Author’s response to reviews

Title: Anxiety, depression and relationship satisfaction in the pregnancy following stillbirth and after the birth of a live-born baby: A prospective study

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Author’s response to reviews:

Dear editor,

Thank you for your interest in this paper and the valuable comments made by the two expert reviewers, which have enabled us to improve the manuscript. We hereby submit our revised manuscript “Anxiety, depression and relationship satisfaction in the pregnancy following stillbirth and after the birth of a live-born baby: A prospective study” for consideration of publication in BMC Pregnancy and Childbirth.

When preparing our revised manuscript, we have done our best to comply with the remarks provided by the editor and the reviewers. Each comment has been given a response in the following point-to-point listing, and the manuscript has been revised accordingly. Any alterations in the manuscript are indicated using Track Changes in Microsoft Word. All listed authors have approved the final version of this manuscript for submission.

On behalf of all the authors,
Editor Comments:

Figure legends: Please remove the figure legends from the figure files and instead provide them in a separate paragraph at the end of the manuscript.

Response: The changes have been made according to the editor’s request. Figure legends can be found in a separate section at the end of the revised manuscript (lines 692-717, page 33-34).

Ana Nikcevic (Reviewer 1):

I enjoyed reading this paper. It is succinctly written and its methodology and results are clearly presented. I do not have any issues to note or requests for modification.

There is only one suggestions which I would like to make for the consideration of the authors:

In Conclusions, line 444-445, it is stated under 2) that health care professionals "should not provide definitive advice about the timing of subsequent pregnancy...." Given that the presented data clearly suggests that there is no risk to becoming pregnant within 12 months after stillbirth, I believe that the advice to parents should clearly state exactly that: i.e. that there is no firm evidence that would suggest that it is important to delay the subsequent pregnancy, but that couples should be guided by their own needs and 'readiness'.

Response: We agree with the reviewer and have changed the sentence in Conclusions to “when timing a subsequent pregnancy, couples should be guided by their individual needs, taking maternal age and medical considerations into account.” (Conclusions section, lines 474-476, page 22).

Maggie Redshaw (Reviewer 2):

This is an interesting paper based on a study carried out with appropriate and useful comparison groups. When the events in focus are relatively rare there is clear value to working with data from an established cohort such as this one in Norway with data linkage.

The following points are made in relation to the different sections of the paper.

1. It would be helpful in the title and elsewhere to refer more to 'satisfaction', than 'dissatisfaction' as the changes over time were mirrored across the groups. And were somewhat less marked than the association with depression and anxiety.

Response: We agree with the reviewer and have changed the term ‘dissatisfaction’ to ‘satisfaction’ in the title and elsewhere throughout the manuscript. The numbers and percentages in table 2 have been modified accordingly.

2. The background and introduction describe relevant literature and research questions. However, the emphasis is on the next pregnancy and some women and their partners will not go on to further pregnancies.

Response: It is correct that our study only address women who become pregnant after experiencing stillbirth. The subsequent pregnancy has been regarded as a particularly vulnerable period with potential to reactivate psychological distress. We have in previous papers (1-2) assessed the long-term impact of stillbirth on quality of life, depression and posttraumatic stress in women regardless of future pregnancies, but this was not the focus in the current manuscript.
References:


3. The methods are generally appropriate, however, the psychological measures used are short forms of what are screening instruments rather than diagnostic measures/interviews and this could be emphasised further in the strengths and limitations section.

Response: According to the reviewer’s suggestion, we have addressed this issue more thoroughly in the strengths and limitations section in the revised manuscript by adding the following section; “The estimates for anxiety and depression in our study relied on self-reporting, using short-form versions of validated screening tools. Even though short-form versions affect the measurement precision, it often remains sufficient for epidemiological purposes (43). Psychiatric symptoms may be more correctly reported in an anonymous questionnaire than in a clinical interview (44) and questionnaire-based screening tools are often used to estimate the proportion at risk of having a mental disorder in a population. However, it is important to highlight that these are not suited to make formal diagnoses” (Discussion section, lines 406-413, page 19).

4. The subgroup analyses on GA of stillbirth and timing of subsequent birth required combining the data on depression and anxiety, with women having anxiety and/or depression as a function of sample size. This is a limitation.

Response: We have added the following sentence to the discussion; “The sample size required that data on anxiety and depression were combined in the subgroup analyses on gestational age at stillbirth and inter-pregnancy interval, limiting the generalisability of these analyses” (Discussion section, lines 414-416, page 19).

5. Over what period were the data reported collected? This appears to have been quite extended, with the cohort starting in 1999 and data up to 2014 being utilised. Changes in practice, support and guidelines will have changed over time and this should be acknowledged.
Response: According to the reviewers suggestion, we have added the following sentence to the methods section; “The current study is based on version VIII of the quality-assured data files released for research on 14th of February 2014 and reports data collected from 1999 to 2012” (Methods section, lines 163-165, page 8). We have also added the following sentence to the discussion section; “Further, the data reported was collected over a relatively long time period (from 1999 to 2012) and changes in practice and support may have influenced our findings” (Discussion section, lines 388-390, page 18).

6. The findings are of wide interest with the decrease in anxiety and depression found during the first year which was then followed by an increase at the 36 months data collection point. A clear point of concern is identified. There is no mention of the wellbeing of partners.

Response: Although this study did not focus on the psychological well being of partners, we agree with the reviewer that this is an understudied and interesting topic that should be addressed in the future. We have added the following sentence to the conclusion; “This field would also benefit from studies that takes prior mental health problems into account and studies that focus on the psychological well-being of partners”(Discussion section, lines 479-481, page 22).

7. The point about higher rates of anxiety and depression during the third trimester among those who those dropped out or did not complete all five questionnaires is important and the possibility of underestimation is well made.

Response: Thank you.

8. The inter-pregnancy interval was reported not to be associated with 3rd trimester anxiety/depression, but it is not clear quite what happened at later time points. More detail on this would be helpful.

Response: The duration of the inter-pregnancy interval was not associated with anxiety and/or depression at any time point. However, due to lack of power caused by attrition and in order to limit the number of statistical comparisons, we have chosen not to include this point in the manuscript. Since the main focus of this paper is on mental health in the subsequent pregnancy, and the results section is already quite extensive, we suggest not elaborating this further, but we leave it up to the editor to decide.
9. The issue of having no information about prior mental health is mentioned. This could be addressed in further research, while the need for continued checks on mothers after stillbirth is discussed, there is little about the research implications of the study, gaps to be addressed, further research questions and so on. Some further thoughts on this point would be useful, given the possibility of further cohort studies.

Response: We have added the following section in the conclusion; “Future research should evaluate the quality of care provided to reduce psychological distress in women pregnant after stillbirth. This field would also benefit from studies that take prior mental health problems into account and studies that focus on the psychological well-being of partners” (Discussion section, lines 478-481, page 22).