Author's response to reviews

Title: Health care utilization, somatic and mental health distress, and well-being among widowed and non-widowed female survivors of war

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Author's response to reviews: see over
Dear Prof. Andersson,

Enclosed is a revision of the manuscript (1120656667608721) titled "Health care utilization, somatic and mental health distress, and well-being among widowed and non-widowed female survivors of war". We carefully considered the comments and suggestions by each reviewer, and revised the paper to address each point. The comments helped us to draft what we think is a much stronger paper than the original submission. This letter details how we handled the comments and suggestions point by point.

Dear Reviewers

We are grateful for your comments on our manuscript and have found them most helpful. As suggested, we amended the paper in the light of these comments. We think that the paper has been substantially improved as a result. This letter details how we handled the comments and suggestions point by point.

Responses to Reviewer #1

This article “Health care utilization, somatic and mental health distress, and well-being among widowed and non-widowed female survivors of war” represents a significant contribution to the literature of post-conflict mental health. The examination of the impact of being widowed in the Kosovo war on the mental health of lone mothers is a novel, interesting, and informative goal. This paper has several strengths, including the use of an interesting and well-defined population, appropriate comparison groups, and a good grounding in the literature of prolonged grief disorder, an emerging and extremely important research focus. The methodology was appropriate and carefully described in this paper. The discussion of the findings had depth, and added to the current knowledge base. Limitations were also clearly acknowledged.

One comment that I have about this paper is that it seems to follow two very interesting, but somewhat separate, lines of investigation – first the relationship between widowed/bereaved status on mental health in female Kosovar civilians; and the second considering the association between mental health symptoms and service utilization. Both of these areas are important and informative, but I wondered whether the authors might consider separating them into separate manuscripts, which would allow for a more thorough investigation of each separate issue. Alternatively, there may be a way to more thoroughly integrate these lines of enquiry.

Our response:

Thank you very much for these supportive comments about our paper. With regard to the suggestion to either separate the two lines of investigation in our article (relationship between widowed status and mental health & relationship between mental health and service utilization) or to more thoroughly integrate these lines of enquiry, we decided to not separate the two aspects in our paper. We did this for two reasons. First, and as stated in our manuscript, the general aim of our study was to investigate whether levels of health distress as well as health care utilization differ among widowed and non-widowed female survivors of war. We believe that reporting both findings in one paper is important as the two aspects are closely associated with each other: in order to have a rationale to investigate potential differences in service utilization among widowed and non-widowed female survivors of war, we first need to demonstrate whether there is reason to assume that service utilization is different in both groups. In our paper we thus first report that widowed mothers reported...
higher levels of somatic and mental health distress than non-widowed female war survivors and then investigate whether these groups differ with regard to utilization of health care. Second, our decision is based on the premise that the second reviewer suggested a revision of the manuscript on the assumption that the main content of the manuscript will not be changed. However, we do see that this comment has a valid point and we therefore removed some of the analyses based on the suggestions made below. Our responses to the comments below describe the changes that we made in this respect.

Specific comments are listed below:

Results

1. **There were a lot of analyses and tables in this manuscript. I would consider condensing some of these, or removing non-primary analyses to allow for a more focused examination of the research question**

   **Our response:**
   As suggested (see also comment nr. 3), we removed the information on single traumatic events and report now the number of war-related traumatic events only (originally reported in Table 2). The number of war-related traumatic events is now reported in Table 1 under “Socio-demographic and war-related characteristics”. We removed Table 2 altogether. See our response to comment nr. 3 for further explanation.

   Furthermore, we removed the analysis on the differences between participants in use vs. not in use of health services regarding somatic and mental health symptoms and well-being. This let us to remove the table previously labeled as Table 5. See our response to comment nr. 5 for further explanation.

2. **Table 1: I would suggest that the authors report the test statistic (t values, chi square values etc.) and the degrees of freedom.**

   **Our response:**
   We now report test statistics and degrees of freedom.

3. **Table 2: My concern with this table is that there were multiple comparisons, inflating the possibility of Type I error. I wondered if the exploration of each of the traumatic events individually was necessary for the paper – the authors may wish to remove this aspect of the table. If a Bonferoni correction was applied, for example, the level of significance of the p-value would be reduced to p < .002, meaning only one of the comparisons would be significant. In relation to the text, I also wasn’t sure about what “on the other side, bereaved married mothers reported significantly more often to exposure to war-related traumatic events not listed on the checklist of war-related traumatic events than widowed lone mothers” – is this referring to the “other” category in the table?**

   **Our response:**
   We agree with the comment that the probability of Type I error is increased if we report the traumatic events individually. Therefore, and as suggested by this comment, we now report the number of war-related traumatic events only (as part of Table 1). This way we further reduced the amount of the presented results as suggested by the reviewer. The text quoted above was indeed referring to the “other” category in the table. However, as we no longer report single traumatic events, this sentence is also no longer part of the current version of the paper.

