Author’s response to reviews

Title: Near-death experiences, attacks by family members and absence of health care in their home countries affect the quality of life of refugee women in Germany - a multi-region cross-sectional gender-sensitive study

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Author’s response to reviews:

Dear editor of BMC Medicine,

We thank the reviewers very much for their thoughtful comments, which we have incorporated in the revised version of the manuscript or addressed in our rebuttal.
Reviewer #1 Sverre Varvin:

This is an important and well-researched paper. Six-hundred-sixty-three women from six countries (Afghanistan, Syria, Iran, Iraq, Somalia and Eritrea) living in shared reception facilities in five distinct German regions were interviewed by native speakers using a structured questionnaire.

This is one of the first large scale studies in connection with the great numbers of refugees that reached Europe in 2015 and 2016. The study gives a good picture of the situation of these women before, during and after flight and give important information on what determines their health condition and quality of life.

The statistical analysis seems adequate and well done but a review by a statistician is nevertheless recommended.

However, some supplementary information is needed as well as some points should be addressed by authors before printing the paper:

1. Although not English speaker it seems that the paper could deserve some language edition.

We had a native speaker check the paper for language and the changes are highlighted in the text.

2. It seems some references have been used carelessly: the authors write: "Women are more frequently less educated than men in their countries of origin and more frequently tend to family and care duties rather than working outside of the home, but the reference is to an article on gender differences in trauma and PTSD in ordinary population in Detroit, hardly applicable to refugees from different parts of Asia, Middle-east and Africa.

Thank you for pointing this out. This is of course the wrong reference and has been corrected accordingly. All other references have also been checked.

3. The traumatic events experienced or witnessed were assessed using the Harvard Trauma Questionnaire, HTQ and psychological symptomatology was assessed using the Hopkins Symptom Checklist, HSCL-25. This means that PTSD-symptoms were not measured, which may be a weakness in the study. HTQ uses the term traumatic events, which may approximates potentially traumatising experiences. In the text, the term trauma comes up some places possibly indication that the respondents had a trauma or were traumatised. This should be clarified in the paper (having experienced traumatic events or potentially traumatising experiences is not the same as having been traumatised.)

Thank you for this important remark and the request to clearly separate the two aspects. We added a sentence to the methods section (page 6) clearly stating that the used questionnaires allow for the identification of traumatic experiences but not for a diagnosis of traumatization.
4. Some places it is not clear whether the authors talk about correlations or effects, like in the following sentence: "Absence of formal schooling positively affected the perception of current living conditions (OR for dissatisfaction: 0.4, 95% CI 0.3-0.7, p<0.001)." This should be clarified.

Thank you for this remark. Of course we can only detect associations with our study, the sentence has been corrected into “.positively correlated with...”. In line with this critique, we also corrected the sentences in the previous and following paragraph.

5. The authors found it surprising that few reported FGM as reason for fleeing taken into consideration the high frequencies of FGM in some countries, but this should perhaps not be so surprising as most FGM happens in childhood.

This is a sensible point. We added this notion to the discussion (page 11)

Apart from this, this reader recommends this paper published with some minor revisions.

Reviewer #2 Angela Nickerson:

Thank you for the opportunity to review the manuscript "Near-death experiences, attacks by family members and absence of health care in their home countries affect the quality of life of refugee women in Germany - a multi-region cross-sectional gender-sensitive study". This study addresses an important and under-researched issue; namely the unique experiences of female refugees and how these contribute to psychopathology in these populations. The manuscript was clearly written and the authors investigated a number of important constructs in the study. I did have some queries about the study.

