Author's response to reviews

Title: Psychological Trauma and Evidence for Enhanced Vulnerability for PTSD through Previous Trauma among West Nile Refugees

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Author's response to reviews: see over
Comments on the reviewer’s reports

**Reviewer 1 Joop TVM de Jong**

The reviewer sees “a variety of major and minor methodological flaws”. Many of the issues pointed out involve requests for more detailed descriptions of the methodology and thus no principal limitations to the study. As described below, we have addressed these concerns. The reviewer raises also a number of issues far beyond the scope of this article, and given that the other reviewers have only made a few requests regarding methodology, and none of them as “major compulsory revisions”, we have decided not to extend our descriptions within the manuscript. Instead, we will refer to a previous paper [Karunakara, 2004 #662] that contains a more detailed description of sampling, translation and assessment procedures, as well as results on comparisons of the demographic, economical, and mental health characteristics of the studied groups (but does not examine the dose-effect model). We are fully aware of the problems mentioned by the reviewer. We do not think that the scientific dispute relating to the cultural validity of trauma should be made part of the present research paper. We will, however, respond point by point to the reviewer’s concerns in this letter:

a. **The authors mention that they use a multi-stage sampling design without providing any specifics of their sampling frame or strategy.**

A multi-stage sampling technique was used to select sites for the survey. The sub-counties were selected purposively based on the absence or presence of the refugees. The Imvepi Refugee Settlement was situated in Odupi, while Midia had substantial numbers of ‘self-settled’ refugees co-existing with their Ugandan hosts. Yivu, with no refugees, served as a comparison group in the study of the effect of post migration residential arrangements on the lives of refugees as well as their hosts. The relatively stable security situation that existed at the time of the survey, allowed access to Otogo, home to many of the refugees surveyed in Uganda. Villages — the primary sampling unit — to be surveyed in the sub-counties were selected randomly. Households — the ultimate sampling unit — were selected systematically. Sample size calculation was based on available under-5 mortality figures so as to calculate the most conservative figure possible. Calculations were made for comparing populations with a design effect factor to account for deviations from a simple random design.

b. **They mention qualitative research on traumatic events without clarifying whether, and if so, how they did that research in the Ugandan and Sudanese context or whether they used categories generated by other authors.**

The checklist was compiled after interviews with key informants (security personnel, doctors, community leaders, women’s representatives) and 30 respondents from all three populations about their personal history of stressful events. Following these interviews, the single events obtained in these studies were rated as being potentially traumatic by experts. The following pilot checklist was pre-tested among further 44 Ugandans and Sudanese in areas not selected for the survey and modified according
to the suggestions of the respondents. A primary item analysis based on inter-item correlations led to the exclusion of some events that were obviously not directly related to traumatic histories, e.g. the experiencing of witchcraft.

c. They mention that they use traumatic event types because they consider this to be more reliable than assessing the frequency of traumatic events. However, they do not mention how they distinguish event types. Further on it appears that they use traumatic events as reported by their respondents.

We assume that the reviewer is aware that every PTSD Interview includes a list of event types, i.e., classes of traumatic events that are common in trauma research and e, like natural disaster, accident, rape, etc. We now clarify that our respective statements refer to this commonly used construct in PTSD research.

d. They then ask for lifetime traumatic experiences/events and 12-month traumatic experiences. They compare the Sudanese and Ugandan populations without controlling for their highly different traumatic histories, both in duration and severity, in the two countries.

This objection is not a valid one, as we did not „compare the Sudanese and Ugandan populations“, and we are uncertain how the reviewer came to this conclusion. On the contrary, we included both populations in one analysis, and the fact that the populations did indeed vary in „traumatic histories, both in duration and severity“ provided us with a sample with a high variance of traumatic exposure. This rationale has been explained in detail in the introduction.

e. They translated their instruments with the help of 'repeated procedures of blind back translations'. There is a vast literature on the complex art of translating instruments for psychiatric epidemiological research (Marsella, Manson, Flaherty, WHO [Sartorius and Janca], Van Ommeren etc) of which they do not seem to be aware. Whatever 'repeated procedures of blind back translation' means, it is not sufficient to deal with semantic, conceptual, content and technical validity, a fortiori with a rudimentary and therefore difficult language such as Juba Arabic.

We are well aware of the different scientific methods to translate instruments, their strengths and weaknesses, but would not necessarily call it an „art of translating instruments“. The questionnaires were translated using the Back-Translation (Double Translation) and the Decentering (Symmetrical) techniques (Flanagan 1999). The goal was to achieve a conceptually and semantically equivalent translation while maintaining ‘colloquialness’ (Flaherty et al. 1988; Manson 1997; Sperber, DeVellis and Boehlecke 1994). An ongoing revision of the questions in both Arabic and Lugbara was carried during the entire training period to keep the instrument comprehensible, acceptable, and comparable as well as culturally relevant (Van Ommeren et al. 1999). During the training period, the research assistants and the supervisors were asked to translate the instruments to Arabic and Lugbara. The Arabic Speakers and Lugbara speakers were divided in 2 separate groups. These groups were further sub-divided to two sub-groups. A sub-group would translate a part of the questionnaire and later compare that translation with that of the other sub-group with the aim of arriving at a consensus translation. This process was iterative
and the first draft of the questionnaire was then sent to Makerere University for back translations. If the back translations identified problem questions, then these questions went through the translation process once again.

The advantages of going through this process with the team were two-fold. First, the questions were reviewed one by one for its comprehensibility as well as its relevance to the context in which the survey was being carried out. The RAs and the supervisors were then, through this process, able to understand the significance of the question better and improve its validity by suggesting changes as well as modifying coding categories. Second, if the instruments had been translated at the University in Kampala, the translated questionnaires would have failed to capture the nuances of the language as well as the local parlance. Through our translation process, the team was able to successfully develop a locally relevant instrument both in terms of the language as well as content. This was particularly difficult in Arabic because, while the Sudanese spoke the language, very few could actually read the Arabic script. So, the painful and long task of transcribing the Arabic questionnaire in the Latin script was undertaken. The translated questionnaires were reviewed by experts at Makerere University for content validity and further modified based on feedback from the interviewers and respondents after the pre-test.

f. They use an instrument to assess PTSD, the PSD, which is a clinician administered instrument often used in the US. They then mention that this instrument was 'modified for assessment by trained lay interviewers' without indicating how they did the modification or how the lay interviewers were trained.

We used the PDS (not PSD) which is not a clinician administered instrument but a self-report questionnaire. As we could obviously not use a self-report interview in populations with a high rate of illiterate respondents, we had to reformulate questions to fit an interview format. We have now included the fact that the PDS is a self-report instrument in the manuscript.

A detailed description of the training procedure seems to unnecessarily extend this article, as we can refer to our methodological paper and to a dissertation thesis by Dr, Karunakara. However, if needed, we could include the following:

The full training of the interviewers took two months. The project objectives and the rationale behind the structure of the survey instrument as well as that of each question in the DFMP questionnaire were discussed in detail. Great attention was also paid to issues such as initial contacts, maintaining a professional attitude while in the field, avoiding influencing the respondent, and reducing interviewer and courtesy biases (Nindi 1985). The importance of collecting information by means of standardized questions so that the same question was asked to all respondents is stressed and questioning and probing skills were developed (Peavy 1996). Supervisors were instructed separately on data collection guidelines, their roles and their responsibility to ensure data quality.

Quizzes, role plays, mock interviews and discussions of refugee-related topics were used to illustrate and emphasize some of the issues that were being taught. For instance, keeping in mind the sensitive nature of some of the questions regarding violence and trauma and the fact that the team members were from the study population and probably had experiences similar to the respondents, a workshop on
sexual and gender-based-violence was conducted by a consultant to the UNICEF office in Kampala, before the survey. The aim of this workshop was to increase awareness and sensitivity of the team towards respondents and their experiences. Another consultant to the project reviewed the team’s interviewing skills and the project’s data quality control measures just before the start of the survey. Problem areas were identified and remedied.

g. Next, they assessed the validity of the instruments with the CIDI and the SRQ ‘performed by German clinical psychologists’. WHO’s CIDI is a lay administered instrument that includes a section on PTSD and that has been validated in a number of countries by (among others) German psychologists. Why do the researchers validate the PDS -a worrisome choice in the trauma field with series of PTSD instruments - with the CIDI? Why do they mention the SRQ which is highly unreliable (see papers Kortmann) and does not include questions on PTSD? Moreover, one either validates an instrument with culturally validated instruments or with clinical judgment. Mentioning the use of instruments and clinicians in this regard hardly makes sense.

The reviewer does not present any reason why he – in contrast to the mainstream in the scientific peer-reviewed literature – he views the PDS as a „worrisome choice“.
We selected the PDS as it is, to our knowledge, the only instrument that combines two features we needed for our study: a) it allows the diagnosis of PTSD without clinical expertise with a satisfying validity and b) allows a rating of the severity of post-traumatic stress.
While the CIDI can be used by adequately trained lay interviewers, in our validation study we used it as a structured interview, i.e. guideline for the judgement of experts. We preferred the CIDI as it is a structured interview that has been translated in many languages before. This is facilitated by the formulation of items in the CIDI that relates more directly to the culturally universe symptom clusters than in other interviews like SCID or ADIS or CAPS. The opinion, that „mentioning the use of instruments and clinicians in this regard hardly makes sense“ is in stark contrast to the view of the scientific community assuming that clinical judgement without the use of instruments is neither reliable nor valid and thus cannot serve as a gold standard. The SRQ was used to examine the convergent validity (a construct the reviewer does not seem to be familiar with; to see if the PDS score is related to anxiety and depression). While the instrument is indeed not perfect (no instrument is) the SRQ is one of the few instruments that have been developed for Subsaharan African populations.

h. They explained that researchers were not working for (N)GOs without explaining the rationale for this choice. Moreover, the title page of their paper mentions vivo and MSF, which by many people are perceived as NGOs.

We are grateful for the reviewer to mention the seemingly contradictory statements in the manuscript. In our original standardized introduction text we said that we did not work for „UNHCR, Red Cross or any Ugandan or Sudanese governmental organization“. This explanation was introduced to prevent false expectations and fears of the refugees that the way they responded to the questions might have a direct influence on support like food supply or protection. We changed this sentence in the manuscript accordingly.
k. They obtained informed consent without explaining how.

Informed consent was gathered using a standardized form explaining the potential risks of participation and explaining that no direct reward could be expected. The informed consent was signed by the respondent and a witness; illiterate respondents gave fingerprints. **We have included a respective sentence in the revised ms.**

l. They mention that counselling services were provided if necessary without mentioning by whom, assuming that this would be done by the NGOs mentioned before in the absence of government services in the area.

If necessary, refugees were referred to various NGOs that were active in the region at that time (MSF, TPO, vivo), and there was an agreement for such a referral with these organizations. Unfortunately, counselling services were and still are not directly accessible for many participants. **We have included the remark „where available“ in the manuscript.**

m. There is no information on the statistical procedures in the Methods section and the statistical methods are not adequate to answer the research question that is posed.

The statistical procedures are explained, straightforward and quite simple: we calculated correlation coefficients that we consider to be adequate to confirm the predicted linear relationship. It has been customary for reviewers to present reasons before making such a statement. Otherwise, an improvement of a ms seems hardly possible.
Reviewer 2 Zachary Steel

Minor essential revisions:

1. The authors indicated that they employed a multistage sampling design but have provided little details of this. Further information about the strategy employed and its representativeness should be provided. Some discussion of response rates should also be included.

We agree that this information is important. We now have made reference to the [Karunakara, 2004 #662] paper that provides detailed information. As mentioned above, a full description of the sampling procedure in the paper under consideration would extend the methods part to an extent unnecessary for the present focus and research question.

2. The authors indicated that they interviewed multiple individuals from within each household but have not adjusted their statistical procedures to take into account the clustering effect that this may produce. I do not think that it is necessary for the authors to reanalyse their results, but a note should be introduced into the text to indicate that the analysis was carried out on unweighted data that was not corrected for the effect of clustering. It would be reasonable for the authors to argue that it is not likely that this would have a substantial effect on the relationships examined in the paper, particularly given the large sample size employed.

Indeed we could have used weights or a nested design within a regression model to account for the clustering effect. This strategy might have been helpful if we would have analysed household data that is shared among individuals in the same household. In agreement with the reviewer, for the analysis of the relationship between events and PTSD symptoms we would not expect a different outcome. We have followed the reviewer’s suggestion to mention that we used unweighted data.

3. Reference to chronicity of PTSD in the concluding comments should be removed as the data presented by the authors did not examine this issue.

We followed this suggestion.

Discretionary revisions:

4. The authors indicated that they examined the validity of the PDS by comparison with diagnosis made by clinical psychologists using the CIDI SRQ but they did not provide agreement statistics between the PDS and CIDI. The paper would be enhanced in sensitivity, specificity and Kappa statistics were reported.

As described above (Reviewer 1), a detailed description of the validation procedure and results requires more explanations and we refer to a forthcoming paper on that.
5. **The paper would be enhanced by the provision of a table listing the % of each trauma type experienced by the respective populations.** Also some discussion of the meaning of trauma types such as “child marriage” should be included given that in some circumstances this practice may be culturally sanctioned.

This table can be found in the Karunakara et al. paper that aims at a comparison of the groups, which was not the focus of this article.

