Author’s response to reviews

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

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Author’s response to reviews: see over
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Martina Ruf-Leuschner, MD
Contributing Editor
BMC Psychiatry

Reference: Submission of revised manuscript 1315152106606504: A rapid screening tool for psychological distress in children 3-6 years old: results of a validation study

Dear Dr. Ruf-Leuschner,

Thank you for reviewing our manuscript, “A rapid screening tool for psychological distress in children 3-6 years old: results of a validation study”

We are grateful to the editors and reviewers for their insightful comments, all of which have been carefully considered and incorporated in the revised version that we are enclosing.

We have changed the name of the tool used to facilitate comprehension and future use of the tool by rendering its name clearer (Psychological Screening for Young Children aged 3 to 6, PSYCa 3-6).

Please also find a point-by-point response to each of the reviewers’ comments.

We thank you for the consideration of our work and look forward to hearing back from you.

Sincerely,

Caroline Marquer

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Reviewer 1

Elisabeth Schauer

Discretionary Revisions:

Comment 1: Can authors please stress the problem that once the instrument is in use there will surely not be enough 'clinical psychologists' to provide care for all children who are in need of care - so as to press on the plight of absence of humanitarian aid and investment as it relates to psychological rehabilitation of children in complex settings.

Response: Following your comment, the problem of scarce mental health professionals in humanitarian field is now highlighted in the discussion.

“However, it is important to highlight that this tool allows for the identification of children requiring further evaluation, but the lack of mental health professionals remains. As with all public health interventions, identification of children in need does not unfortunately always follow with their receipt of appropriate care. Greater investments are needed to ensure that children mental health needs are addressed, and certainly that they are only screened if appropriate care is available”

Comment 2: Can authors please stress that trauma-related childhood experience (violence, abuse, neglect, etc) are key to be found early on, since this is a major issue for adolescent and adult psychopathology and that mother’s might not be the best people to honestly report these experiences, since they might be the main perpetrators or they might want to protect a family perpetrator. Ideally mothers must be interviewed alongside their children to get a full picture of the source of distress and psychological suffering inflicted on children. Can authors please mention excellent diagnostic tools such as MACE, which should be translated and adapted for use in international settings.

Response: We thank the author for this comment and now have added reference to these comments in the discussion.

Reviewer 2

Reviewer: Mark Jordans

In general, the authors have answered very adequately to the issues raised. The paper has improved significantly. As mentioned before, this study is an important contribution to the field, especially given the young age that the study focuses on. Introduction and discussion are strong and convincing.

Response: We thank the reviewer for this positive feedback.

Major compulsory revisions:
Comment 1: While the authors are much clearer in providing a rationale for the instrument and validation thereof, they fail to clarify the core goals, or intended use, of the instrument in settings such as Niger. Is it primary screening to be followed up by clinical assessment? This would beg the question whether instrument is planned to be used on a population level. Or is to screen for provision of services? In which case it is unclear what services are to be offered after such screening (especially as it concerns a non-specific construct that is being assessed). Especially as the RST-22 will be made available to all health actors in Niger, the question as to what will be done upon use of the RST-22, in settings with no available treatments, remains largely unanswered.

Response: We thank the reviewer for this comment as this indicates that additional clarification is needed in the manuscript. The tool was validated in humanitarian settings, on the general population level. As mentioned by the reviewer, for ethical reasons, it should be used with an initial identification of care providers; either local organizations providing mental health care or in case of the absence of mental health facilities on the field, expatriate clinical psychologists. In Niger, medical care is provided through formal government structures with the support of both local and international NGOs as the country remains one of the poorest in the world. As is the case with all medical treatment, individuals should not be “tested” unless they have the possibility to be treated. We envisage the same for this scale. However, it is important to recognize that without easy to use tools, we will have difficulty motivating and moving children’s mental health needs to the forefront. We also recognize this catch-22. We hope that by highlighting this important point concerning the absence of mental health care related to psychological rehabilitation of children in complex settings, this point can be sufficiently emphasized.

Minor compulsory revisions:

Comment 2: The authors refer to ‘standard cross-cultural validation’ several times throughout the article. I have two issues with that. First, it is not clear what is meant by a standard cross-cultural validation? Does the standard refer to the golden standard, or otherwise? While, I agree that the authors follow the correct steps for validation in a different cultural context, I am not sure what they mean with ‘standard’. Second, rather than validation across cultural settings (and comparisons between these), the present study is the first to validate the RST-22 in a cultural setting that is different from the one where it was developed. If I am not mistaken the label of cross-cultural validation is misleading.

Response: Thank you for your comment. By “standard cross cultural validation”, authors meant that they used a classic validation of a tool in a cross cultural set up, validating the tool in 3 different fields. For better understanding, we have now used “cross cultural validation” instead of “standard cross cultural validation”. We had hoped that by using the term “standard”, we could focus on the methodology, highlighting that the study included a large sample, a strong gold standard and repetition of results as opposed to the many other small scale validations.
Comment 3: On page 6, the authors refer to existing instruments that are mainly trauma-focused. I think it might be good to also demonstrate that work has been done in conflict affected settings with instruments that focus on non-specific psychosocial distress (such as the CPDS).

