Author's response to reviews

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

Authors:

Kennedy Amone-P’Olak (kpamone@gmail.com)  
Tlholego M Lekhutlile (Tlholego.Lekhutlile@mopipi.ub.bw)  
Emilio Ovuga (emilio.ovuga@gmail.com)

Version: 3  Date: 22 August 2014

Author's response to reviews: see over
August 22, 2014

The Editor in Chief
BMC Psychiatry

Dear Sir/Madam,

MS: 1089182111125903 - Mediators of the relation between War Experiences and Suicidal Ideation among Former Child Soldiers in Northern Uganda: The WAYS Study

On behalf of my co-authors, I am pleased to resubmit our manuscript for possible publication in your esteemed journal, BMC Psychiatry. We are extremely grateful to the reviewers for their comments that have enabled us to improve our work to make it worthy of publication in your esteemed journal, BMC Psychiatry.

Our manuscript is a cross-sectional report based on a longitudinal research on the associations between different types of war experiences and suicidal ideation among former child soldiers in Northern Uganda. The manuscript examines whether different types of war experiences predict suicidal ideation differently and whether post-war hardships and symptoms of depression/anxiety explain current suicidal ideation. After discussion with co-authors, we carefully considered and agreed to resubmit the edited manuscript for publication in your journal, BMC Psychiatry.

We have now improved the manuscript in accordance with the reviewers’ suggestions and comments and clarified issues raised by the reviewers in relation to the introduction, methods, results, and analysis, checked all referencing, and gave the manuscript to a native speaker of the English language for careful editing.

We believe that the manuscript is now appropriate for publication by the BMC Psychiatry. Again, this manuscript has not been published and is not under consideration for publication elsewhere. We have no conflicts of interest to disclose. In the manuscript, the reviewers’ comments are in black texts, our responses in are in red texts, and suggested changes to the manuscript in blue texts.

We are optimistic that, this manuscript will be suitable for publication in your esteemed journal, BMC Psychiatry this time round.

Yours sincerely,

Dr. Kennedy Amone-P‘Olak, Ph.D.
Associate Professor, Department of Psychology
University of Botswana

www.ub.bw
Reviewer's report

REVIEWER # 1

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

Version: 2 Date: 4 July 2014

Reviewer: Judith Bass

Review of: Mediators of the relation between War Experiences and Suicidal Ideation among Former Child Soldiers in Northern Uganda: The WAYS Study. This is an important paper for the post-conflict and mental health field. The methodology is generally well described and the manuscript adheres to the relevant standards for reporting and data deposition. The discussion and conclusions are well balanced and adequately supported by the data. The article is also clearly written.

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.

We are grateful to the reviewer for requesting for clarity regarding our sample and the title of our manuscript. Nevertheless we are of the opinion that the title is clear and specific to “… former child soldiers in Northern Uganda” as clearly stated in the following title:

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

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.

We thank the reviewer for pointing this out. We have now removed this sentence altogether. The first paragraph has now been modified as follows:

Globally, suicide is not only a serious public health problem but is among three leading causes of death among 15 – 44 age bracket (WHO 2012). Annually, it is estimated that one million people die as a result of suicide (WHO 2012; Schlebusch, 2012; Obida, Govender, 2013) and it is projected that suicide will account for 2.4% of the total death burden by 2020 (Bertolote, 2009). In Africa, the annual incidence rate is estimated to be 3.2 per 100,000 people (Mars, Burrows, Hjelmeland, Gunnell, 2014). Suicidal behaviour is a process and it evolves through a continuum ranging from ideation (thoughts about suicide) to attempts (varying degrees of attempts to end one’s life), to completed suicide, i.e. successfully taking one’s life Meehan, Peirson, Fridjhon, 2007 [3]. Previous studies on suicide indicated that cases of completed suicides are higher in males while suicidal ideation (SI), attempted suicides, and suicidal threats are higher among females Madu, Matla, 2003; Roberts, Chen, Roberts, 1997; Nye, 2007 [4, 5, 6]. Focusing on SI is important
for identifying those at risk of suicide attempts and completion. In addition, preventive strategies have been suggested to be successful at this stage Schlebusch, 2012 [1].

