Reviewer's report

Title: School-based mental health intervention for children in war-affected Burundi: a cluster randomized trial

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Reviewer: Frank Neuner

Reviewer's report:

The authors have carried out a randomized controlled trial comparing a school-based intervention (“Classroom Based Intervention”; CBI) with a waitlist condition for traumatized children in war-affected Burundi. Psychological trauma in children living in war-torn areas is one of the major current mental health challenges and much research is needed to learn about economic ways to reduce the symptoms of affected children with pragmatic interventions. The authors must be congratulated for their enormous effort to study a psychosocial intervention with the high methodological rigor of a randomized trial in such a challenging context and I think that the publication of the findings of this study will have a major impact on the field.

However, the conceptualization of the rationale of the study, the analyses and the writing of the paper is surprisingly confusing. First of all the paper remains unclear what the precise aim of this study was. The whole direction of the paper turns towards determining indications for a differential indication of the treatment approach in this context through a moderator analyses. However, to solve such a question, a randomized trial (at least not in this form) is neither necessary nor helpful. The registration of the study shows that the initial aim of the paper was to test the efficacy of the intervention against a control condition, this aim does fit the design of the study and I would strongly recommend shaping the writing of the paper, including the abstract and the discussion towards this primary and most important aim.

This would require identifying the primary outcome measure as defined in the protocol of the study. I guess that it was PTSD symptoms. If the study does not show a difference between the trajectories of the participants in both conditions, and if the study was carefully planned and carried out with enough power (which has been clarified in detail) then the first and major conclusion of the paper should be that CBI did not prove to be a successful intervention in this context, and that there was not even any indication of a unspecific effect of CBI let alone a specific effect. As discussed in the limitation section, some methodological shortcomings (e.g. the lack of blinding) possibly operated in favor of the intervention, there is no reason to assume that methodological problems were responsible for the non-effect.

From a contemporary presentation of a treatment trial I would also expect the analyses of individual changes in both direction, the counting of clinically or at least statistically reliable (reliable change index) improvements and worsenings.
The discussion of the paper should focus on the lack of efficacy, and try to find solutions as well as to compare the findings with the results of studies using other, sometimes more effective interventions. The most important conclusion is that this study does not support recommending the use for CBI as intervention in crises countries. This statement is important, in particular as CBI and other group interventions with children are increasingly used. I would expect a discussion about the potential and limitation of school-based interventions in general, as well as a comparison with other intervention approaches to learn something for practice. This is particularly true as the children in most need seem to have dropped out more commonly than the others.

The presentation of the procedure of CBI does not allow determining the amount and quality of the exposure or trauma-focused component of this procedure, it seems that CBI does not qualify as a current trauma-focused procedure which are recommended for the treatment of PTSD in children and adults. It would be important to specify more in detail how the narration is being done in the group format and how guided drawing is supposed to allow exposure. Unfortunately this information can currently not be assessed through a published manual of CBI. In the discussion, the authors are often confusing prevention with intervention, it would be important to separate these two strategies.

I would advice not to overate the meaning of the moderator analyses. The hypotheses related to moderators are very vague, and the expected effects are sometimes only found for a single variable, not existent, or even in the opposite direction. It is not unlikely that much or all of these results are in fact chance findings. Despite the large number of tests and potentially plausible tests I could not find out whether the authors have adjusted the # level here. In addition, I am not familiar with the details of the analysis that has been carried out, and I wonder if the estimates are in any way standardized and allow the estimation of an effect size. In any case, understandable effect sizes of the moderators should be reported to see whether any of the moderators really matters from a clinical perspective, and this should be discussed. This is particularly important as, if there are clinically significant moderators in a study with an overall zero-effect, it is obvious that some subjects improve on the cost of others who deteriorate. Again, this argues against the widespread application of CBI and indicates that CBI might even be harmful for some children, in this case especially for the most vulnerable children who have been worse off in the beginning. I would expect a clear statement here, also in the abstract.

**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