Reviewer's report

Title: A rapid screening tool for psychological distress in children 3-6 years old: results of a validation study

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Reviewer: Elisabeth Schauer

Reviewer's report:

1. The authors are to be commended for having undertaken the development of a short screening tool for mental health needs of children in humanitarian settings. The need to develop screening tools for lay counsellors is certainly important, given the many children who are exposed to family-, organised violence and complex emergencies around the globe.

2. Research in humanitarian settings shows that despite the often claimed ‘over focus’ on trauma, little has been done in terms of screening and treating trauma spectrum disorders in resource poor settings. So it is commendable and of key importance that trauma and its psycho-pathology is part of any screening tool that wants to adequately represent the mental health rehabilitation need in settings of humanitarian emergency in a given child population.

3. Much of the paper however lacks sufficient explanation and description. As a reader it is hard to follow the logic of the methods part, through to results and discussion. For example: What was the reasoning to draw the specific sample? How exactly was test-retest carried out? How was validation carried out? Where is the result of the qualitative research reflected in the discussion? The paper should be reworked, better structured and much extended in explanation.

Introduction:

4. In general it would be good, if the paper would have a more elaborated background section on the situation of children in humanitarian settings and especially a more guiding explanation of the local setting from which data was drawn.

5. The description of the tool, the QGE, should be moved to the methods section.

6. The authors claim that their paper is the first assessment tool designed for humanitarian setting and to be applied by local lay practitioners. This is not up-to-date current knowledge on peer-reviewed journal publications. Earlier such instruments have successfully been validated and presented in peer-reviewed papers, e.g., by Elbert et al. (2009) for Tamil children, Ertl et al., in a sample of former child soldiers or by Odenwald et al. for a Somali context, as well as others. It would be useful if authors include existing research and their findings.

7. There are a number of statements in the introduction, which remain unclear to
the reader. For example: “Psychological distress in young children is particularly difficult to evaluate by non-specialists, because of its specific psychopathology”. What exactly is ‘specific psychopathology’? And why if lay persons were appropriately trained, could they not conclude with a diagnosis? It would be useful if authors could explain such statements in more detail.

Methods:

8. It is stated by the authors that the present aim was to develop a tool for a complex humanitarian context, yet the tool was tested on a non conflict-exposed part of the Niger population. It thus might not accurately serve now for populations of children who have been exposed to conflict or humanitarian hardship in the past or present. The aim of the paper should therefore be rephrased, since the tool was not validated in a population who suffers from a humanitarian crisis.

9. Several studies have shown that especially mothers have very poor judgement of the mental health status of their own children. In some studies the data of mother have been withdrawn due to misrepresentation. Best results tend to be obtained through direct child report and adjunct third person report of non-family members, e.g. class room teachers, or family members but not the primary care taker. Can authors please clarify why they have chosen mothers’ as key informants?

10. Can authors please explain how exactly the ‘random sample’ was drawn?

11. Can authors please explain why they did not ask children and their care takers for distinct trauma experiences, including questions re family violence, conflict experiences, human suffering? Latest research shows that children exposed to humanitarian/complex/conflict settings, suffer an extra amount of family (parents/step parents, extended family e.g. grand parents, foster families) and community violence (teachers, neighbours), which adds to the development of psychopathology caused by experiences of war/organised violence/forced migration.

13. Can authors please further clarify how many clinical psychologists carried out clinical diagnosis?

14. Was this a direct and confidential child interview?

15. Was an interpreter present? How were interpreters trained?

16. Local interviewers were trained for 2 days only? Who trained them and what was the content of the training?

17. It remains unclear why more severely affected children were withdrawn from the study sample and who diagnosed a ‘psychosis’ in a child of less than 6 years?

18. Can authors please explain who carried out the validation interviews and with
which instruments? It is mentioned that the CGI was applied. We need a good
description of the CGI-S, including its psychometric properties.

19. For validation, the last 255 interviews were used, why that? Why did authors
not draw a randomized sub-sample?

Results:

20. There are far too little results presented. The reader would like to see more
calculations and also tables, to better understand the results.

21. Were the interview results of mothers’ compared to clinicians’ results? or
were the interview results of mothers’ compared to the clinical results of child
interviews?

Especially, test-retest, inter-rater reliability and external validation should be
presented in clear structure and with tables if possible.

22. Also measures of objective validation are missing. A small child with a mental
health disorder, presenting with symptoms of distress, should also present with
symptoms of cognitive delay/dysfunction, with problems in motor control and
physical challenges, such as stunting and weight problems. These features
should be considered in future validation studies.

Discussion

23. The paper lacks a clear description of its practical implications. Authors
should explain why rapid non-specific screening is important in settings of
complex emergencies, since a screening tool, like the one that is presented,
which is general and does not help elucidate on the specific origin of children’s
distress might be redundant. Massive evidence has been provided in the past
years on the mental health impact of children in circumstances of e.g. forced
migration, war, and organised violence around the globe. We know what children
in humanitarian settings who have been exposed to trauma suffer from; the
challenge is to understand the specific issues of the chosen child population at
hand.

24. Since screening for mental distress of children also carries the ethical
obligation to build-up psychological treatment structures (with clinical experts as
well as lay counsellors), it is unclear why the questionnaire was designed to only
test for distress and does not allow clinical diagnosis. Clinical interviewing by
local lay counsellors, resulting in diagnosis and thereby allowing to gain a clear
picture of the present problems, has been successfully developed and
implemented in a good number of (post-) conflict settings in recent years. It
would be helpful if the authors could reference those. Examples by Catani et al,
2008 & 2010 in a samples of war affected Tamil children in Sri Lanka, by Catani
2009 in a sample of Hazara school children in Kabul, Afghanistan; by Elbert 2009
in a sample of war-affected Sri Lankan school children; by Ertl 2009 in a sample
of war affected children and youngsters in Northern Uganda; by Neuner 2006 in
a sample of war and tsunami affected children in Sri Lanka, among other.

25. The authors might further assume that based on the intended outcome of the questionnaire “does the child need psychological/psychiatric care” (yes – no) the children might now be referred to accessible mental health treatment facilities, where proper clinical diagnosis will follow. However reality on the ground shows that those structures (e.g. locally based clinical psychologists, clinical counsellors and psychiatrists) almost never exist in complex humanitarian settings. Having screened only for ‘psychological difficulties’ now, does not really allow the development of counselling or treatment services that can possibly fully address children’s needs in a given population.

Can authors please clarify the usefulness of such a tool and the practical implications it might yield nevertheless?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.