Reviewer's report

Title: Post-conflict mental health needs: A cross-sectional survey of trauma, depression and associated factors in Juba, Southern Sudan.

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Reviewer: Claudia Catani

Reviewer's report:

The paper 'Post-conflict mental health needs: A cross-sectional survey of trauma, depression and associated factors in Juba, Southern Sudan' addresses the interesting topic of mental health characteristics in a post-conflict population. The manuscript is very well written and has several methodological strengths in particular with respect to the high standards that were used to recruit a representative sample of participants.

However, I am not convinced of the overall scientific importance of the study given that its main outcome is a high prevalence of PTSD and depression in a post-conflict population which does not really add anything new to the existing literature. Also the possible predictors of PTSD such as gender, displacement and trauma exposure have been addressed already in previous studies. Of course, this may be the first representative study with people in Juba town, but I am not convinced whether this fact alone uniquely contributes to the discussion about PTSD in the aftermath of organized violence.

Apart from this, there are other shortcomings of the manuscript that I will list below in a point-by-point discussion.

Major Compulsory Revisions

1) The Introduction is too short. Instead of only addressing the current and past political situation in Southern Sudan, it should review as well the existing literature with respect to mental health consequences of civil war and challenges in post-war societies in general. In particular, I am thinking about the already existing epidemiological insight into trauma load and related consequences in a sample of Southern Sudanese nationals and refugees (Karunakara et al., 2004; Neuner et al., 2004). The authors should clearly state in the Introduction the additional value of the data presented here.

2) On page 3 (bottom), the authors state that “the study included a continuous outcome measure (with findings presented elsewhere)...”. I think it is important to provide more details about these data and to also give a reason for the decision to present these data in a separate publication.

3) page 6 (2nd paragraph): What were the contents of the one week training provided for the study? Did interviewers already have clinical knowledge? Given the diversity of clinical concepts included in the interview (trauma, PTSD, depression), I think that it is essential to know how well the interviewers were
4) I am really surprised about data collection being accomplished in only 10 days (p.6, “data collection took place between 20 and 30 November 2007). By taking into account that there were 20 interviewers who conducted 1242 interviews in that period of time, I understand that every interviewer did carry out 6 interviews a day on 10 consecutive days. From my experience with epidemiological data collection in war-torn populations, I consider this an incredibly high interview load. I therefore have some doubts about the reliability of data and am not clear whether and when supervision of the interviewers did take place. I would invite the authors to address this issue. Also, the average duration of an interview should be noted.

5) Description of the results related to ‘Exposure of Trauma’ (page 9):
In the first paragraph, the authors write “23% of respondents had ever experienced 8 or more of the 16 trauma events covered in the questionnaire”. In the next paragraph, it reads “13.7% of men and 9% of women had experienced 8 or more of the 16 trauma events asked in the questionnaire.” These numbers do not fit to the percentages given for the whole sample.

6) Results, page 10: The strong association of “gender” in the multivariate logistic regression analysis needs further exploration. In particular, I am not sure, whether this strong effect of gender could not be attributed to the frequent experience of sexual violence in women as it is often the case in epidemiological studies on PTSD. Were there any differences between men and women with respect to exposure to sexual violence? Also, I am surprised that “rape or sexual abuse “did not result as a variable associated with PTSD. Has this factor been included in the analyses, at all?

In the discussion (page 15, first paragraph), the authors acknowledge that “there may have been underreporting of certain sensitive traumatic events”. Are they referring to disclosure of sexual violence? Actually, comparing the percentages in the present study to the findings in a study with a large population in Southern Sudan (Karunakara et al., 2004) the prevalence rate of rape and sexual abuse are much lower. Could this also be due interviewer skills or the way this question was asked for?

For instance, in the Karunakara study, people were presented with a detailed event checklist including a number of different experiences related to sexual violence (e.g. forced prostitution, rape, sex for food, witnessed rape of a woman etc.) and not only a single question. I wonder how this difference in addressing this very sensitive topic might have affected the prevalence rate presented in the present study.

7) With respect to the discussion and conclusions, I have the same major reservations I mentioned regarding the introduction section. All of the findings of the present study (high prevalence of PTSD, dose-effect of depression and PTSD, and specific predictors of PTSD) confirm findings of previous studies with war torn populations, in particular with Sudanese refugees and Southern Sudanese. It is very difficult to tell what the additional value of this manuscript is,
and unfortunately even the authors do not make that clear in their discussion part.

Minor Essential Revisions

1) Somewhere in the method part, the authors should mention that all participants signed an informed consent. Also, I would like to know whether, after a completed interview, the interviewer did provide some psychoeducation in case, the participant was diagnosed with a clinical disorder. Were there any counselors or mental health institution affected people could be referred to?

2) Page 5 (2nd paragraph): Instead “life-time exposure to traumatic events” it should read “traumatic event types”

3) Spelling error in table 5 (page 11): “being inured”

4) Page 9: I think it would be important to mention the overall percentage of people who had experienced at least one traumatic event asked for in the questionnaire.

5) Page 12, bottom: I am not quite sure, why the authors do mention PTSD prevalence rates in an Afghan population as a comparison to the present data.

**Level of interest:** An article of limited interest

**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.