Author's response to reviews

Title: Returning Home: Forced Conscript, Reintegration, and Mental Health Status of Former Abductees of the Lord's Resistance Army in Northern Uganda

Authors:

Phuong N Pham (ppham1@berkeley.edu)
Patrick Vinck (pvinck@berkeley.edu)
Eric Stover (stovere@berkeley.edu)

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Author's response to reviews: see over
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Sabina Alam, PhD
In-House Editor
Tel: +44 (0)20 7631 9921
Facsimile: +44 (0)20 7631 9923
e-mail: editorial@biomedcentral.com
Web: http://www.biomedcentral.com/

Re: Response to Reviewer’s report

Original Title: A Cross-sectional Study of Factors Associated with the Social Well-Being and Mental-Health Status of Former Abductees of the Lord's Resistance Army in Northern Uganda

New Title: Returning Home: Forced Conscription, Reintegration, and Mental Health Status of Former Abductees of the Lord’s Resistance Army in Northern Uganda

Dear Dr. Alam:

Thank you for considering our research article. We made changes and edits throughout the word documents. Per your question, we uploaded the questionnaire as supplemental document on the BioMed Central webpage. Please note that we collected our data using a PDA (e.g., each question had its separate screen) and hence we did not focus on finalizing the formatting of the questionnaire that we attached. In addition, below are detailed responses to the two reviewers’ questions. Please do not hesitate to contact us if you have any question.

**Response to Reviewer 1 (Verena Ertl)**

**General:**

1. The title claims to study factors associated with the social well-being and mental-health status of former abductees, yet only depression and PTSD appear as dependent variables in the results section. Later the authors state their aim: “…to examine the types of violence to which former abductees have been exposed while being held captive, the extent to which it may have affected their psychological and social well-being, and the network of social support available to them.” I suggest to either run a model with social well-being as the dependent variable (as far as it makes sense) or to rephrase the title of the article.

The title was edited to “Abducted and Returned: Association between Experience of Abduction, Social Relations, and Mental Health Status among Former Abductees of the Lord’s Resistance Army in Northern Uganda”. The text was edited to remove references to social well being.

2. The terms „psycho-social“, „social well-being“ are typically used in many ways, especially in the field of development aid, which makes their meanings rather „fuzzy“. They are repeatedly used in the manuscript and need proper definition. It seems that „social well-being“ here, is defined only
by the respondents’ answers to 3 items exploring the perceived quality of the relationship to family, friends and community.

The text was edited to remove references to social well being except where it was a reference to the literature. The term “social relationship” was used where appropriate to reflect the 3-items scale that was used to measure that concept.

3. In the methods section of the abstract (page 2) the authors claim to perform multivariate linear and logistic regressions, yet only logistic regressions were performed.

The reference to linear regression was deleted.

4. In the results section of the abstract (page 2) the authors mention “older male” as a risk factor for Depression, yet in the results section age is not reported as a risk factor. Probably the authors meant „older age of males at the time of abduction“.

The text was edited.

5. In the conclusion section of the abstract (page 2) the authors claim that abduction and forced conscription of civilians seem to have a major impact on the social and psychological well-being of residents living in northern Uganda. I don’t consider the statement “impact on the social well-being” sufficiently supported by the data presented by the authors. On the contrary the means reported on the 3-item “relationship with family and community scale“ for abductees (12.26) and non-abductees (12.01) don’t seem to differ significantly.

The reference to social-well being was deleted.

Methods:

6. The authors put a lot of effort into thorough sampling and can therefore base their findings on a large representative (for the northern Ugandan regions) sample. The progressive equipment (PDAs with GPS) used for data collection promises high data quality. Still there might be a slight bias due to the high rates (more than 23%) of missed respondents, supposing that those who were missed were the most active and the most functional individuals of the communities. But this is not a main concern, my main concern is that the quality of the data and therefore the stated rates of PTSD and Depression have to be questioned and handled with care, since I consider the reliability and validity testing of the used instruments as not sufficient. The PCL and the HSCL may have been used in different settings and also found reliable and valid there, but could be of limited use in northern Uganda. It seems the authors didn’t put their Luo-Versions of the PCL and the HSCL to the test. The reported Cronbach’s alphas as the only measures of reliability, which are easily reaching high values are not convincing. The cited other study published by one of the authors also doesn’t give more information on validity and reliability of the Luo-Instrument-Versions. The authors mention that the PCL has been correlated with the CAPS. In case they are talking about a correlation derived from data of the present study they should report those figures. In case no further reliability and validity testing has been carried out I would recommend to be very careful with the use of symptom scores for reaching diagnoses of PTSD and Depression and with the interpretation of the results in general. In this case I would also recommend not to use the diagnoses of PTSD and Depression as dependent variables of logistic regression models, but to rather use linear regression on Symptom scores.
We agree with the reviewer comments, in this paper to state we referred the outcome variables as symptoms of PTSD and depression and not diagnoses of them. We conducted extensive background research prior to selecting the PCL-C as our measurement instrument. At the inception of the study we collaborated with a psychologist who was working in one the treatment clinic. Based on his clinical work, we felt confidence that we could use the PCL an indicator scale. PCL-C has one item anchored to each of the seventeen key symptoms required for DSM-IV TR diagnosis of PTSD. The PCL-C has been shown in a wide variety of research with survivors of diverse types of trauma to have excellent internal reliability, and high convergent validity. (references below)