4. **Table 3: I would be interested in seeing a comparison in PGD symptoms between widowed lone mothers and bereaved married mothers, and to see what the authors made of any differences observed here in the discussion.**
Our response:
PGD symptoms were measured among widowed lone mothers only (i.e., and not among bereaved married mothers). This was done because we did not have a hypothesis with regard to a comparison of prolonged grief symptoms among widowed and non-widowed mothers. Thus, the suggested comparison cannot be made due to lack of data.
We now report more clearly under Measures that symptoms of prolonged grief were measured among widowed lone mothers only.

5. Table 5 and 6 seemed somewhat disconnected from the previous analyses – although the results were very interesting; particularly the role of PGD in predicting number of contacts over and above other mental health symptoms. Again, I would suggest that the authors consider more thoroughly integrating these analyses, or reporting them in a separate manuscript.
Our response:
We agree that Tables 5 and 6 were somewhat disconnected from the previous results. Therefore, we removed Table 5 altogether, as reported above. With regard to Table 6 (now Table 4), we now report that no study yet has investigated the role of symptoms of prolonged grief in predicting use of health services among widowed survivors of war and we specifically state this as an aim of the current study.

6. Note that, in the results section, these analyses were described as “logistic regressions” while in the statistical analysis section, they were described as “linear regression”. I think these were linear regressions, given the authors reported beta values rather than odds ratios.
Our response:
Thank you for pointing out this mistake. We have changed the term logistic into linear.

Discussion
I thought the discussion of the findings was excellent. I have a couple of suggestions that the authors may wish to consider:
7. The authors devoted quite a lot of space to the comparison of mean scores of prolonged grief between studies. I was wondering as to the extent to which these comparisons are meaningful considering differences in populations, methodologies etc., as well as adaptations of the instruments. It might be more meaningful to apply an algorithm based on proposed criteria for PGD and estimate rates of “caseness” to be compared across studies rather than comparing mean scores.
Our response:
We agree that comparing severity of prolonged grief in different populations using different adaptations of the instrument to measure prolonged grief is problematic. Therefore and with regard to the severity of prolonged grief, we now only offer comparisons with previous studies conducted in Kosovo using the same instrument and deleted comparisons with studies from other countries.
As mentioned under Methods (page 6), preliminary findings on prevalence rates of mental disorders were published elsewhere (Morina & Emmelkamp, in press). Therefore, we now report the prevalence rates of PGD among widowed lone mothers in the Discussion section only. There, we also report that the prevalence rate of PGD was higher than in other studies conducted in Kosovo and that it was also higher than in survivors of the 1994 Rwandan genocide with loss of a parent or husband before, during, or after 1994 reporting a prevalence rate of prolonged grief disorder of 8% (Schaal et al., 2010).

8. One area that I thought may have relevance for the present findings is current conceptualizations of prolonged grief in the context of attachment relationships. It would be
interesting to read the authors’ thoughts on how the loss of a husband may play into adult attachment and contribute to ongoing distress. This may be especially important in the context of a culture in which it is not customary to remarry (e.g., form new attachment relationships), which may contribute to the extent to which the individual is “stuck” in grief reactions.

Our response:
We agree that the issue of attachment is extremely important in the context of developing and maintaining prolonged grief following loss of the husband. However, given that attachment was not included in the original aims of the study, we believe that this is an aspect to be investigated in future studies.

We report nevertheless that the accepted norm in Kosovar society that a widowed mother should not remarry, often resulting into forced lone motherhood, is likely to negatively affect coping mechanisms following exposure to war-related events and the killing of the husband and to hamper attempts at mastering socioeconomic circumstances. Thus, lone motherhood is likely to constitute a significant factor in the elevated rates of psychopathology.

9. I thought the discussion of the impact of socioeconomic circumstances on grief reactions was very interesting. There has been much research amongst refugee groups attesting to the impact of post-migration living difficulties (e.g., unemployment, financial stress) in contributing to psychopathology. The authors may wish to cite some of this literature. It seems that the loss of a central figure and source of support in the widows’ lives may be compounded by practical difficulties which serve as a constant reminder of the loss, not only of a close and important person, but a crucial source of practical and financial support.

Our response:
We now report several studies with refugees that have shown that post-migration living stressors contribute to psychopathology in addition to war- or torture related events (Gerritsen et al., 2005; Momartin et al., 2006; Silove et al., 1996; Silove et al. 1998) (page 15). Furthermore, we now report more clearly that (page 15)

“the interaction between exposure to war-related events, post-war practical difficulties, and mental health status among war survivors living in the areas of former conflict is still only poorly understood and needs further examination”.

10. I would have been interested to see more description about what “mental health services” refer to in Kosovo. Is this referring to social workers/psychologists/psychiatrists/primary care doctors? This would inform the discussion of the availability of services and potential reasons why people are not accessing these.

