Reviewer’s report

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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Reviewer: Angela Nickerson

Reviewer’s report:

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.

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

2. Table 1: I would suggest that the authors report the test statistic (t values, chi square values etc.) and the 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 Bonferroni 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?

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.

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.

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.

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.

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.

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.

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.

**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