Reviewer's report

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

**Version:** 2  **Date:** 18 April 2011

**Reviewer:** Kathleen Pajer

Reviewer's report:

MS: 1015795179460318

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.

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
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.
   • Omit the comma after “Although” in line 7, paragraph 3.
   • “partnerships” in the next line should be singular.
   • “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 re-worded 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.


   • “Block A, B, C, and D” do not seem to be used anywhere else. Consider omitting that phrase.
   • The sample size calculation reference was helpful, but again, this is not a case-control study.
   • What is “Burkino Faso”? This should be explained a bit.
   • 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.
3. Results: much improved. Figure 2 would be more useful if the histogram also included the non-bereaved group at each time point.

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

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.

**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.