the monitoring tools for the trial? It is also not clear when the data collection was done and if and how the findings have fed into the intervention design.

**We have altered the introduction to clarify our aim to guide our process evaluation and inform the intervention development. We have also altered the discussion to clarify how this research has been used.**

**Minor essential revisions**

1. Another limitation of the study: although the views of women are vitally important in understanding why they deliver at home, it is also important to understand the views of the other decision makers in households, who may have more influence / power over the choice of place of delivery.

**We have added to the limitations section as advised by the reviewer.**

2. “Data were collected in Nepali by Nepalese researchers who had experience and training in research methods” (p6). Does this experience and training relate to qualitative research methods? How did the authors assure the quality of the data collection?

**We have added further information regarding training and experience of researchers, and steps taken to assure data quality.**

3. How was the topic guide developed?

**We have added how the topic guide was developed**

4. Page 9: “A few women said that it was easy to deliver at home”. What does this mean?

**We have changed ‘easy’ to ‘convenient’**

5. Page 16: “Barriers leading to delays in the home may change”. This is not clear.

**We have clarified the sentence on p16.**

6. Page 17: “Generally, there is little evidence regarding how quality improvement initiatives can improve outcomes [28].” This reference refers to the district demographic profile of Nepal. Other references may be more relevant here.

**We thank the reviewer for identifying a typing mistake, and have corrected the reference.**

7. Page 17: Sentence beginning “Women in our study reported...were less...
prominent from other types of respondents.” This is unclear as there were no other types of respondents in this study. We were referring to participants in the quality of care study, but this has now been removed.
Reviewer: Mary M Cameron

Reviewer's report:
All of the comments are compulsory revisions.
1. Is the question posed by the authors well defined? The topic has been researched in many other settings in Nepal and therefore it is not a substantial contribution. Framing it in terms of development goals is problematic for a health related journal; I would expect to see more about community, family and individual health care needs taking precedence. Also, framing some barriers as sociocultural or cultural is very simplistic. There’s an enormous ethnographic literature on women in Nepal, none of which is cited.

The paper uses the framework by Thaddeus and Maine, who refer to ‘cultural factors’. One of the reasons we have made reference to MDGs is to highlight the acknowledged global importance of the issue. This is common in health related journals. References to some ethnographic literature have been added to the discussion section.

2. Are the methods appropriate and well described? The methods seem appropriate though the authors don’t explain why they only translated half of the interviews. They also use quantitative analytical terms like tabulation that are unclear in this context, while also referring to comparison to another study that doesn’t seem integrated into the discussion.

We have added an explanation to the text of the reason why only half of the transcripts were translated. It is best practice that data is analysed in the language it is collected, but in order to ensure data quality, half was translated enabling the principal investigator (who does not read Nepali) to lead the analysis.

Triangulation is explored by comparing transcripts. Tabulation of coded data by respondent group enables comparison. It is common in some types of analysis strategy, for example in framework analysis. As we have removed all reference to the quality of care study, we have removed this reference to tabulation in the abstract and methods sections.

In response to both reviewers, comparative data from the quality of care study has been removed, and is only discussed in the discussion section.

3. Are the data sound? Hard to evaluate as the numbers are small and the research experience of the team is not discussed. The narrative sections are interesting. Some of the claims, like the hours of hospitals, are not correct. Very few demographic characteristics like age are given.

The research experience of the team is discussed on p7

“Data were collected in Nepali by Nepalese researchers who were trained in qualitative and quantitative research methods by the Nepali speaking Principle
Investigator (JM). The Principle Investigator accompanied researchers in the field, to observe initial data collection. Topic guides were piloted and adjusted in an iterative way."

Our sample is an acceptable qualitative sample size. We have presented data that adequately describes our sample, and factors affecting their reasons for home delivery (whether it was their first child, their ethnicity, type of health facility). This descriptive data indicates the age of the woman.

The hours of periphery level health institutions (Primary health centres, health posts, sub health posts) are from 10am until 2pm. This is correct. We have added the word periphery to clarify that we are not referring to hospitals.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? No. I found the data to be simplistic and not well contextualized. There are more traditional health providers than shamans, who typically don’t assist in deliveries, but midwives do. I found the parenthetic (intervention) and (control) and other such notes in the document to be unusual and distracting, and more appropriate for the researchers own future work, but not relevant to a research-based article. Certain claims are not supported by citing the relevant literature, such as women not having decision-making authority, or having little contact with others in the community.

Study respondents told us that they used a healer when they were ill, and we have supported these findings with a quotation. This concurs with the literature. Traditional healer, or shaman, is used as a catch-all term in this study – we are aware that there are many different types, and in Makwanpur we know (based on our previous research) that many families first call a healer who lives close to the pregnant woman, or a family member to bless the delivery. We have added an additional reference. In Makwanpur we know that the use of Traditional Birth Attendants is very low (based on our research), particularly since they are being ‘phased out’ as a cadre.

There is no Midwifery cadre in Nepal – there are Auxiliary Nurse Midwives (ANMs), who are not recognised by the International Confederation of Midwives, and do not have the level of training and skills necessary to provide midwifery services. There is a national movement to start Midwifery training in Nepal (Bogren et al 2013) http://dx.doi.org/10.1016/j.midw.2013.07.019i).

It is usual to have identifiers on quotations, regarding how respondents were sampled, but as the reviewer found these distracting, we have removed them.

We have added references in the discussion section regarding women’s status and decision-making authority.

5. Are the discussion and conclusions well balanced and adequately supported by the data? The discussion of status is limited to only patrilocal residence. A much better context should be provided, particularly given the ethnic
heterogeneity of the sample.

We have added some discussion and references in regards to ethnicity and women’s status in society. We maintain our focus on the women’s status within the family as this is the most relevant to our study.

6. Are limitations of the work clearly stated? Somewhat, although the context of interviews isn’t discussed and the familiarity of the community with the research team and the research team’s experience with ethnographic methods isn’t fully discussed.

The research team did not use ethnography. We have stated the team’s experience and training in the methods section. We have added a sentence in the method section detailing the context of the interviews.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? I didn’t see such a discussion.

We state in the introduction that this study is part of formative work to inform a randomised controlled trial. We also discuss in the discussion section how this research will inform the development of our intervention. We have altered the text in the discussion to make this clearer.