There is a broad range of factors associated with SI. For example, mental health problems (e.g. depression), behavioural problems (e.g. drug and substance abuse), terminal illnesses and health conditions such as HIV/AIDS, and exposure to social conditions such as extreme poverty and previous traumatic life experiences such as war events, are known risk factors for SI Schlebusch, 2012; Meehan, Peirson, Fridjhon, 2007; Hawton, Van Heeringen, 1995; Schlebusch, 2005; Wasserman, 2001; Wasserman, Wasserman, 2009 [1, 3, 8, 9, 10, 11]. The current study explored factors that account for influence of war experiences (WE) on current SI among former child soldiers in Northern Uganda. Northern Uganda endured a long and brutal war for about two decades in which thousands were killed, the local economy ruined, social life and individual lives wrecked, thousands of children abducted, and over 90% of the population in war-affected region displaced (Coalition to Stop the Use of Child Soldiers, 2008; Amone-P’Olak, 2004; 2005; 2009). Studies on suicide in Northern Uganda have associated suicidal behaviours with alcohol (Kizza, Hjelmeland, Kinyanda, Knizek, 2012), negative life events (Kinyanda, Hjelmeland, Musisi, 2005), and despair (Kizza, Knizek, Kinyanda, Hjelmeland, 2012) in the aftermath of the war.

Additional references


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.

We have now included the following references to portray suicide as a global issue:


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.

We agree with the reviewer that we should have made a case for the temporal order of events that is a prerequisite for mediation model. We now make a case for temporal order and change the manuscript as follows in the introduction section of the manuscript:

Mediation analyses is appropriate for this study because the WAYS study assessed previous war experiences (more than six years ago), post-war difficulties and depression in the past year and current Suicidal Ideation. Consequently, the temporal order of events in this study (prerequisite for mediation analyses) meets the condition for a mediation analyses.

- 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.

We agree with the reviewer that we might not have been clear enough. We now add more information and clarify as follows:

The list compiled by UNICEF was previously used to allow formerly abducted children get assistance from NGOs, including UNICEF; given the importance of being on this list, it is assumed that the list is comprehensive and fairly accurate. Young people on this list were eligible to enter the study if they met the following inclusion criteria: 1) history of abduction by rebels, 2) lived in rebel captivity for at least 6 months; and 3) aged 18-25 years. Those who met the above inclusion criteria were invited through their local council leaders to participate in the study. In total 650 formerly abducted children were invited to participate and data was collected from 539 of them representing 83%. The cohort profile is described in detail elsewhere (Amone-P'Olak, Jones, Abbott, Meiser-Stedman, Ovuga, Croudace, 2013). Baseline data was collected between June 2011 and September 2011. The data presented in this paper are drawn from the baseline data.

3. Page 5 lines 20-21 notes that “The psychosocial outcome 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.

We now provide more information regarding the response rate as follows:

A total 650 participants were invited to participate in this study and data was collected from 539 of them representing 83%. Non-responders were either sick, attending to their sick children or had gone to attend to their farms. The psychosocial outcomes between responders and non-responders are discussed in a Cohort profile of this group (Amone-P’Olak, Jones, Abbott, Meiser-Stedman, Ovuga, Croudace, 2013) [41] In general, the difference between responders and non-responders was limited and may not have affected the outcome of the study as demonstrated in the cohort profile of the study populations (Amone-P’Olak, Jones, Abbott, Meiser-Stedman, Ovuga, Croudace, 2013).

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 its own instrument specifically developed for use with adolescents in this context as per the citation Betancourt (2009).

We regret this incorrect information and now correct and change the manuscript as follows:


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 its 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.

