Author's response to reviews

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

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

Dear Editors,

Further to your email of 17 November 2008, a revised version of the above referenced manuscript has now been submitted. We are extremely grateful for the prompt and detailed comments provided by the two reviewers. We have listed below point-by-point responses to the reviewer comments, including references to alterations made in the revised manuscript.

As requested by the editors, the manuscript includes a strengthened justification for the contribution made by the study, in reference to the existing literature. This is given in the first five paragraphs of the discussion section of the revised manuscript. This contribution includes the following elements. (i) our study is the first study to be conducted on mental health in post-conflict Southern Sudan. (ii) The study provides important new evidence on high levels of PTSD, and also depression which has not been previously researched in Southern Sudan. (iii) It investigates the association of individual trauma variables on PTSD and depression. (iv) It investigates factors associated with exposure to trauma. (v) Our study highlights the continued exposure to traumatic events amongst populations in Juba, and the influence this has on PTSD and depression. The study therefore contributes to evidence on the scale of need and also guidance for interventions to address mental distress and prevent future exposure to trauma events.

Please do not hesitate to contact me should you require any further information.

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REVIEWER ONE

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.

- A brief overview of some of the existing literature on mental health consequences of conflict has been added to the introduction (please see page 4, paragraphs 1, 2).
- The existing epidemiological data on trauma and mental health in Southern Sudan nationals and refugees has been included in introduction (Karunakara et al., 2004; Neuner et al., 2004) (please see page 4, paragraph 3).

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.

- The continuous outcome measure was the SF-8 and it was used to measure general physical and mental health. We have clarified this in the revised manuscript (please see page 5, paragraph 4). We chose to present this data elsewhere as we felt that the inclusion of multiple outcomes which mixed specific conditions of PTSD and depression with general health status outcomes would add unnecessary complexity to the manuscript. We also felt there was no intrinsic value in trying to include and compare these different health outcomes in the same manuscript.

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 prepared.

- The training methods involved presentations, group discussions, group and paired role plays and discussions on interview technique and questionnaire familiarisation. Written training guidelines
were provided as training aids to the data collectors. A summary of the training contents has been added to the revised manuscript (please see page 8, paragraph 3). This training covered the following elements:

- Background, aims and intended value of the study.
- Presentations and group discussion on mental health and research, focusing on PTSD and depression (including background, symptomology, effects, examples from other studies).
- Detailed review of the individual items in the survey.
- Ethical issues of informed consent, confidentiality, and anonymity.
- Ensuring high quality and avoiding bias during the interview process. This included accurately reading and recording questionnaire items and response options; techniques to ensure clear respondent understanding of the questions and response options; good interview techniques to improve accuracy and avoid respondent distress.

- There is a complete dearth of clinically trained mental health workers in Southern Sudan and it was not possible to find interviewers with clinical knowledge who spoke fluent Juba Arabic and/or Bari. The interviewers all had previous experience in data collection, including training on survey interview techniques. The developers of the HTQ and HSCL-25 note that these instruments can be administered by lay persons who have received training (Mollica RM, L. Massagli, M. Silove, D. 2004). We have clarified this in the revised manuscript (please see page 8, paragraph 3). It is important to note that the interviewees were not expected to screen individual respondents for PTSD and depression. This was a population-based research survey and analysis for population level rates of PTSD and depression was conducted after the data collection had been completed.

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.

- We recognise the importance of interview load and interviewer burden. The average interview time was 42 minute and we have noted this in the revised manuscript (please see page 8, paragraph 4). There was very little travel time as the survey took place within the small geographic area of Juba town and so the working day could be spent on data collection. The work load was discussed and agreed by the interviewers. Supervision was conducted by the lead study author (BR), another member of the study staff, and 4 team leaders. We would monitor the actual data collection process, rapidly review completed questionnaires for missing or anomalous items; and observe and discuss with the interviewers their work burden. We do not believe that this was an incredibly high interview load and certainly do not feel that it may have systematically negatively affected the reliability of the data.

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.

- The wording for this paragraph was misleading and the wording and data have been corrected to more accurately reflect the intended meaning of exposure rates within male/female categories rather than the overall sample (please see page 11, paragraph 2). In addition, we have added a new table to present the rates of reported exposure to trauma, by men/women, so as to provide more and clearer data (please see Table 4).
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.

- All variables recorded in the questionnaire (including all individual trauma exposure variables such as sexual violence) were included in the multivariate analyses and we have clarified this in the revised manuscript (page 9, paragraph 1). There was a reported difference in exposure to sexual violence between men and women. However, this difference was not statistically significant (please see new table 4), and so was not included with the text of other trauma exposure variables which showed statistically significant differences between men and women.
- The multivariate regression analysis did not show a statistically significant association between sexual violence and PTSD. We have noted the need for further investigation into the association of “gender” with PTSD and depression (please see page 14, paragraph 2).
- We have made more explicit that the underreporting could apply to the questionnaire item on sexual violence We provide comparative data on rape with other studies, and note that our study did not explore different types of sexual violence such as forced marriage or forced prostitution through separate questionnaire items which may mean the results may not adequately reflect the different types of sexual violence experienced by respondents (please see page 16, paragraph 3).

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.

