Author’s response to reviews

Title: Maternal deaths in eastern Indonesia: twenty years and still walking, an ethnographic study

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Author’s response to reviews: see over
Dear Jane

Thank you for your comments and the reviewers’ comments. In this cover letter we give specific responses to your comments and reviewer ZM comments below. We note that reviewer LH did not require any revisions before publication.

Editor's Comment:

I would have liked to see more of a focus on practical strategies that emanated from their findings. The focus is on choosing the birth place and selecting a birth attendant as well as decisions about birth preparations. It is clear that women prefer giving births in their communities so it would have been helpful to have focus on how to empower women to ensure that their preferences are safe.

Various practical strategies are discussed against the findings in the discussion section: access to functioning telecommunications; educating medical staff to address social stigma; misoprostol available at village level; antibiotics and hand washing around birth; raising awareness amongst village leaders for response i.e. desa siaga. These practical strategies are reiterated in the conclusion section.

It was also difficult to hear women's voices in this article as so much of those perspectives were mediated by men and community members.

The interviews were conducted with families, the dead women’s relatives, and about half the respondents were women. These women included mothers, sisters and aunts of the dead women, traditional birth attendants (often relatives).

This society is a traditional, patriarchal society and so the men often make the decisions about health care and so the men’s opinions are important in understanding access to care.

Some examples would have benefited from more specific responses, for example on page 7 the authors indicate that the houses “were not far” from emergency maternal care but it is unclear what this means.

We have added more specific information about distance (and more importantly the time taken to travel) to health facilities for the cases discussed, including estimated travel time.

Stigma is mentioned several times in the discussion section so it would have been helpful to have a definition of stigma.

A definition of stigma has been inserted.

Otherwise, well done!

Reviewer's report

Title: Maternal deaths in eastern Indonesia: 20 years and still walking: an ethnographic study
Reviewer: Zubia Mumtaz

Reviewer's report: This is a poor manuscript. An ethnographic analysis of 11 maternal deaths in a remote region of Indonesia has the potential to produce a rich body of evidence to answer the question of why, despite over 20 years of active government efforts, Indonesia has failed to reduce its MMR. Unfortunately this paper has failed to meet this potential. It reads like an undergraduate paper. I normally like to suggest ways to improve the manuscript, but in this case I think there is a need for a major revision, starting from re-analyzing the data.
We have clarified the scope and context of the research in the introduction section – establishing the focus of the study at the family and village levels. We have added an account of the coverage of the Indonesian government initiatives, health care cards and resident village midwives, for these cases.

**Major Compulsory Revisions**

1. **Is the question posed by the authors well defined?**
   
   Not really. Overall, the rationale for the research question is poorly thought out and structured. Rather than focusing on the 3-delays model, a relevant but old idea, it would be better to argue, why despite 20 years of active government interventions to address Indonesia’s high MMR, women continue to die. The authors need to read up on the large body of lit on the Indonesian maternal health program and develop a rationale that goes beyond simplistic statements about factors that affect maternal health services delivery or that skilled birth attendants are needed.
   
   The 3-delays methods has been used to describe the data. I think a better framework to describe this data is one developed by Price (2007).
   
   The focus of this study is the village and family. Higher level studies have been attempted and alone fail to solve the problems of high MMR. Our study aimed to gain a deeper understanding of the fine scale factors that affect health outcomes in rural settings.
   
   Thank you for the suggestion of using the framework described by Price and Hawkins (2007). We have included reference to this framework, however we retain the three delays model of Taddeaus and Maine as our main framework. It is the seminal framework for maternal health care as it pertains specifically to emergency maternal health care and there is a continuing development and refinement of this framework in the literature, including Gabrysch and Campbell (2009) as referred to in our paper. We suggest that the three delays framework is more appropriate for consideration of responses to maternal emergencies for which time is such a crucial factor, unlike for treatment of diseases which is the basis of the framework of Price and Hawkins.

