Author's response to reviews

Title: The influence of demographic characteristics, living conditions, and trauma exposure on the overall health of a conflict-affected population in Southern Sudan.

Authors:

Bayard Roberts (bayard.roberts@lshtm.ac.uk)
Eliaba Yona Damundu (eliaba2004@yahoo.ca)
Olivia Lomoro (achaber@yahoo.co.uk)
Egbert Sondorp (egbert.sondorp@lshtm.ac.uk)

Version: 2 Date: 29 April 2010

Author's response to reviews: see over
Dear Editors,

Re: MS: 1460759913294802. The influence of demographic characteristics, living conditions, and trauma exposure on the overall health of a conflict-affected population in Southern Sudan.

Thank you for your email of 7 April 2010 concerning the above referenced manuscript. Please find attached a revised manuscript. We greatly appreciate the very thorough and constructive comments from the reviewers. We have addressed their concerns in the revised manuscript (shown in track changes as requested), and have also given a point-by-point response to their concerns below.

Your promptness in this review process would be much appreciated given the long delays in the prior review process that you acknowledged in your email. Please do not hesitate to contact me should you require any additional information.

Kind regards.

Dr. Bayard Roberts
(corresponding author)
Referee 1 (U.K. Karunakara)

The authors have to be commended for carrying out this survey in southern Sudan. The associations outlined in the study support what has been found in other studies carried out in Uganda and in Ethiopia. The results and recommendations of this study however, should inform aid agencies and public health officials to provide appropriate services for the population of southern Sudan.

Major Compulsory Revisions

None

Minor Essential Revisions

1. Increasing age has been shown to have a negative association with both physical and mental health. It is therefore important to describe the age structure of the respondents a bit more. Include, for example, the median age and/or range in Table 1. Place this in context with census data that was available at the time of the study.

We have added age data to Table 1 (median and quartile range). However, we have not been able to obtain publically available age data for Juba from the 2006 census in order to make comparisons.

2. The authors mention that the study instrument was an adaptation of the Harvard Trauma Questionnaire. In a short paragraph in the Questionnaire section, outline socio-economic variables included in the study questionnaire. For example, educational attainment, income, household size etc.

The demographic characteristics and living situation variables in the questionnaire have now been described in more details (please see page 6, paragraph 2).

3. The conclusions presented in the abstract and the paper are not the same. The conclusions should include a short summary of the analyses of data. In a separate short chapter perhaps, the authors should include recommendations for policy makers.

The manuscript conclusion has been revised and is now specific to the main study findings. The conclusion is now the same in the abstract and the main manuscript. We were not sure about the suitability for a separate chapter to BMC Public Health and so seek guidance from the BMC Public Health Editorial team as to whether this is necessary.

Discretionary Revisions
Referee 2 (Melanie Abas)

This paper describes a cross-sectional survey of general physical and mental health in Juba, the capital of southern Sudan, and an exploration of associations between general physical health, general mental health and three broad groups of risk factors. The work that this research group are conducting in this under-served area is clearly very important and of interest to a wide international readership.

1. While there is some background on the context of the study at the beginning, I think the authors should give more information on the context in Juba as it links to the characteristics of the participants. For instance, was this both a place from which people fled and to which both original residents and other displaced people are returning? Does the sample reflect this? What are the various living situations in Juba and for the participants e.g. are there camps for displaced people, are most people living in rented or owned accommodation, are they living alone or with family members? The choice of descriptive characteristics in table 1 could usefully be broadened to provide more context.

We have added more description on Juba in the background section (please see page 4, paragraph 1) and adding data on IDPs in the sample (Table 1; page 8, paragraph 4). However, we did not ask questions in the survey about whether people lived in rented or owned accommodation and so cannot provide data on this.

2. The government of Sudan in the north is mentioned and later the Government of Southern Sudan. Please could this be clarified?

This distinction between the government of Sudan in the north and the Government of Southern Sudan has now been clarified (page 3, paragraph 2).

3. Broadly, what are the current patterns of help-seeking for health care and/or emotional distress? What services exist and how will the results feed into any services or policy.

We could not find data on patterns of health service utilisation in Juba. We have noted as a limitation in our study that we did not collect data on health care utilisation and have recommended future studies should explore this issue (please see page 12, paragraph 3). We have also reiterated the findings from our study on the lack of access to essential medical care and have highlighted the need to increase availability of health services (please see page 11, paragraph 2); and the risk of increasing insecurity (please see page 12, paragraph 1).