- The study by Karunakara, Neuner et al is an extremely important and valuable study and we have increased references to the study in the manuscript. However, their study does not negate the need for further studies. Our study contributes new information in a number of ways. (i) our study is the first study to be conducted on mental health in post-conflict Southern Sudan. (ii) The study provides important new evidence on high levels of PTSD, and also depression which has not been previously researched in Southern Sudan. (iii) It investigates the association of individual trauma variables on PTSD and depression. (iv) It investigates factors associated with exposure to trauma. (v) the study highlights the continued exposure to traumatic events amongst populations in Juba, and the influence this has on PTSD and depression. The study therefore contributes to evidence on the scale of need and also guidance for interventions to address mental distress and prevent future exposure to trauma events. We have adjusted the discussion section to better clarify the contribution made by this study. (please see paragraphs 1 to 5 in the discussion section).
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?
   • The issue of informed consent has been clarified (please see page 8, paragraph 3). The interviewers did not provide some psycho-education in case participants were diagnosed with a clinical disorder. The results of the individual questionnaires were not calculated on a real-time basis so interviewees could not refer respondents at that point in time (please see response to point number 3 above). Given the anonymous and confidential nature of the survey, it was not possible to return to individual respondents whose scores indicated symptoms of PTSD and depression to refer them to sources of support. In addition, the survey instruments provide indication of symptoms of PTSD and depression, not a clinical diagnosis, and the interviewers were not qualified to offer psycho-education (please see response to point number 3 above).

2) Page 5 (2nd paragraph): Instead “life-time exposure to traumatic events” it should read “traumatic event types”.
   • We have altered this to be ‘life-time to exposure to traumatic event types” (please see page 7, paragraph 2).

3) Spelling error in table 5 (page 11): “being inured”.
   • We have corrected the spelling error to “being injured” (please see Table 6).

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.
   • The overall percentage of people had experienced at least one traumatic event asked in the questionnaire has been added (please see page 10, paragraph 3).

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.
   • The Afghanistan references were included to show comparisons with other ‘post-war’ populations. This has been moved to the introduction section. (please see page 4, paragraph 2).
REVIEWER TWO

The literature on forced migration and mental health is not extensive. The study therefore is an important one, assessing the prevalence of PTSD and Depression in a post-conflict community. The difficulties involved in carrying out such a survey should not be underestimated and I comment the authors in carrying out a well thought-out and analyzed study.

The major limitation of the study is the lack of assessment of function. Assessing function and coping strategies would have added considerably to the study and strengthened any recommendations for policy or action.

- We have noted as a limitation that the study did not assess function and coping (please see page 17, paragraph 2).

The authors acknowledge and cite previous work done in the field. The introduction should have ideally included a background on the conflict and post-conflict situation in Juba as well a discussion on the implications of the study.

- We have expanded the introduction to include a brief description on the conflict and post-conflict situation in Southern Sudan (please see page 3, paragraphs 1-3). We have also added some additional information on Juba in the methods section (please see page 5, paragraph 3).
- We have expanded on the implications of our study in the discussion section (please see page 15, paragraphs 2 and 3).

Major Compulsory Revisions
None

Minor Compulsory Revisions

1. A short history of the conflict with available data on forced migration should be included in Introduction. This will help contextualize the results of the survey.

- We have expanded the introduction to include a brief description on the conflict and post-conflict situation in Southern Sudan (please see page 3, paragraphs 1-3).

2. Also, briefly discuss the state of security, health and other social services in Juba Town.

- A brief summary of health services and security has been added to the revised manuscript (please see page 5, paragraph 3).

3. What is the break down by ethnic groups in Juba and is the distribution reflected in data presented in Table 2?

- The closest comparative data is from an HIV behavioural survey in Juba by UNHCR in November and December 2006. This survey recorded that 56% were Bari-speaking, 13% Muru, and 6% Madi. These data have been added to the manuscript (please see page 5, paragraph 3). These data also correspond with data from our study.

4. In the Methods section, describe the continuous outcome measure on which sampling calculations were made.

- The continuous outcome measure was the SF-8, used to measure general physical and mental health. This has been clarified in the revised manuscript (please see page 5, paragraph 4).
5. Elaborate EPI method used for selection of households.
   • A brief description of the EPI-based method has been added (please see page 7, paragraph 1).

6. The Methods section should be abridged.
   • We have adjusted the methods section to include additional points raised by reviewer number 1.

7. Restatement of results in the discussion section is unnecessary. The results should be discussed in depth.
   For instance;
   a. How do the results reflect the nature of life in Juba?
   b. How do the current levels of PTSD and depression affect the rebuilding of post-conflict societies, as stated in the introduction?
   c. What are the current gaps in services and what specific actions need to be taken?
   • (a). It is difficult to comment on how the results reflect the nature of life in Juba without relying on speculation as there is limited data on other social indicators. We have therefore not really addressed this recommendation.
   • (b). We have briefly noted that given the high levels of mental distress and absence of services and support, it is likely that there will continue to be high levels of poor mental health and associated physical health problems, and that the ability of individuals to properly function and support themselves and their families may also be impacted (please see page 15, paragraph 3). However, we have not given any more detail on this as we did not collect data on functioning and we have we have noted this omission as a limitation and an area requiring future research (please see page 17, paragraph 2). We have also noted that studies have explored how exposure to traumatic events and high levels of mental distress may influence respondent attitudes to reconciliation in post-conflict countries such as Southern Sudan (please see page 16, paragraph 1).
   • (c). We have responded to the issues of current gaps and services and what specific actions need to be taken in the discussion section (please see page 15, paragraph 2).

Discretionary Revisions
1. A brief review of the literature on PTSD, Depression and other psychiatric morbidities in complex emergencies, after the introduction section, will add to the results and discussion of mental health in South Sudan
   • A brief overview of the existing literature on mental health consequences of civil war has been added to the introduction (please see page 4, paragraph 2).