First, the authors state that this sample is "representative" of female refugees in the region at the time. They state that they contacted the Federal Office for Migration to obtain statistical data about the distribution of the refugee population and to calculate quotes for enrolment in each project site. Was this the primary criterion the authors used to determine if the sample was representative? To my reading, the sample seemed to be largely volunteer-based following information sessions presented at shared reception facilities. Accordingly, while the number of refugees who participated from each site may have been representative of the number of refugees in each area; it is not clear that these refugees were indeed representative in terms of the number from each community of origin, nor whether the individuals were representative of their communities. A volunteer sample such as this (rather than approaching pre-selected individuals on the basis of random sampling) limits the extent to which prevalence rates can be investigated as it may be the case that those individuals who self-identified as willing to participate either under- or over-reported psychological distress. Accordingly, it's not clear to me the extent to which prevalence rates determined from this study can be compared to the European reference sample - this would depend on the methodology used in this comparison sample.
Thank you for this remark. We absolutely agree that the method of sampling, based on the national quotas, does represent the absolute numbers but might not be completely representative of the community of origin of the participants. We had, however, to make a choice based on feasibility and ethical acceptability. Since women are being extensively questioned about traumatic experiences with a potential for re-traumatization a randomized design did not appear ethically suitable for the investigated population. Free choice and consent, as well as general willingness to participate were even more relevant than for a less emotionally-engaging research topic. Furthermore, feasibility has also to be taken into account. Performing research in an acute crisis situation, with almost one million displaced people entering a country in a short amount of time is challenging, especially, if a multi-center study has to be conducted. The recruitment process in such a situation is hampered by structural challenges (differences in local ethics requirements for performing research on displaced individuals, gatekeeper function of the shelter administration, willingness to offer locations for the performance of the study by the shelters etc.) and practical challenges (time-coordination with the women, presence of translators who have to give priority to acute medical or bureaucratic needs over support for research projects, childcare during interviews etc.), which are much more acute, complicated and challenging than in chronic settled situations. This needs to be taken into account. We added a paragraph about the limitations (e.g. potential over or under-reporting of distress etc.) and the factors involved to the Discussion (page 13).

From my reading of the methods, it appears the authors considered the correlation between individual traumatic experiences and sociodemographics and mental health outcomes, which allowed them to determine which variables should be included in the regression. The authors assessed over 20 traumatic events. I'm not sure the extent to which a correlation between the occurrence of a single traumatic event and mental health symptoms is meaningful, especially given trauma exposure tends to cluster in this population. I did wonder why the authors did not take a dimension reduction approach, identifying sub-categories of trauma exposure which may provide more robust variables within which to conduct these analyses.

Thank you for this question. Single associations of events with mental health have been widely reported in the literature and reviewed (e.g. Steel Z et al., JAMA, 2009) as well as dimension reduction approaches, which have, however, been mostly applied for validation of questionnaires or to prove inter-population reliability (e.g. Vindbjerg et al., BMC Psychiatry, 2016; Wind TR et al., Eur J Psychotraumatol, 2017). In our case, a dimension reduction approach would not have allowed to define the most relevant single factors correlated with overall quality of life, as this was the analyzed outcome in the current study. We specifically chose to investigate quality of life in this manuscript to allow for comparison with other European studies using a dimension applicable to the entire population as well as control groups – quality of life – compared to one that might sub-select our sample – such as mental health, which is not commonly assessed e.g. in national statutory surveys. If we had chosen a dimension reduction approach, we would most likely have identified broad categories such as e.g. physical trauma to others or self, lacked necessities, abduction of family member or friend and prosecution (these are taken from Arnetz BB, J Imm and Minority Health, 2014) and our study population would most likely have fallen within most of them. From a practical standpoint, however, it might be more relevant for a health care worker to know that the specific absence of health care (and not any form of deprivation) or the attack by a family member (rather than generic trauma to self) correlate with a worse
subjective perception of quality of life. Of course, we cannot establish any form of causality within the current sample, but clear association will aid us for the future establishment of this. Last, quality of life assessment is also part of the statutory German national health surveys, which will include a fraction of the refugee population in the future allowing for comparison with the current data.

Overall, this was an interesting study that investigated an important issue. I believe it could represent a potentially important contribution to the literature if the authors are able to address the queries raised above.