Author's response to reviews

Title: Psychological and social consequences among mothers suffering from perinatal loss: perspective from a low income country.

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Author's response to reviews: see over
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To

Editor in Chief, BMC Public Health
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Dear Dr. Norton,

Thank you for giving us the opportunity to revise our manuscript “Psychological and social consequences among mothers suffering from perinatal loss: perspective from a low income country”. We would also like to convey our sincere thanks to the reviewers for their valuable expert opinions in improving the quality of the manuscript. We have addressed the reviewers’ comments and made the necessary changes to the manuscript as suggested by them. Our responses to each of the comments by the reviewers are attached.

Thank you.

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Response to comments

Version: 2  Date: 15 April 2011

Reviewer 1: Guoqing Hu

Minor essential revisions:
1. The authors added more literature to the introduction. But the authors need to improve the organization and summarization of related literature, thus emphasizing the limitation of existing studies and the importance of the present study.

   Thank you for the valuable suggestions. Please see changes in the Introduction section of the revised manuscript, page 2 second and third paragraphs.

   Please drop 'indeed' at the beginning of 1st sentence of last paragraph (page 3).

   The suggested correction has been incorporated in the revised manuscript.

2. Please use the unified reference style for Beutel et al (1995) and add the number of women in the same sentence.

   Thank you. The correction has been done. Please see changes in page 1 last paragraph of the revised manuscript.

3. Figure 2: According to the explanation of authors to comments from Reviewer 2, they did not match the sample at two time points. In this case, it makes no sense to put them.

   We have revised figure 2 according to the suggestion. Now we have included the depression status of non-bereaved mothers in each time point (i.e. at 6 weeks and at 6 months). Please see page 16 of the revised manuscript and page 9, first paragraph.

   Sorry for the confusion about study design. Please note that the current study is a prospective study not a case-control match pair design therefore we have done separate analysis at two time point. In addition, please see the ‘reviewer 2 comments’ along with responses.

4. Table 2 and Table 4: Please provide the coding of independent and dependent variables including reference group to help understand the results of logistic regression.

   Thank you for the suggestion. Considering the word length (>5700 words) of the manuscript we avoided the basic description of coding for the variables.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Version: 2 Date: 18 April 2011

Reviewer 2: Kathleen Pajer

This is the first revision of a ms. describing an important study of the effects of perinatal death on maternal psychological and social function 6 months after delivery in Bangladesh. The authors have been quite responsive to the previous critiques, but some work remains to be done.

Major Compulsory Revisions

1. The design of the study, which was difficult to determine from the original ms., is described as case-control. However, with the major improvements in writing that have occurred in this version of the paper, it is clear that the design is a cohort design, not a case-control.

A case-control study is one in which the sample is recruited into two groups based on the outcome (disease) of interest and then the possible risk factors for the outcome are searched for in the past exposures of the Ss. In this case, if it was a case-control study, the investigators would have assembled the Ss on the basis of postpartum depression or not and then looked retrospectively to calculate the rates of exposure to the hypothesized risk factor of perinatal death.

However, the investigators did not do this. Instead, they have a much stronger design that is prospective, with a sample that elegantly assembled groups of Ss exposed and unexposed to the putative risk factor. This was not clear in the previous version of the ms., but is quite evident now. This increases the usefulness of this study enormously, but needs to be correctly stated throughout the ms.

Thank you for the valuable suggestions and constructive criticisms that helped us to improve the quality of our manuscript significantly. The scientific and expert opinions are also much appreciated. Now we have stated the word ‘prospective’ throughout the study. Please see changes in the revised manuscript. We have deleted the paragraph on sample size calculation.

2. The authors have clarified and added an analysis about the persistence of depression, using their data from the EPDS for T01 and T02. This was in response to my earlier criticism, but they did not quite understand what I was suggesting. To be more specific: they should give the reader a comparison of the rates of EPDS scores > 10 within Ss at T01 and T02 in the exposed and unexposed groups. From the data they have shown, I suspect that the rates of persistent depression would be significantly higher in the exposed group. This is important information because we know that chronic depression is associated with poorer outcomes than more acute. They could do this even with the small n in the group of persistent depression and they have a unique opportunity to examine this.

Thank you for the suggestions. We completely agree with the valuable comments of the reviewer on the aspect of persistent depression with poorer outcomes. This is an important issue that needs to be addressed in relation to postpartum depression.
research and more notably when the outcomes of childbirth are worse for example perinatal death.

