Reviewer’s report

Title: Mediators of the relation between War Experiences and Suicidal Ideation among Former Child Soldiers in Northern Uganda: The WAYS Study

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Reviewer: Judith Bass

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Review of: Mediators of the relation between War Experiences and Suicidal Ideation among Former Child Soldiers in Northern Uganda: The WAYS Study.

The issue of suicidality is a major public health issue globally and this manuscript studies it in the context of prior war experiences and current challenges among a high risk sample in Uganda. While this is an important study, there are some methodological challenges that need to be addressed described below. Also, it is not clear whether the sample is formerly abducted children – which would include those who were made to be soldiers as well as those who may not have been soldiers – or only the subset of abducted children who were actively soldiers. If it includes abducted children more generally, then this should be clarified in the title of the manuscript.

Major Revisions:

- Introduction
  1. The statement in the introduction lines 9-10 needs clarification “This study will focus on suicidal ideation (SI) because all suicide attempts and completed suicides start as SI [7].” It is not clear that the citation provides ‘proof’ that all suicides start as ideation and I don’t think the literature holds that this is always the case, particularly in the case of manic episodes or impulsive suicidal actions. The authors could either clarify this further or could remove this statement as the subsequent statements already note why it is important to study suicidal ideation.
  2. The first 4 citations are all from studies related to South Africa. Given the statements in the introduction about suicide as a global issue, it would strengthen the manuscript to include citations from the global context such as the 2012 GBD study by Wang et al. in the Lancet and the very recent article by Mars et al. (2014) in BMC Public Health on Suicidal Behaviour across the African continent.
  3. The 3 objectives of this study need to be somewhat clarified in light of the cross-sectional nature of the data. Incidence is difficult to calculate with cross sectional data and causality implied in the third aim may not be possible. This is particularly important as while the WE items can be assumed to be in the past, both the SI and depression/anxiety items are assessed in the current context – and so directionality cannot be inferred.

- Methods
1. The description of the sample needs clarification. Specifically, what is meant by page 5, line 6: “the FCS are members of particular groups formed for easy access and social support”

2. Can the authors explain how they can assume the lists of FCS are comprehensive and accurate? (line 13 page 5). Did they formally check the accuracy and comprehensiveness or simply assume it? I could conceive or reasons that there may be discrepancies, including that FCS get services so people might want those services even if they are not FCS. And some FCS might not want to be identified. I do not think the authors can assume complete accuracy or comprehensiveness, and so should accept this possibility – although even having a large sample is quite good – it probably is not 100% complete.

3. Page 5 lines 20-21 notes that “The psychosocial outcomes between responders and non-responders at follow-up are discussed elsewhere in a Cohort profile of this group” – but the sample here is not those who weren’t followed up but rather those who weren’t assessed at baseline, so the differences between those who were and were not followed up is not relevant to this paper. What is relevant is whether the 17% that were invited but not assessed are different from those who were assessed at baseline. It would be helpful if the authors could describe the non-respondents a bit more.

4. Page 7 – the authors state that the APAI is a modified version of the AYPA but I do not think this is correct. The APAI is it’s own instrument specifically developed for use with adolescents in this context as per the citation Betancourt (2009).

5. The classification of WE is not clear. In the description of the WE measure it indicates there are 52 items, each measured as present/absent. But in the table there are only 12 items. Are these 12 items the grouped categories of the 52 items? How were they generated? If a respondent indicated yes to any of the items within a group was that category of WE considered present? This needs to be clarified in the methods.

6. How was the general war exposure variable calculated? Was it a sum of all 52 war exposure items? This is not described in the methods. If it is a sum of the 52-WE items, what is it’s distribution? It is used in the regression analyses as a simple scale, which assumes that it is normally distributed. Please clarify and if it is not normally distributed, it might need to be transformed for the analyses.

7. In the analysis process with the analysis that included all the types of WE concurrently, special care needs to be taken for potential multi-collinearity given that many of the respondents will have experienced multiple WE, so simply including them all in a model without first testing for multi-collinearity is not appropriate.

8. It is not clear in the methods whether clustering by sub-district or district level was done or even evaluated, but given the potential for variation, it is suggested to account for clustering in the regression analyses.

9. In the univariate analyses for WE on SI – it is not clear if any covariates are controlled for. Specifically, given the differences in SI by sex, I would think that all
analyses should control for sex. It may be also that length of time abducted and age at abduction are important factors. The author noted in the methods that these data were collected, but they are not described in the Table 1 demographics nor in any of the analyses. They should be included if the data is there.

- Results
1. Table 1 could be significantly clarified by removing the correlation among variables information – which is not relevant for a table simply describing the sample. This table should be re-formatted to a standard model simply describing the sample with binary variables (i.e. sex) presented as N (%) and scales presented as Mean (SD). For scale items, it would also be helpful to have the range for each scale. The basic analysis of whether SI is related to the sample characteristics can be presented in a separate table with regression coefficients, rather than correlation coefficients.

2. The results and tables will need to be revised based on the suggested methodological revisions.

- Discussion
1. The limitations section needs further expansion – related to the decision to cluster WE (i.e. not using all the 52 items); related to the gaps in data for other potentially important variables not included related to SI (i.e. social support); and to the multiple analyses conducted which may lead to finding significant results by chance.

- Minor Revisions:
1. Define SI prior to its first use as an acronym on line 8 page 3.
2. Lines 8 and 9 on page 4 seem to be a repetition of text earlier in the same paragraph and lines 15-17 seem to be a repetition of text in the previous paragraph – editing of these paragraphs on all the pathways from WE to SI would be helpful.
3. Page 5 line 15 – is the age requirement at time of abduction or at time of assessment? Also, please clarify if there were any exclusion criteria.
4. Page 7 line 20 – the authors note that the depression/anxiety scale is on a 1-4 scale and then define the response categories as 0-3. This needs clarification.
5. The results on page 9, paragraph 1 would be easier to understand in table format.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests