Author’s response to reviews

Title: Course of Post Traumatic Stress Disorder and healthcare utilisation among resettled refugees in the Netherlands

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Author’s response to reviews: see over
Dear Sir, Madam,

You will find here enclosed a revised version of our manuscript entitled “Course of Post Traumatic Stress Disorder and healthcare utilisation among resettled refugees in the Netherlands”.

This new version took the comments of the reviewers into account by modifying the text at several places. You will find below a point-by-point description of the revisions since the last version of the manuscript, in the form of response to the reviewer’s comments. We grouped comments referring to similar points. The manuscript has been checked by a native English speaker, and the Competing Interests section has been added at the end of the manuscript, as requested by the Editor.

We hope you will find this new version suitable for publication.

Sincerely,

Majda Lamkaddem

Reviewer 1

Minor Essential Revisions:

“1. Abstract, results--They are difficult to understand without reading the rest of the paper. First sentence should just state that prevalence was essentially unchanged, with percentages. It would be better not to use words that suggest analysis in this section and instead just state the numbers. I suggest deleting the section that starts with "partly attributable to" and instead start a second sentence with the parenthetical section. For example, Half of the respondents with PTSD at time 2 were new cases, for whom PTSD developed after 2003. Then should delete sentence that starts with "the second reason..." as well as the second half of sentence 3 (starts with , while the effectiveness...) These both seem more like interpretation and would be better in conclusions.”

And

“2. Abstract, conclusions-- I find this section awkward. I would suggest something such as, "A persistently high prevalence of PTSD was found in a group of refugees. The findings of this study suggest there are two possible explanations for this phenomenon. One is the late onset of PTSD and the other is low utilization of mental health care. Healthcare professionals should be aware of these issues, especially given the effectiveness of mental healthcare for this condition.""

As this study is not a straightforward prevalence study (which would need another study design), we prefer not to draw the attention of the prevalence figures, but focus on the results explaining changes in PTSD through time. Therefore, we go straight to the these results in the abstract.

On the second point, we agree with the reviewer and we changed the Conclusion section of the abstract, following this useful suggestion.

“3. Background, general--The paragraphs that start with first, second and third possibilities do not transition well from one to the next. Should instead start each with something such as "One (or another) explanation for the persistent high
prevalence of PTSD is..." It should be more clear that there are 3 potential explanations based on the literature--1) new cases/late onset, 2) same patients but untreated, or 3) treated but unsuccessful.

4. Background, paragraph 2--Please rephrase the "to be in contrast to" part and make more clear.

5. Background, paragraph 3--See above in general. Also, please change "this disease" to PTSD. Please rephrase "quasi-unchanged". The second part of the sentence with reference (10) needs clarification. I am not clear on what this part means.

6. Background, last paragraph--Please state research question and hypothesis. These are very clear in the analysis section of the methods and should be more explicit here."

The three hypotheses are now called 'hypotheses', instead of 'possibilities'. We hope this introduces better the role of these possibilities through the paper.

We rephrased paragraph 2 and stated: "this persistent high prevalence seems to be in contrast to the availability of effective forms of treatment".

We replaced 'this disease' by "PTSD" and rephrased 'quasi-unchanged'.

Finally, the research question is now made more explicit, and the hypotheses are formulated before the research question, using the appropriate terminology ('hypothesis' instead of 'possibility')

"7. Methods, Premigration section--Please define HTQ before using abbreviation (done in next section).”

This was changed in the text.

Reviewer 2

Major Compulsory revisions

"You should explain why you chose a 7-year gap in particular? Was there a literature-driven rationale behind this choice of interval or was this just a matter of convenience."

We agree with the reviewer and explained in the text (p. 6) that this time gap was mostly driven by practical circumstances. However, the time period was also in line with some logical considerations concerning resettlement and health changes of recent and longstanding permit holders.

"Did you test to see of there were systematic differences between respondents and non-respondents at T2 by comparing them on baseline data. It would make your paper stronger if you could test for systematic differences and interpret your main findings accordingly.”

And

"Were statistical tests used to examine these differences? If yes, please report. If not then please explain why not as this is standard practice.”

And

"Yes but you do not show any statistical tests here to examine differences. Also could you compare respondents with non-respondents specifically instead of the total T1 sample?"

We tested whether there were significant differences between responders to both periods compared to all responders. The results of these analyses are presented in the first part of the Results section. We also added information about statistical testing of the differences in the text. Given the objective of this specific study (understanding the reasons for long-lasting PTSD among resettled refugees in the Netherlands), we did not perform a classical non-response analysis (comparing responders to non-responders), because non-
responders were mostly refugees who did not resettle. They were therefore by definition excluded from our study population for this specific study and research question. However, we wanted to give an overview of the attrition for the whole study. Therefore we performed the analyses presented in Table 1. In the Discussion section (last remark), we changed the sentence on these results by not calling these analyses ‘non response analyses’. We agree with the reviewer that this was not the correct term.

“How does this cut-off match with what is recommended for the complete HTQ assessment? You need to explain why this cut-off was chosen. Does it have any clinical relevance?”

We added information on the choice for this cut-off point in the text (p. 7).

“What if scores remained the same, was that coded as 0? Was any change considered improvement/deterioration or was a threshold established for change to be considered true improvement versus true deterioration? This information is important as ‘any’ change could sometimes merely be random variation. However, this depends on the psychometric properties of the instrument used.”

We agree with the reviewer, this information was lacking. We added it in the text, p.7.

“It is not always appropriate to treat a variable originally measured on an ordinal scale as a continuous measure. You need to do some principal component analysis and report Cronbach’s co-efficient alpha to first prove that the 18 items do indeed measure the latent construct of post-migration difficulties. If you do not have the expertise to do so, it should be reported as a major limitation.”

We agree with the reviewer on the fact that it is not always appropriate to treat an ordinal variable as a continuous one. However, this processing of the variable concerned changes in the severity of post-migration difficulties, which are taken as one of the possible determinants of change in PTSD. In these explanatory analyses, we were mostly interested in the general direction of the association than on the relative contribution of each category. Also, changes in living difficulties (measured by these questions) has been more often processed as a continuous variable (Nickerson A, Steel Z, Bryant R, et al. Change in visa status amongst Mandaeans refugees: relationship to psychological symptoms and living difficulties. Psychiatry Res 2011 15).

“Also the significance of these results should be interpreted cautiously as the confidence interval is quite large and the p-value is barely significant. Please add this not here.”

This was discussed in the Discussion section. However, we agree with the reviewer on the fact that this should be made clear in order to allow the reader to be cautious in the interpretation of the results. We therefore added a note in the text (p.11).

“What about the implications of your findings? What needs to be done to help refugees recover from PTSD post-resettlement? Further, what needs to be done to prevent late onset PTSD?”

This remarks concerns the entirety of the Discussion section. In the Conclusion, we state: “[...]The findings emphasise the need for primary care providers to follow existing guidelines on quick referral to mental healthcare for patients presenting with PTSD symptoms [...] the results show the importance of improvements in contextual factors (e.g. in employment, social/family networks, getting familiar with the new culture and social position) in addition to the use of mental healthcare in the course of PTSD.”
Based on our study results and the limited numbers of respondents, this is in our opinion as far as our recommendations can go. An intervention study could be a logical following step, and would help formulating more concrete implications and recommendations.

**Minor essential revisions and discretionary revisions**

We adapted the text following all minor essential revisions, and most of the discretionary revisions. We wish to thank the reviewer for the attention paid to the form as well as to the contents of this paper, and for all valuable comments.