Our response:
We now report in more detail under Measures that:

“Mental health care consisted of services provided by either a psychiatrist or a psychologist” (page 8).

Responses to Reviewer #2
The authors should describe in more detail the fact that the widowed lone mothers had far more severe trauma. In addition, only the widowed lone mothers had lost more than one relative in the war! This is a major methodological issue. Just controlling for number of trauma events will not eliminate this across-group difference. For example, trauma scales are imperfect measures of trauma and hence certain trauma differences between the groups will not be controlled for even taking into account trauma events. When one has such radically
different groups just controlling for trauma events is not sufficient to eliminate this issue. This problems needs to be far more described and addressed.

Our response:

We definitely agree that checklists of traumatic events are imperfect measures. Yet, we would like to clarify that widowed lone mothers did not necessarily have more severe trauma (apart from war-related bereavement); the percentage of participants exposed to several traumatic events was significantly higher in the group of widowed lone mothers than non-bereaved married mothers.

Nevertheless, we are grateful that the reviewer raised this point and we have added the following paragraph to the current version of the manuscript (pages 15-16):

“The fact that widowed lone mothers reported higher number of war-related traumatic events than non-bereaved married mothers needs further explanation. Checklists of traumatic events such as the one used in this study pose a limitation with regard to comparing number of traumatic events among groups exposed to several traumatic events. The very nature of single traumatic events cannot be captured with the existing checklists of traumatic events. This indicates that even after controlling for number of traumatic events in the current study, health distress differences between widowed lone mothers and the comparison groups might exist not only due to the consequences of losing the husband and being a lone mother but also due to the experience of other war-related events. Furthermore, 16% of widowed lone mothers reported war-related loss of first-degree relatives other than the husband as compared to none in the control groups. Although we recruited both bereaved non-bereaved participants in the same localities, killing of the husband seemed to have increased the likelihood of being exposed to more traumatic events and also losing other family members as compared to participants whose husbands were not killed during war. Our findings should be seen as preliminary and need replication”.

The authors should describe whether the instruments were given by the interviewer or self-report. If by the interviewer, was the interviewer aware of the study hypotheses? This would effects that results.

Our response:

Self reports were filled in by participants themselves. Yet, two of the instruments were used in an interview format. We now report under Measures that the PGD-I and the Client Service Receipt Inventory were given by the interviewers. We also report that the interviewers were not aware of the study hypotheses. Finally, we now also report that the interviews were conducted by five female psychologists who had been trained by the first author in conducting clinical interviews.

I think that the three groups should be named in a way that is less confusing. The terms for these three groups should be better outlined.

Our response:

Unfortunately, we cannot find better labels to name the three groups than the labels that we had in our first submission: widowed lone mothers, non-bereaved married mothers, and bereaved married mothers. Furthermore, as we stated under Study Design, the current report is based on a project that has produced one prior publication (Morina & Emmelkamp, in press) in which these labels for the three groups were used. Thus, using the same labels again should be less confusing for readers. Yet, more importantly, we believe that the labels we use do concisely capture the major distinctions between the groups. All participants have in common exposure to war-related events and are mothers and what is different is that:
1. One group of mothers has reported loss of the husband during the war and having been lone mother ever since (thus, widowed lone mothers)
2. One group of mothers has reported loss of first-degree relatives after the war (and no war-related loss) and is still married (thus, bereaved married mothers)
3. One group of mothers did not report loss of first-degree relatives during or after the war and is still married (thus, non-bereaved married mothers).

The authors should better describe what is meant by specialist health care. It is confusing. The logical thing would be to predict number of visits to non-mental health care providers, perhaps also looking at just primary care.

Our response:
We now report under Measures the following:

“Specialist health care included any sort of specialist physical health care (for e.g., gynecologist or cardiologist) and/or mental health care. Mental health care consisted of services provided by either a psychiatrist or a psychologist.” (Page 8).

Furthermore, the reviewer seems to have overlooked that we had conducted the analysis looking at the prediction of number of visits to primary care only (see Table 4, previously Table 6). The second regression analysis we had conducted with number of contacts in specialist health care, including both physical health care and mental health care. In accordance with the suggestion by the reviewer, we now report a regression analysis with number of contacts in specialist physical care only (i.e., excluding mental health care). Given that only 3% of widowed lone mothers and 4% of non-bereaved mothers reported utilization of mental health services, the exclusion of mental health care led to only marginally different results (see Table 4).

Furthermore, we report now under Statistical Analyses:

“Given that only three percent of widowed lone mothers and four percent of non-bereaved mothers reported utilization of mental health services during the last three months, we did not conduct regression analysis with number of contacts in mental health care”.

In conclusion, we are grateful for the time and energy spent in offering insightful and helpful comments on this manuscript. We believe the manuscript has been markedly improved as a result, and hope that the paper will now be considered for publication in the BMC-Psychiatry.

Sincerely,
The authors