**Response:** The sentence had been rewritten to provide more details.

Comment 4: The authors make reference to ‘three validations’ (page 7). I think this should be ‘three steps in the validation process’. But more importantly, it is not quite clear what the ‘principal validation’ is that is presented in this paper. What are the two secondary steps, if they are not included here?

**Response:** Thank you for the clarification. The cross cultural validation of the tool included 3 validations, one called principal including more than 500 children, and two called secondary validations were implemented to strengthen the overall results using 200 children in each of the other two sites. The overall process will be presented in another paper including the two secondary validations. The sentence was changed and three steps in the validation process was added.

Comment 5: Page 13, two clinical psychologists carried out the interviews. If these were part of the team of authors, it would be good to include their initials.

**Response:** The two clinical psychologists were part of the team of authors, their initials were added.

Comment 6: Page 14, the authors state that they use the ‘cut-off of 17 based on prior use’. This is not clear. How was this cut-off used and why was it used, as the purpose of the validation study should be to establish this.

**Response:** A previous validation of the tool was performed, initial version including 40 items, and the cut off was defined at 17, after analysis of ROC curve and specificity and sensitivity. This cut off was used as an indicator for the validation in Niger, at the end analysis were performed and cut off defined at 9, which was the purpose of the validation study.

Comment 7: How do the authors explain the result that the optimal cut-off is 9 (with differences between indicated and non-indicated groups of ‘only’ 4.7 points), with regards to the large response-scale of 0-44?
**Response:** The cut off of 9 was the value that minimize the distance between the ROC curve and the top left-hand corner (where sensitivity = 1 and specificity =1) (G. Zweig, M.H. and Campbell, 'Receiver–operating Characteristics (ROC) Plots: a Fundamental Evaluation Tool in Clinical Medicine', *Clinical Chemistry*, 39 (1993), 561–577.) We have added this into the manuscript for further clarification.

Comment 8: Related to the previous point, it is not clear that the response scale of the reduced RST is 0-44 (instead of the 0-80 as mentioned in the methods section).

**Response:** In the method section, the tool is presented in its initial version including 40 items (which referred to a total score from 0 to 80). At the end, a reduction of item was performed including 22 items. This paper only presents the results of the scale including 22 items instead of 40. A sentence was added to clarify for the reader.

Comment 9: Table 2 does not include the statistics/ results on the comparisons between the RST-22 scores compared with the indicated/non-indicated group.

**Response:** Table 3 includes " RST-22 scores compared with the indicated/non-indicated group". To make it clearer a legend was added to table 3.

Comment 10: In the conclusion the authors refer to the need to further test the ‘post traumatic component of the tool’. It is not clear to me how the authors come to this conclusion as results and discussion do not really make mention of this.

**Response:** Niger was a stable context at the time of the study, by stable authors mean no violent conflict. The post traumatic component needed to be tested in complex setting where young children presenting psychological difficulties link to trauma exposure were expected. Authors choose to validate the tool in such context, Buenaventura in Colombia and a slum in Nairobi (Kenya) where children overcome difficult events such as exposure to violence (secondary validations).

Comment 11: Also it is not clear from table 2 or analyses paragraph in the methods section whether the ROC analyses were done against the CGI results or the response on the question “Does the child need psychological/psychiatric care?”

**Response:** The ROC analyses were done against the response on the question “Does the child need psychological/psychiatric care?”. The sentence had been modified to make it clearer.

Discretionary revisions:
Comment 12: Page 8; rather than 'hetero-questionnaire', it might be better to use 'multiple informants', if that is what the authors mean.

Response: Thank you for your comment. By "hetero-questionnaire", the authors meant items directly asked to caregivers through an interviewer and not to the child. The tool had been designed to ask the questions to caregivers exclusively, a person who is in charge of the child's "daily care". It had not yet been designed to multiple informants such as teachers or other siblings.

Comment 13: The authors have explained that the item reduction process and the factor analyses results will be presented as a separate paper. I think it would be better to mention that in this paper, so that the reader does not interpret this as a potential weakness of the article.

Response: A sentence was added in the data analysis paragraph to highlight this for the reader.

Comment 14: Page 17-18; the first two limitations are very clear and very pertinent, but I would still argue that both are not necessarily limitations, but rather discussion points. Should the authors not consider presenting both points as discussions rather than limitations (a lengthy translation process I believe is a pre-requisite, and using clinical assessments are often the chosen method over other self-report instruments). The authors have answered in their reply that this is because there is still debate about this, but about the former point I am not familiar with that debate, only with the recommendation for thorough translation.

Response: The paragraph had been rewritten to make it clearer as suggested by the reviewer.

Comment 15: In the paragraph in the conclusion on non-golden standard validation, it would be good to refer to some other literature on this topic (e.g. Bolton et al, reference # 41).

Response: The references had been added.

Comment 16: Not sure whether figures 2 and 3 are needed, I would consider omitting these.

Response: We agree with the reviewer, but to accommodate previous requests from reviewers we have chosen to keep these two figures.