It is possible that WE measure was not adequately described. We now further describe it in the manuscript as follows:

Consequently, the WE measure consisted of 52 items from which 12 types of war events were derived. The 52 items were summed up for occurrence to generate general exposure to WE. The composite general war exposure (WE) was not normally distributed and was transformed by computing its square roots before inclusion into 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.

Indeed, we considered the potential for multi-collinearity in our analyses although we did not include it in the method section. We now include this information in the method and result sections as follows:

Method under statistical analyses:

Given that many of the respondents multiple WE, the potential for multi-collinearity was assessed as the variance inflation factor (VIF) (Glantz, Slinker, 1990). A VIF >10 indicates serious multi-collinearity and values >4.0 may be a reason for concern.


Result:

In this study, the VIF were all less than 3.0, indicating that multi-collinearity is not so much of an issue.

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.

Indeed we clustered by district and accounted for clustering in the regression analyses. We now include this information in the method section as follows:

Sampling:
A cluster sampling technique was used to recruit participants in the study from a list compiled by UNICEF.

Statistical analyses:

In addition, the current study employed cluster sampling to recruit participants in the study. Given the potential for variation by sub-counties from which the participants were sampled, we accounted for clustering by including it in our 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.

In the manuscript, we indicated clearly under statistical analyses (third last sentence that we controlled for gender. In preliminary analyses, duration in captivity and age at abduction did not significantly predict the outcome variables and were subsequently dropped from further analyses. We now include this information in the method section of the manuscript as follows:

Duration in captivity and age at abduction did not significantly predict the outcome variables and were subsequently dropped from further analyses.

- 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.

We are of the opinion that table 1 is clear and gives all the basic information required to further evaluate the manuscript. We think that it should be left as it is currently formatted.

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

Ok

- 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 includes related to SI (i.e. social support); and to the multiple analyses conducted which may lead to finding significant results by chance.

In the univariable and multivariable analyses, we did not use all the items of the war events questionnaire. However, in both mediation analyses, the general war exposure consisted of all the 52 items in the war events questionnaire. We now include this in the limitations as follows:

Finally, due to multiple analyses of different types of war events, chance findings are possible. However, in both mediation analyses, the general war exposure consisted of all the 52 items in the war events questionnaire. Likewise, in this study, the VIF were all less than 3.0, indicating that multi-collinearity is not so much of an issue. Finally, potentially important variables for determining suicide such as social support were not assessed at baseline and follow-up. These important variables will be assessed in the next wave of data collection.

- Minor Revisions:

1. Define SI prior to its first use as an acronym on line 8 page 3.

We regret this error and correct it forthwith.
Previous studies on suicide indicated that cases of completed suicides are higher in males while suicidal ideation (SI), attempted suicides and suicidal threats are higher among females [4, 5, 6].

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.

We are sorry for this repetition. We have now revised the subsequent sentence as follows:

In a previous study based on the same population and data, exposure to previous WE was associated with post-war hardships, which in turn, were linked to mental health problems including depression and anxiety [27].

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.

Both age at abduction and baseline were assessed. Age at baseline was used as an exclusion criterion. We now clarify this in the manuscript as follows:

Recruitment of participants for the study were based on the following criteria: (1) history of abduction by rebels, 2) having lived in rebel captivity for at least 6 months, and 3) aged between 18-25 years at baseline.

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.

We are sorry for this mistake. We now correct it in the manuscript as follows:

For each question responses were scored on a 0–3 scale, ranging from 0 = never, 1 = rarely, 2 = sometimes, and 3 = always.