We have shown the comparison of rates of depression (EPDS>10) among women with and without exposure (exposure: perinatal death) at 6 weeks (T01) and at 6 months (T02) in table 3 of the manuscript. In addition, please see the text in page 7 last paragraph of the revised manuscript. “Study women with a perinatal death were significantly more likely to be depressed at 6 weeks postpartum than women who did not have a perinatal death (Table 3). After a perinatal loss, 43% (95% CI: 33.7 - 51.8%) of women were found depressed at 6 weeks postpartum compared to 17% (95% CI: 13.7 – 21.9%) with healthy babies at 6 weeks postpartum (p = <0.001). However, at 6 months after childbirth, there was no statistically significant difference (p = 0.65) in the prevalence of depression between women with perinatal death and women who had not experienced perinatal death.”

Minor Essential Revisions

1. The Background section is markedly improved and reads very smoothly. The argument is well-described. A few minor changes are recommended:

   • Last sentence in paragraph 2 should be the leading sentence for the new topic presented in paragraph 3. The current first sentence in paragraph 3 would then need revision to remove redundancy.

   Thank you for pointing this out. We have incorporated the suggestions and revised the sentences. Please see changes in page 1 paragraph 3 of the revised manuscript.

   • Omit the comma after “Although” in line 7, paragraph 3.

   Thank you. We have corrected the typographical error. Please see the correction in revised MS.

   • “partnerships” in the next line should be singular.

   Correction was done on grammatical mistake in the manuscript. Thank you.

   • “One of the hypotheses of this current study was that women with poor pregnancy outcomes (e.g. stillbirths, early neonatal death) were at risk of suffering from further social and mental consequences compared to those with normal deliveries with no complications. This paper reports the findings of a prospective community-based study in rural Bangladesh to estimate the magnitude of psychological and social consequences of childbirth resulting in perinatal death. We hope that the study findings can be used to inform discussions around programme interventions and policies to address maternal
psychosocial health and welfare following perinatal death.” These sentences should be reworded so that the purpose and hypotheses are clearly stated. Something like: “The purpose of this study was to estimate the magnitude of psychological….death in rural Bangladeshi (not sure if this is the right term) women. We hypothesized that women who had experience perinatal loss would be more likely/have higher rates, etc. of depression and impaired social function at 6 months…etc.

Thank you for the valuable suggestions. We have incorporated the changes in the revised manuscript. Please see page 3, first paragraph.


• “Block A, B, C, and D” do not seem to be used anywhere else. Consider omitting that phrase.

We agree. However, we would like to keep the sentence about geographical blocks (Block A, B, C and D) for the description of study areas covered by the Matlab MCH project.

• The sample size calculation reference was helpful, but again, this is not a case-control study.

Thank you for the suggestion. We have deleted the paragraph on sample size. Please see the changes in the revised manuscript on page 5, first paragraph.

• What is “Burkino Faso”? This should be explained a bit.
Thanks. Changes made. Please see page 6, first paragraph.

• Data Analysis: In this revision and in the authors’ comments, I now understand why they did not want to use a repeated measures analysis or a trajectory analytic approach. However, I’d reframe it for the paper. Their main purpose (as now would be stated in the purpose and hypotheses in the Background) would be to assess the effect of perinatal loss on 6-month outcomes of postpartum depression and social function, controlling for 6 week PPD and demographic factors. They are also collecting the 6 week EPDS for an additional purpose: to determine the role of perinatal death in persistent depression from 6 weeks to 6 months. Then their data analytic approach maps nicely onto their goals and the sentence, “As the controls (women with live babies) for this study were not matched either at individual… and unexposed groups.” is no longer necessary for clarification.

Thank you for highlighting this and the positive comments. It is much appreciated.

3. Results: much improved. Figure 2 would be more useful if the histogram also included the non-bereaved group at each time point.

Thank you for the suggestion. We have revised figure 2 according to the suggestion. Now we have included the depression status of non-bereaved mothers in each time
point (i.e. at 6 weeks and at 6 months). Please see page 16 of the revised manuscript and page 9, first paragraph.

4. Discussion – much improved. The paragraph about strengths and limitations is choppy and should be revised.

We agree. However, considering the length of paper (over 5700 words) we have added a sentence to the strengths and limitations section of the manuscript.

5. Some typographical and grammatical errors remain, although most have been eliminated. The language is fine, but these other errors do need to be corrected before publication.

Our apologies for these errors, we have rectified these in the revised manuscript.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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.