In addition, because of the public health implication for this study, logistic regressions analysis provides a better way to interpret the findings. The purpose of this paper is not to be clinically precise (i.e., coming up with a predictive model), but to obtain a greater understanding of factors that are associated with mental health. Logistic regression allows one to compute an odds ratio, an estimate of relative risk, and it is easier to interpret for practitioners.


7. The authors should clarify why they only used a cutoff of 44 as diagnostic criterion for PTSD. In the literature different scoring rules can be found, e.g. cutoffs reaching from 44 to 50 and more sophisticated mixed scoring approaches (e.g. Ruggiero et al. 2003).

There are several scoring approaches discussed in the literature. Blanchard et al., for example, found that a score of 44 (rather than 50) maximized diagnostic efficiency. In addition, the cutoff of 44 provides comparability with earlier studies by the authors in Uganda, Rwanda and elsewhere.


8. Was a measure of impaired functioning included? How was the A criterion assessed?

Impaired function was not included in the study. We believe was not necessary for the purpose of this study. Criterion A was assessed as listed in Table 2: Exposure to Traumatic Events and War Crimes.

9. Moreover the authors should clarify why they chose to use a cutoff of 42 for the HSCL. This cutoff strikes me as very conservative for the 15 depression items of the scale. Other scoring options have been suggested since Parloff et al. 1954. Bolton suggests an algorithm in his papers and Roberts et al., 2008 recently used the cutoff of 1.75 in Uganda.

The aim was to obtain the most conservative estimate as the current percentage of respondents indicating symptoms of depression is relative high.

10. Another concern I have considering the quality of the data on psychopathology in this paper is the extremely short training period for interviewers. Learning sophisticated sampling techniques, interviewing techniques, handling of the PDAs and additionally understanding the rationale of diagnosing PTSD and Depression symptoms and applying the diagnostic knowledge correctly in the own language and culture seems quite impossible to me.
All the interviewers were recruited among university students or graduates. The selected individuals had experience conducting survey work for various agencies working in northern Uganda. As a result they were familiar with the standard sampling and interview techniques used for this survey. Nevertheless, those topics were included in the 7-days of training.

Being familiar with computers, the use of PDA did not represent a difficulty, but in fact facilitated the training since it reduces greatly the risk of errors compared to a paper version (for example, invalid entries are rejected, and skipping patterns are automatic). However, it should be noted that the PDA training took place over a 3 days period in addition to the 7-days of training on the questionnaire.

Several pilot surveys were conducted in real-life context (i.e. in non-sampled camps) to ensure that all the procedures were understood and appropriately implemented. During data collection, teams moved together to a sampled location and were always joined by a UC Berkeley researcher. The researcher would ensure that the appropriate procedures are followed.

Finally, in addition to in-depth discussion and training on the questionnaire, a significant amount of time was dedicated to the development and translation of the questionnaire to ensure that the instrument used language that was simple and easily understandable to both the interviewer and the interviewed.

The text was edited to provide more information on the training.

Comments:

11. The authors state that the mean age of those reporting abduction at the time of the survey (June 2007) was 35.3 (S.D. 13.51) and the average age at first abduction was 25.8 (S.D.13.57). Statistics (Blattman et al. 2007) about abductions in northern Uganda show that the largest group of children abducted (in all the years between 1989 to 2004) were clearly younger at the time of their abduction than those in the present study (between 12 to 15 years of age). Moreover the older youths were at time of abduction the less time they spent with the rebels. This means that due to the constraint of interviewing participants above 18 years of age the results of the present survey can’t be considered representative for all the formerly abducted in northern Uganda. They are representative for “older-aged” abductees who most likely spent less time in the bush than younger abductees. This fact should be mentioned by the authors and considered when it comes to the interpretation of results.

The work of Blattman et al. is based on eight sub-counties in Kitgum and Pader. It also focuses on youth population (below 30). This leaves out population living outside of the selected areas in Kitgum and Pader as well as older population. The sampling universe for the present study and that of Blattman are therefore very different. For this survey, since abduction started in the 90’s the sampling universe includes individuals who were children at the time of abduction. The text was edited to emphasize that the sampling universe consist of adults aged 18 or more.

12. In general the discussion of the results could be more extensive: e.g. the finding that those who had better relationships with their family, friends and community were less likely to have symptoms of depression, but there was no association with PTSD is very interesting. Does this mean PTSD is rather independent from social factors? This finding leads the authors to stress the value of reintegrating former abductees with their families and developing programs to help them move ahead economically and socially. I agree that family and community acceptance seem to be very important,
but what about the finding concerning PTSD? Maybe PTSD needs to be dealt with differently, e.g. psychological treatment and social support.

A sentence was added to highlight the finding in the discussion.

13. Cumulative Exposure to 4 categories of different event types are some of the core measures of the present study. One category is introduced as “secondary trauma”, it seems it didn’t qualify as a risk factor and was excluded from the regression model, yet I think this should be mentioned when discussing the results. The same holds true for a more extensive discussion of other variables (abduction time, stay in reception centre)

The text was revised to provide additional discussion.

Limitations:

14. The authors claim that the seven days training of interviewers was a means to minimize social desirability and recall errors in the respondents’ answers. I don’t understand this without further explanation.

Additional details are provided in the text.

Conclusions:

15. I would not recommend talking about prevalence rates of PTSD and Depression when reliability and validity of the diagnoses is not sufficiently assessed. Terms like symptoms of PTSD could be used instead or at least stress this point clearly in the limitations section.

The text was edited to systematically use “symptoms of PTSD” and “symptoms of depression” where it was not already done.

16. The authors claim that those who reported having better relationships with their family friends and community were less likely to report symptoms of PTSD and/or depression. This statement is not supported by the authors’ own data regarding PTSD, since no association was found.

The text was edited.

Tables:

17. Table 1: Table heading „by length of abduction“ is not really what is presented in the table, probably the authors meant simply „by abduction“.

Heading was edited as followed: Socio-Demographic Profile of Respondents by Abduction Status

18. Table 2: Reporting the p-values for group comparisons also in the table would be helpful for the reader.

We added p-values for group comparisons for cumulative exposure scores.
In order to assess symptoms of PTSD as a dichotomous scale. We treated “moderately” or above (3 thru 5) as symptomatic and follow the DSM scoring rules to consider the three clusters: (i.e., 1 Cluster B item (questions 1 thru 5; reexperiencing), 3 cluster C items (questions 6 thru 12; avoidance), and 2 cluster D items (questions 13-17; hyperarousal).

20. Table 5: For „cumulative general traumatic exposure“ figures/or NS are missing in the PTSD column. For the sake of consistency the non-significant p-values in the rows Acholi/Langi and Acholi/Others should be changed to NS.

The Table was edited for general traumatic exposure. For the ethnicity, because the variable used is categorical (nominal) all comparison levels are included in the model, even if non-significant. This standard reporting when the variable is significant but the specific level comparison within the variable is not.

21. On page 10 (second last line) the authors write PTSD but mean Depression.

The text was edited

Response to Reviewer 2 (Emilio Ovuga)

Title:

1. The title that appropriately describes the content of the paper would be: "The Social Well-Being and Mental Health Status of Former Abductees of the Lord's Resistance Army in Northern Uganda".

The title was edited based on comments from reviewers 1 and 2

Abstract:

2. While the types of violence that former abductees experienced was the main subject of the survey; the authors did not include this in the abstract. The abstract is wordy and should be shortened by half without loss of the message of the paper. Examples of how to achieve this are illustrated throughout the paper with strikethroughs of words of whole sentences that are inappropriate or unnecessary.

We did not receive the edited copy of reviewer 2 but edited the text to take into account the comment.

Text:

3. While describing the mental health status of the respondents, it is more appropriate to refer to "probable post-traumatic stress disorder (PTSD)" and "depressed mood" as the instruments (scales) employed in the study were designed to screen and not to make clinical diagnoses. In line with this critique the words probable PTSD and depressed mood are suggested instead of PTSD and depression respectively in the review of the paper.
The text was edited. The words “symptoms of PTSD” and “symptoms of depression” were systematically used.

Bias:

4. The researchers introduced a serious systematic bias without telling the reader the female to male ratio of interviewers on the assumption that women needed to be protected from sensitive issues. However Ugandan females tend to confide in and disclose more intimate information to males who are sensitive to issues of human sexuality.

To address the sensitivity of some of the questions and after careful discussion with local experts, it was decided to assign interviewers to same sex respondents – men interviewed men and women interviewed women. Teams of interviewers were composed of the same number of women and men. The text was edited to better reflect the gender ratio of the interview teams.

5. A second bias was the inclusion by the researchers of 12 respondents whose forms were “incomplete”; and no further information is provided to justify the inclusion of the 12, or the anticipated magnitude of error in the content of the paper and how they minimized this.

Respondents were given the possibility to stop the interview at any time. A total of 12 interviews were not completed because the respondents chose to stop the interview. Those records were included in the sample since they provided responses to at least part of the questionnaire. The 12 questionnaire represent less than 0.5% of the total sample and are unlikely to have had any impact on the results. In addition, the researcher had no rationale to exclude those data (i.e., including them did not change the interpretation of the data).

6. The authors should also tell the reader the inter-rater reliability of data that the several interviewers gathered. The explanation that the researchers used the same instruments before in previous research in Uganda is not a justifiable assumption that systematic errors and bias are avoided.

During the training of the survey, we conducted several exercises with the respondents to ensure that for a given response, separated interviewers would code the same way. During this training exercise, the inter-rater reliability ranged from 90-95%. During the survey, we had one interview interviewing one respondent so inter-rater reliability could not be assessed. We estimated Cronbach’s alpha reliability measure for each scale.

Ethical clearance:

7. It appears that the Uganda National Council for Science and Technology (UNCST) did not grant national clearance for the researchers to carry out this community study. It is not sufficient for local governments of respective districts to grant permission as many of the officials that might have been approached lacked the necessary knowledge, skills and experience to evaluate ethical issues involved in research in northern Uganda communities affected by war. Local governments are only part of the series of agencies for researchers to approach; ordinarily it is the UNCST that introduces researchers to local government officials where community-based research is to be conducted anywhere in Uganda.

The authors conducted a similar study in collaboration with Makerere University in 2005 and were not informed at that time of the role of UNCST. Human Subjects Review Boards from Tulane University and UC Berkeley approved of the study and all steps were taken to ensure that local
permissions were obtained to carry out the study. No officials mentioned the role of UNCST. The researchers however understand that such authorizations do not replace the review by UNCST and take good note of the role of the UNCST for future studies.

Comments:

8. The findings of authors are similar to the observations of Ovuga et al [1] among former child soldiers at a primary boarding and rehabilitation school in northern Uganda. Children who reported more experience of traumatic events while in rebel captivity were more likely to report symptoms of depressed mood, as were male and older children in the rehabilitation section of the school, and those who returned to their communities via any of the several reception centers that were established to enhance their reintegration into their respective communities; the staff of most reception centers had no specific training to recognize, assess and diagnose clinical depression and post-traumatic stress disorder, and refer them for specialized mental health care. The implications of these two reports appear obvious; the reintegration and community-led mental services in northern Uganda require an integrated and comprehensive psychological, mental health and social services delivery that involve communities, political leaders, traditional social structures and mental health care providers in the reintegration of former combatants from both sides of the conflict into their communities.

At the time of writing of the article, the referred reference was not yet published. However, we added the reference in this revised version.

Limitations:

9. While the authors acknowledge limitations that might affect the quality of their findings, the report provides useful information on the quality of life and experiences of former combatants in any armed conflict, organized violence, political terror, war and domestic violence. The interested reader will find useful account for more information in Herman [2].

We agree with the reviewer that Herman is a good reference and have previously referenced it in our work. However, we do not see any appropriate place to reference this in the current article.

Recommendations:

10. Despite the non-approval of this research by the UNCST, I recommend the paper for publication because of the potential value it has for improving the care and reintegration of former northern Uganda combatants back into their communities. The authors should, however, respond adequately and satisfactorily to comments and queries that have risen in the paper and be prepared to conduct of future research in Uganda after the UNCST has cleared them. I recommend that the authors shorten the paper as illustrated by the strikethroughs and deletions.

We thank the reviewers and the editorial board for considering our paper. Please do not hesitate to contact us if you have further questions or need more editing.

Sincerely,

Phuong Pham, Ph.D.