6. **Finally I think the paper would be enhanced by making some reference to the current debate (cf Summerfield) about whether PTSD has any cultural salience in such a post-conflict setting.**

Within this article we have presented no data to discuss the validity of PTSD in African countries, and this was not the aim of the present article. As this debate is quite heated and generally without the presentation of empirical data, we decided not to take a position here.
Reviewer 3 Rita Rosner

Minor Essential revisions

All spelling errors will be corrected

Discretionary revisions

1. Abstract: The conclusion is not really connected to the results section and reaches far beyond the scope of the paper.

We have modified the conclusion in the abstract accordingly.

2. Background: a) The background information could be more succinct and more closely related to the manuscripts main subject. For example reference to DSM-III (I guess this is meant in sentence two) is not necessary. But if this sentence remains in the manuscript the DSM-version and reference has to be included.

We included the reference.

c) When risk and protection factors are discussed, then the authors should mention as well peritraumatic and posttraumatic factors such as social support and life stress after trauma. Although the paper focuses on pre-traumatic factors and only on events, it should be mentioned that there are some difficulties to call them – in the special situation of multiple trauma – pre-trauma factors. After each traumatic event follow psychosocial consequences (loss of home, family etc.) which lead to a different entrance into the new traumatic event.

We have included a note on peri- and posttraumatic factors.

d) Dose-response assumptions are probably the best tested ones in research on PTSD and in the special case of multiple trauma it is – to my knowledge – unusual to find no relationship (see text “failing to confirm a relationship”. Page 4). Rather the correlations are small like .2, but still very consistent and in the case of war trauma most correlations are around .4. Thus the recommendation is to write this section more precisely.

We modified the sentence about dose-effect assumptions.

e) Cumulative trauma was estimated by assessing the number of different traumatic events. Why used the authors not the absolute number (5-times shot at, three times wounded etc)? Is there a reason for choosing this type of estimation?

To our experiences many subjects are not able to report the absolute number of some events that have happened often, like being close to a combat situation. Counting the absolute numbers would only be feasible if we could get reliable numbers from each respondent about each event.
3. **Method:** It might be helpful to use more subsections in the method part, such as “Sample description”, “Measures” etc. a) The sampling design should be explained in more detail. b) Is the sample somehow representative? c) 75% of participants were women. How come? Are all men dead or soldiers? Having a sample consisting of a majority of women leads very likely to higher prevalence of PTSD. In many samples the PTSD-prevalences in women are twice as high as those for men. d) Please add some information about responder rates

We have referred to Karanukara et al.

e) CIDI and SRQ have to be explained and references have to be mentioned. f) Validating the PDS with the CIDI is actually a very problematic procedure. Did you have a validated version of CIDI for this region?

See reviewer 1, g/h

4. **Results:** a) The correlation given in the first part of the results section is probably a pearson correlation. Yet as events are ordinal and the distribution is skewed I wonder whether the use of a rank correlation coefficient would not be the adequate coefficient.

The number of different event types is interval scaled and not ordinal. The pearson correlation is quite robust towards deviations of a bivariate normal distribution as long as there are no outliers and there are no heterogeneous subgroups in the sample that could explain a linear relation. Both assumptions can be examined in the scatterplot, with no hints on outliers or subgroups.

5. **Discussion:** a) Parts of the study have obviously been published before and are mentioned in the text. For a better understanding the quotation of prevalences (see first sentence of the discussion section) needs to be given earlier, for example in the section on sample description. Again given that there are 75% women in the sample, prevalences should be given gender specific.

We have modified the ms accordingly.

b) Understanding avoidance criteria as unspecific depressive items is a very interesting observation. Did you verify that observation with your data? Maybe PTSD is not as interculturally valid as we all assume, and the avoidance symptoms are not as strong in this culture?

We do not propose to understand avoidance criteria as unspecific depressive items. On the contrary, we found a relationship between traumatic events and avoidance symptoms, as opposed to Mollica et al.

c) In the discussion about the “psychobiological breaking point” the very probable cooccurrence of traumatic events and major stressors such as loss of home, bad nutrition etc. is neglected. Maybe the 100% of PTSD in the high-event group are not only a consequence of traumatic events but of loss of
resources as well. In summary a discussion of living conditions in the region and for this particular sample might be helpful for a better understanding.

The hypothesis is interesting. For the relationship between economic factors and PTSD we can refer to Karunakara et al., who could not verify such a relationship in the same sample, so it is unlikely that the loss of resources contributes a lot to the onset of PTSD.

6. Conclusions: Although I agree on a personal level with the conclusions, the conclusions do not follow from the results of this paper. The connection should be more succinct.

We have modified the text accordingly.

7. References: In my version of the manuscript there are two references at the beginning of the reference section which obviously belong to another manuscript on a completely different subject.

We have deleted both. Thank you!