Reviewer's report

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

Version: 2 Date: 8 March 2009

Reviewer: Verena Ertl

Reviewer's report:

I refer only to comments of the authors I think they still should work on. All other comments from my side in the original review have been sufficiently addressed in the revised article version.

I consider all following statements from my side as minor essential revisions the authors should still address and I trust they will do so.

Adding the missing validation of the HSCL and the PCL-C in the limitations section should be considered a major compulsory revision.

original review comment:

1. 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 where 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.
answer of authors:
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)


answer to author comments:
The authors now correctly mainly refer to sympotms of PTSD and symptoms as depression as outcome variables and not diagnoses, yet in some few occations it has been overlooked to change the wording. E.g. on page 12 at the bottom the wording needs to be still changed to “criteria for symptoms of PTSD” and “criteria for symptoms of depression”, on page 16 (last line) the authors still state “the prevalence of depression was also high in our sample” and the heading of Table 5 also still states “Associations of Socio-demographic and Exposure Characteristics with PTSD and Depression” instead of symptoms of them.

In my review I do not claim that the PCL-C is an instrument you cannot use in cross-cultural settings. This is not my point of concern and I am sure the authors put enough thought into choosing the instrument and I acknowledge their consultation with a psychologist from a treatment clinic. The PCL-C has indeed been used in several settings and surely is a suitable instrument. The cited validity-study by Ruggiero et al. 2003 was conducted among 392 college students in the USA, yet the authors don’t cite validation studies reporting reliability or validity testing of the PCL-C in Uganda or East Africa because there is none. I think the authors have to just admit this fact in the limitations sections instead of relying on face validity only (page 7 at the bottom) “the questionnaire was […] validated in non-participating sites and mock interviews…” without refering to data (e.g. test-retestreliability, convergent and discriminant validity, etc.). The article will still be of value.
I am especially stressing this point since the authors insist on using logistic regression analyses instead of linear regression models with symptom scores (i.e. symptom severity) as outcome variables which would be more correct considering the lack of a validation study.

author-comments:
2. 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.

answer to author-comments:
I am fine with the authors using logistic regression to make results easier to interpret for practitioners, but then the above line of argumentation has to be stated in the article (that the purpose of the paper is not to be clinically precise and logistic regressions have been used despite diagnoses are not actually appropriate to make results easier to interpret).

original review comment:
3. Was a measure of impaired functioning included? How was the A criterion assessed?

author-reply:
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.

answer to author-comments:
I don’t understand why measuring impaired functioning should not be necessary for the purpose of the study. The title of the study “Returning Home: Forced Conscription, Reintegration, and Mental Health Status of Former Abductees of the Lord’s Resistance Army in Northern Uganda” implies that statements about the mental health status of former abductees will be made. Impaired functioning is a key criterion for every mental health disorder and the presence of functional impairment is as important as the presence of a special pattern of symptoms. Moreover I suspect that the level of functional impairment due to symptoms of depression and PTSD plays a key role in successful reintegration. Criterion A is not just about whether exposure to traumatic events and war crimes was present. Criterion A has an objective and a subjective part. The
The objective part states: The person experienced, witnessed, or was confronted with an event or events that involved actual or threatened death or serious injury, or a threat to the physical integrity of self or others. The subjective part states: The person’s response involved intense fear, helplessness, or horror. For some of the events chosen by the authors both, the objective and the subjective part are highly questionable. Consequently calling some of the listed events traumatic events is questionable. This is especially true for: “was displaced”, “lost income due to conflict”, “house destroyed”, “lost productive assets”. Especially these events should be rather called Macro-stressors than traumatic events. For all the other events it is more correct to state that they are potentially traumatic events if the A criterion was not checked properly.

In summary I suggest that the authors should mention the missing assessment of functional impairment and the “rough” assessment of the A Criterion in the limitations section.

original review comment:

4. 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.

author-reply:

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

comment to author-reply:

I agree that using a conservative estimate is probably appropriate for this study, yet the choice of certain cutoff scores still remains arbitrary without thorough validity testing. Theoretically it could be possible that the percentage of respondents having symptoms of depression is higher or lower than indicated, depending on the quality of the instrument and/or knowledge of the interviewers. A sample derived cutoff would have solved this problem. As stated above (for the PCL-C) this should be mentioned in the limitations section.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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