Author's response to reviews

Title: Mental health and wellbeing in spouses of persons with dementia: the Nord-Trondelag health study

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Author’s response to reviews: see over
Dear Editor

Thank you, once again, for your positive response on the manuscript, "Mental health and wellbeing in spouses of persons with dementia: the Nord-Trøndelag health study". We have incorporated revisions suggested by the reviewer. Attached is a list of responses to the comments made by the reviewer.

We look forward to hearing from you!

Regards,
Helga Ask

RESPONSES TO REVIEWER

1. Abstract. The authors state that “Having a partner with dementia that resided in a nursing home was associated PERHAPS with more symptoms of anxiety compared with at-home caregivers”. I think this statement is not appropriate, since their results show that the difference between caregiver groups was not significant for anxiety (p13).
Response: I agree. The statement is removed from the abstract.

2. Introduction. Although the authors have tried to restructure the introduction considerably, I think this section is still not very focused and some parts are more appropriate for the Discussion (e.g. strengths of their study). Also, I would advise to put the aims of the study together and make these more specific.
Response: We have tried (once again) to make the introduction more focused, especially the last section. As suggested, the aims of the study are put together. Also, we have tried to make it clear that the main aim is to examine the difference between caregivers and non-caregivers (and not to estimate the level of anxiety and depressive symptoms in caregivers).

3. Methods. The methods section provides a lot of details which can be omitted. Also, it would be helpful for readers to structure the measures section and differentiate between Outcome measures, main determinant, potential mediators and moderators.
Response: Redundant details have been omitted from the method section in this new version of the manuscript. As suggested, additional headlines (outcome measures, main predictor, potential mediators and moderators) have been included.

4. There are way too many tables. I could see dropping Table 1 and 2, or present this in an Appendix. Also, consider to present only the adjusted analyses and combine Table 4-6.
Response: As suggested, Table 1 and 2 were dropped, and Tables 4-6 were combined into one, resulting in a total of 3 tables in the new version of the manuscript.

5. Discussion. Comparison with literature: Although it might be difficult to compare findings with previous studies investigating the incidence with diagnostic measures, there are lots of studies which evaluated symptoms of depression (and anxiety) with self-report questionnaires. E.g. in the introduction the authors mentioned several previous studies which showed that 20-50% of dementia carers present high levels of depression. How do the findings of this study relate to these figures?
Response: We have now made it clear in the introduction that the main aim of this study is to compare the caregivers with the non-caregivers, with focus on the difference in level of depression between the dementia caregivers and the rest of the population (page 5, line 16-19). Our aim is not to estimate the level of anxiety and depression in the caregivers in terms of high versus low levels of distress, but to tell something about the association in the total population. One important feature of our design is that we do not dichotomize our outcome
measures. By using continuous measures, we keep all of the variance between individuals. Our results are based on mean levels, and they do not say how many of the caregivers that had high and how many had low levels of depression. In the beginning of the discussion we compare our estimates with studies relevant and similar to our, for example to a large meta-analyses based on 84 studies comparing caregivers and non-caregivers. These results are directly comparable to ours. Those studies you mentioned from the introduction were included there based on your previous comments to this article. In this context they are relevant because they all indicate an association between caregiving status and distress, but due to the differences in measurement assessments and study designs are not comparable to the results from this present study.

6. Discussion. Clinical relevance of the findings: How many persons scored above the HADS cut-off indicating the presence of clinically significant anxiety and depression? Can the authors elaborate on this in their discussion?
   Response: As described above, the aim of this study was to estimate group differences without the use of clinical cut-offs. Our results do not give any indication on how many scored above or below a certain cut-off. The data used for the analysis in the present study includes a valid measurement for assessing symptoms of anxiety and depression, but not clinical levels. A section where we elaborate on the clinical significance is already included in the conclusion section.