Reviewer's report

Title: Reintegration of child soldiers in Burundi: A tracer study

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Reviewer: Rebecca Horn

Reviewer's report:

Firstly, I’d like to say that I greatly enjoyed reading this paper, and it addresses an important issue. As the authors state in the introduction, a great deal of resources are invested in the reintegration of child soldiers, and there is a requirement to ensure that people really are benefiting from this input.

Major Compulsory Revisions

The author must respond to these before a decision on publication can be reached. For example, additional necessary experiments or controls, statistical mistakes, errors in interpretation.

1. The authors identify the main limitations of the research design, but I feel they acknowledge them and then move on, without really dealing with the impact these limitations are likely to have on the data obtained, and the conclusions that can be reached.

The issue of recall is crucial. The problems associated with asking people to recall events and situations are summarised concisely in Streiner & Norman (2008, p105-6) (Health Measurement Scales: A practical guide to their development and use Oxford University Press) - there must be considerable doubt about the accuracy of retrospective assessments. I agree with the authors that in situations where there is no baseline data, a retrospective assessment is probably better than nothing, but only if it is presented with the very clear caveat that any conclusions based on this data must be extremely tentative.

We could have more confidence in the findings if there were a comparison group who did not participate in a reintegration programme, since we would expect similar errors of recall to affect both groups. In the absence of a comparison group, we don’t know whether the changes identified are due to (a) real changes in the circumstances and wellbeing of programme participants (both child soldiers and others) due to participation in the programme; (b) real changes in the circumstances and wellbeing of programme participants due to factors other than the programme; or (c) errors of recall.

I realise that there isn’t much the authors can do about this at this stage, and the data they have is certainly worth publishing. However, I feel there needs to be:

a. More clarity about what the authors aim to do in this study. There are currently multiple statements of aims, some of which can be achieved and some of which cannot.

i. At the end of the 3rd para under ‘Background’, they imply that this study will
identify factors that play a role in the differential post-conscription outcome among former child soldiers (which it doesn’t – except for gender differences).

ii. At the end of the following paragraph they state that this study aims to increase the evidence around the impact of reintegration programmes on former child soldiers. This study can’t draw any conclusions about the impact of the reintegration programme – it wasn’t designed in a way that allows this.

iii. At the beginning of the next-but-one paragraph, it says that the study ‘aimed to assess reintegration trajectories ... ‘. This seems the clearest statement of aims, and I suggest that this statement is made more prominently, and the earlier references to aims are omitted/ reworded, to avoid confusion.

iv. Later in the same paragraph, it says ‘The aim of the study is to contribute to a better understanding of the factors that contribute to, or hamper, successful reintegration.’ I’m not sure the research design really allows this to be explored, or only in a very limited way.

b. More open discussion about the ways in which the limitations (the retrospective nature of the study; lack of a comparison group – 8th para of ‘discussion’ section) may affect the data and the conclusions which can be drawn. This seems essential to me. Currently the limitations are briefly mentioned, and then put to one side, and conclusions drawn anyway.

I also have some concern about the brief mention given to the lack of information about the validity of the instruments with this population. Again, this seems to me to be a significant limitation which can’t be referred to and then dismissed, and conclusions drawn as if they are known to be valid.

2. I got rather confused when reading the ‘analyses’ section. The descriptions of the different analysis make sense, but they all run into each other in one long paragraph. Readers would be assisted by some more signposting. For example, six lines before the end of the paragraph, the authors describe a new analysis – this could go on a new paragraph. This section could be structured more clearly.

3. In the 9th para of the discussion section, the authors state that since programme satisfaction is the stronger predictor of outcomes, one could argue for increased participation of beneficiaries in programme design. Whilst this makes sense, I also wonder whether it could be the other way round – those who benefit most from the programme, and who experience the most positive change in their lives, are likely to be those who are more satisfied with the programme.

Minor Essential Revisions

The author can be trusted to make these. For example, missing labels on figures, the wrong use of a term, spelling mistakes.

4. The authors’ first mention of how they are defining ‘child soldier’ comes under ‘method/participants’. It seems to me that it could come earlier in the paper, towards the beginning of the introduction.

5. Similarly, a description of the reintegration support programme (currently after ‘methods/procedure’) would be better presented earlier – either at the end of the introduction or beginning of the methods section. This would help the reader to understand the context within which the study was conducted.
6. The location of Table 1 (under ‘Results’) hasn’t been indicated.

7. In the second paragraph under ‘Results’, it isn’t clear whether a higher score on the 7-35 point scale indicates more impairment or better functioning. I think it means more impairment, but it would be helpful to clarify this.

Discretionary Revisions

These are recommendations for improvement which the author can choose to ignore. For example clarifications, data that would be useful but not essential.

8. I was surprised not to see any reference to the SWAY project in northern Uganda in the introduction, since this is one of the largest studies (to my knowledge) of war-affected youth, and includes evaluations of reintegration programmes.

9. The research raises so many interesting questions and issues – it feels to me that it would have been greatly enhanced by a qualitative component to explore some of these findings.

10. I notice in Table 5 that there is little change between T2 and T3, but statistically it is still highly significant. I wonder whether effect sizes might be a more meaningful way to report these differences? Also, as a minor point, I understood that since it isn’t statistically possible to have a p of .000, the correct way to report is <.001.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.