4. The results section of abstract is currently somewhat confusing e.g. what association was found with gender? and age?. Please make sentence 2 in the results section more clear. In the methods section it would be helpful if you could say
what type of validity of SF-8 you assessed and how you assessed it (please see below). Some data would improve the abstract.

We have re-worked the results section in the abstract to make the associations clearer, clarify the tests used on the SF-8, and added results data (except for each of the trauma variables as we felt this may make the abstract too wordy but we can obviously add this if it is felt to be necessary).

5. As ?PCS? and ?MCS? are not well-known abbreviations I wonder if it might be preferable not to use them in order to improve clarity of the text.

We have replaced PCS and MCS with the terms general physical and mental health throughout the manuscript.

6. My key question is what made you choose a general measure of physical health and of mental health rather than a tool that would elucidate specific symptoms? This requires some justification in the background section which would help clarify to readers the main question/s posed by your study. Linked to this, in the discussion section it would be useful to read how these results from a short test of general physical and mental health will used to improve outcomes. In the abstract you refer to addressing all three types of variables ? please expand in the discussion and make this comment more clearly in the abstract. It seems that you are mainly directed to address the risk factors and variables underlying poor health than implementing treatments for poor health.

The starting point for this analysis was to view health broadly as overall physical, emotional and social well-being rather than only as specific health diseases and conditions. This broader conception of health has the advantage of capturing the multiple dimensions of health, which cannot be captured in disease or condition specific outcomes and measures. It can therefore complement the findings from studies of these more specific outcomes. We have added this explanation to the revised manuscript (page 3, paragraph 1).

We have adjusted the conclusion in the abstract and main manuscript to make it clearer and more specific to the study findings (please also see reviewer 1’s suggestion # 3 on this issue).

7. I am not qualified to comment on the sample size calculation and the sampling method in the context of the cluster design and suggest that if the editor wishes to get a further comment on this that he or she requests a statistical opinion.

It may be helpful to clarify that these are standardised and widely used methods for calculating survey sample size and for sampling methods, and references have been provided to support the selection of these methods (references 25-27).

8. It would be helpful if the authors could make clear in the section on analysis the differences between the separate and combined analysis as it seems in the separate analysis you are still adjusting for all other variables in the model?

We have clarified the differences between the separate analysis (where variables were only adjusted for the influence of the other variables within the same model) and the combined
analysis (where variables were adjusted for the influence of all the variables in all three models). Please see page 7, paragraph 2; and also in Table 2.

9. Convergent validity and divergent validity using item-test correlations and the internal features of a test are only limited ways to test construct validity. I think that the tests used for convergent validity here are essentially tests of internal consistency? As Cronbach has said, high internal consistency can in some circumstances even lower validity. It would be helpful to make it clear in the abstract and method the actual approach taken to assess convergent and discriminant validity and in the discussion to comment on the limitations of this as an approach to test construct validity, which is a broad issue usually requiring reference to other likely measures of a theoretical construct. Again, the statistician I am recommending to the editor is likely to comment also on this.

Thank you for this note of clarification. We have corrected this point and changed the terminology from construct validity to internal consistency (page 7, paragraph 3; and other relevant points in the manuscript). We have also clarified in the methods section why we chose not to use the more commonly used Cronbach’s alpha score which is that the SF-8 contains only a small number of items and these fairly heterogeneous items which cover the two different health domains of general physical and mental health (please see page x, paragraph x). We also agree that there can be problems with high internal consistency using Cronbach’s alpha, but our understanding is that these problems commonly relate to scales with many items which can misleadingly make a scale look more homogenous than it is. It has been noted that scales with 14 or more items are prone to this limitation (Cortina, J.M. (1993)., What is coefficient alpha? An examination of theory and applications. *Journal of Applied Psychology*, 78, 98-104). The SF-8 only consists of five items for the physical health scale and 3 items for the mental health scale and so should not be subject to this problem. We also appreciate that further work is required to validate the SF-8 in Southern Sudan and have strengthened our acknowledgement of this. We have noted that it would have been preferable to have validated the SF-8 against other likely measures of a theoretical construct such as using other validated instruments or clinical data. While this was not possible because of the absence of validated instruments in Southern Sudan and the limited resources available to collect clinical data in the study, we have recommended that further work should be conducted to validate health status instruments in Southern Sudan (please see page 12, paragraph 3).