2. **Are the methods appropriate and well described?**
   
   No, there are issues. There is confusion about the exact methodological approach taken. Initially you mention this is an ethnographic study, but later it becomes a phenomenological approach.
   
   Can you explain the use of the two terms, and how is this a phenomenological study?
   
   Clarification has been included in the Methods section but phenomenology is about people’s experiences, and ethnography is a method used to collect people’s experiences.

   Please provide more details about how the data was collected? How were the group discussions recorded, permission taken etc. Were any observation notes. How long did data collection last?
   
   What was the role of the Australian researchers in data collection? Where do they come from, issues with language, were they involved in questioning or were they silent observers. How trained were the local researchers in qualitative data collection?
   
   The methods are described in detail on pages 3 and 4. Some more information has been added about the training in data collection by the research team, and about the roles of the research team in the collection of data.

   Please provide more details about the manner win which the data were analyzed…how were the codes developed, what methods were used to extract themes. Was a qualitative data analysis package used.
   
   The method of analysis is clarified and justified. No computer package was considered necessary and so was not used. Use of computer software is not compulsory in the analysis of qualitative research (Grbich 2012; Liamputtong 2012)

   Was the research team invited by the local church leader to come and explore the high maternal death rates, or did the researchers request the help of the leaders. Unclear, but important distinction.
   
   Previous projects on health mapping had identified this area as one with a high MMR. When this information was discussed with the church leader in this area, via a local Indonesian researcher, the church leader invited the research team to work in her area. This has been clarified in the text, with reference to a paper explaining the choice of research location.

   Overall, the methods section reads like an epidemiological study…not a ethnographic study.
The methods are ethnographic but are complemented by spatial science approaches in a paper currently in review. In our ethnographic study we aimed to investigate the circumstances of maternal deaths. Although the stories recounted suggest possible biological causes for the deaths, these medical causes are not surprising or informative. We were interested in the social and cultural factors that influenced decisions about seeking care and the sequence of social, cultural and geographic factors that influenced access to care.

3. Are the data sound?
Possibly yes, but given the issues with the analysis, I am not sure.
The detailed description of the methods indicates the data are sound. We hope this is clear. We have added reference to a chapter in press which gives more detailed description of the methods used.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
There are huge issues with the way the data is analyzed. The fact that there are 7 themes is problematic. In a manuscript there should be 2-3 themes. The fact that there are so many categories suggests that data needs to be analyzed to a higher level of abstraction. Moreover, each of the categories (I will not call them themes) are poorly titled ...when is a single word ‘poverty’ a theme? Each category is poorly described and more often than not the quotes don’t support the main argument being made.

There are five themes identified: 1. remoteness; 2. poverty; 3. Traditional customs; 4. Health system dysfunction; 5. Fatalism. We are not aware of any limitation on numbers of themes in the literature. We feel that these themes are quite distinct and represent the information shared with us by the respondents. For example Price and Hawkins (2007) identified six key issues, which included “poverty”. We have expanded the explanation of the themes in the revised version of the paper. We have checked and revised the quotes used, adding linking and clarifying sentences.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
Again no. The discussion section contains results. It fails to locate the results in the literature. In paragraph 2 of the discussion the results are presented in a diagram to illustrate the sequence of barriers to accessing care. We leave to your editorial discretion as to whether this should be moved to the results section. We have added references regarding the issues of stigma and fatalism.

6. Are limitations of the work clearly stated?
No
Additional sentences stating the limitations of the study have been added at the end of the discussion section. These limitations are acknowledged in the scoping of the study in the introduction and in the statement of conclusions.

Level of interest: An article with potentially large interest for people working in global maternal health.

Quality of written English: Not suitable for publications unless edited. Words like ‘distressingly tenacious’ and ‘evocatively’ look odd in a academic manuscript.
The word “distressingly” has been removed. The word “evocative” has been retained because this word describes why this article has become a citation classic (cited 923 times according to Google scholar).

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests.