Reviewer’s report

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

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Reviewer: Verena Ertl

Reviewer’s report:

Thank you for asking me to review this interesting paper. The manuscript presents data on the mental-health status of former abductees of the Lord’s Resistance Army in northern Uganda and explores factors that may have increased the risk of former abductees to develop symptoms of PTSD and Depression. Results showed that being female, cumulative number of traumatic events witnessed, cumulative number of forced acts of violence and reported difficulties coming home to the community were associated with increased risk of developing PTSD. Being an older boy at the time of abduction, a low score on perceived quality of social relationship, the cumulative number of general traumatic event exposure, the number of forced acts of violence and reported problems when returning to the community were associated with increased risk of developing Depression. Being an older female at the time of abduction and a high score on perceived quality of social relationship suggest themselves as protective factors. This is a solidly done study and I consider it a relevant contribution to the field, especially since there is not much scientifically sound literature concerning the mental health status of formerly abducted yet.

Nevertheless, there are also a number of concerns, which the authors should address before publication is warranted.

{Minor Essential Revisions}

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.

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.
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.

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“.

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.

{Major Compulsory Revisions}

6) methods:

   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.

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

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.

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.

{Minor Essential Revisions}

7) comments:

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” abducted 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.

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.

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)

{Major Compulsory Revisions}
8) limitations:
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.

9) conclusions:
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 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.

{Minor Essential Revisions}
10) tables:
Table 1: Table heading „by length of abduction“ is not really what is presented in the table, probably the authors meant simply „by abduction“.
Table 2: Reporting the p-values for group comparisons also in the table would be helpful for the reader.
Table 4: The authors used a cutoff-score of 44 on the PCL as criterion for PTSD, but what criteria where used to consider the clusters reexperiencing, avoidance, and hyperarousal as fulfilled?
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.

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

Level of interest: An article of importance in its field

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

Statistical review: No, the manuscript does not need to be seen by a statistician.

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