5. The results on page 9, paragraph 1 would be easier to understand in table format.

We now tabulate the information in the manuscript as follows:

Table 2: Descriptive statistics: predictors in the study stratified by suicidal ideation and gender

<table>
<thead>
<tr>
<th>Predictors</th>
<th>Suicidal ideation (mean, SD)</th>
<th>No suicidal ideation (mean, SD)</th>
<th>t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td>War experiences</td>
<td>45.63 (5.94)</td>
<td>40.98 (7.70)</td>
<td>$t (537) = -5.56, p &lt; .001$</td>
</tr>
<tr>
<td>Perceived post-war hardships</td>
<td>11.58 (4.38)</td>
<td>15.50 (5.69)</td>
<td>$t (537) = -7.80, p &lt; .001$</td>
</tr>
<tr>
<td>Symptoms of depression</td>
<td>18.01 (8.60)</td>
<td>32.05 (8.72)</td>
<td>$t (537) = -15.67, p &lt; .001$</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Male (mean, SD)</th>
<th>Female (mean, SD)</th>
<th>t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td>War experiences</td>
<td>42.64 (7.31)</td>
<td>41.39 (8.87)</td>
<td>ns</td>
</tr>
<tr>
<td>Perceived post-war hardships</td>
<td>6.67 (3.24)</td>
<td>6.75 (3.50)</td>
<td>ns</td>
</tr>
<tr>
<td>Symptoms of depression</td>
<td>19.20 (9.91)</td>
<td>24.74 (10.49)</td>
<td>$t (537) = -6.03, p &lt; .001$</td>
</tr>
<tr>
<td>Suicidal ideation</td>
<td>.19 (.40)</td>
<td>.31 (.46)</td>
<td>$t (537) = -3.07, p &lt; .001$</td>
</tr>
</tbody>
</table>
This is an important paper for the post-conflict and mental health field. The methodology is generally well described and the manuscript adheres to the relevant standards for reporting and data deposition. The discussion and conclusions are well balanced and adequately supported by the data. The article is also clearly written.

Major Compulsory Revisions

1. The authors stated that there was evidence of partial mediation, after removing anxiety items from the analysis. It would make sense to present this as a separate outcome - for depression, anxiety, and depression/anxiety, as suicidal ideation is arguably more related to depression. Alternatively, the authors may consider focusing on depression alone.

   In APAI, depression and anxiety (18 items) were combined into one scale. Previous studies also showed a strong overlap among items in the depression and anxiety subscales (Brodbeck, Abbott, Goodyer, Croudace, 2011).


2. I wonder if the authors collected any data on PTSD. It looks like the anxiety subscale taps into the more generic anxiety-related symptoms.

   Data on PTSD will be collected in the subsequent wave of data collection.

3. how the items are analyzed should be clarified, whether as continuous variables or.

   The predictors and outcomes were analyzed as continuous variables. We now clarify this in the manuscript as follows:

   Except for SI and gender, WE, postwar hardships, and depression were analysed as continuous variables.

4. Building on model 1 (figure 1) I wonder if the authors looked at depression, anxiety as separate outcomes in addition to suicidal ideation?

   Although depression and anxiety symptoms commonly co-occur the questionnaire items that assessed depression and anxiety psychopathology were mixed together in one scale for common
mental health problems, preventing them being considered separately as distinct outcomes. Previous studies also showed a strong overlap among items in the depression and anxiety subscales (Brodbeck, Abbott, Goodyer, Croudace, 2011).

We now modify the manuscript as follows to reflect this:

In APAI, depression and anxiety (18 items) were combined into one scale. Previous studies also showed a strong overlap among items in the depression and anxiety subscales (Brodbeck, Abbott, Goodyer, Croudace, 2011).


5. the authors stated that gender was a confounding variable and thus included in the models. Please provide more information on how this was done in the analysis. Could gender play a moderating role in the mediated relationships amongst war experiences, post-war hardships, depression/anxiety, suicidal ideation?

It is possible that gender could play a moderating role in the mediated relationships amongst war experiences, post-war hardships, depression/anxiety, and suicidal ideation. However, our interest in this article was to show the extent to which depression and post-war hardships account for the influence of war experiences on suicidal ideation and not so much on gender differences or moderation by gender.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests: I have no competing interests. I would like to acknowledge Kuowei Tay the statistician in our unit for reading and commenting on the statistics in this paper.