Author’s response to reviews

Title: Social support attenuates the link between torture exposure and post-traumatic stress disorder among male and female Syrian refugees in Sweden

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Author’s response to reviews:

Dear Editor of BMC International Health and Human Rights,

Thank you for the opportunity to submit a revised version of our manuscript “Social support attenuates the link between torture exposure and post-traumatic stress disorder among male and female Syrian refugees in Sweden” (IHHR-D-18-00166) for possible publication.

We are grateful for the comments on our manuscript and have revised the manuscript accordingly. We resubmit the revised manuscript in two versions; one ‘clean’ and one with tracked changes. Our responses to the reviewer’s comments, point by point are included at the bottom of this letter.

We would like to highlight one comment by the statistical reviewer in which it is stated that “To claim model fitness, the chi-square statistic need NOT be significant.”. We fully respect this comment but would like to point that the scientific community is divided on this issue and there is in fact not a consensus on this approach. We reviewed the 20 most recently published BMC Medical research Methodology and BMC Public Health papers which have reported the fit indices of CFA, and found that only one study with very small sample size (n=107) adhered to this statement. We have addressed this in more detail in our response to the reviewer. We have added a comment acknowledging the reviewer’s comment in the discussion section and hope it will be satisfactory.

All co-authors have read and approved the revised version of the manuscript.
We are looking forward to hearing from you.

On the behalf of the authors,

Maria Gottvall

Reviewer’s comments

"The authors mentioned on page 7 that postal questionnaires were distributed to a group of 4000 respondents. Meanwhile, only 34% of participants responded to the survey. The response rate was extremely low and it is important for them to explain and address the low response rate."

- Thank you for pointing this out. We agree that the non-response is a limitation of the study. We have outlined and addressed this more clearly under Strengths and limitations. Given that newly resettled refugees comprise a hard to reach population for survey studies, and since the present study’s sample constitutes a unique case compared to many studies on refugees in high income countries (which are based on highly selected small samples) both in terms of random sampling and the size of the sample which corresponds to 12.6% of a known and completed sample frame (N=9662), we still hope that the study provides valuable results despite the low response-rate.

"The authors did not explain in detail how respondents were sampled. Was the sample random? I ask because a major assumption underlying standard regression techniques including structural equation models is that samples need to be selected randomly. If this assumption was relaxed then the authors need to provide strong justification as to why they applied this technique to a non-random sample."

- Thank you for pointing this out. The study was based on a randomly drawn sample by Statistic Sweden from the Total population register, covering every individual that has resided in Sweden on a permanent basis. We have clarified this more in the text under Study design and participants.
and we have added it to the Abstract. It is also addressed as a strength under Strengths and limitations.

"The authors mentioned on page 8 that their scale on social support is a seven-item instrument. On six of the items, the response is a five-point Likert scale. The last item was a binary variable that asked respondents whether they were currently married or living with partner. This is quite odd given the inconsistencies in the levels of measurements. Did the authors then dichotomize the five-point likert-scale items in creating their scale on social support? How did they handle this inconsistencies in levels of measurement?"

- We agree that combination of Likert scales and a binary indicator represent an inconsistency. The particular combination has been primarily suggested by the original authors [1] as a means for scoring ESSI and in order to identify those with low social support. Other scoring methods based on the Likert-scale items have been suggested and used since [2,3]. All these methods rely on summary score which discards measurement error and item loadings on the latent variable of social support. In our analysis we have treated social support measured by ESSI as a latent variable in order to account for e.g. the measurement errors and differential item loadings. This improves the measurement inferences over simple summary scores. In this way, we have not allowed the inconsistency in the 7th item to affect the factorial structure of the 6 Likert-type items. Our strategy to handle this inconsistency is in line with Borsboom et al [4] account for reflective and formative latent variables. The 6 items included in our analysis represent reflective indicators of social support, and the 7th item, in fact, represent a different formative indicator (the cohabitation status cannot be assumed to be “caused” by social support, rather to contribute to it).

In our analysis, we assessed the fit of a model consisting of 6 items which are consistent in the level of measurement, and used the 7th item throughout the proceeding analysis as a formative indicator. Rather than dichotomizing the items which would lead to downgrading the information inherent in the Likert-type items, we have allowed the analysis to estimate the formative link between the 7th item and the latent variable of social support. The measurement model accessed is, thus, the latent reflective variable constructed of the 6 Likert-type items.

We have clarified in the discussion that the analysis targets the 6 Likert scale items to handle the inconsistency in measurement levels, and have stated this explicitly in the methods section.
"Several indices are often used to determine goodness of fit for structural equation models. The author used the Satorra-Bentler (SB) chi-square statistic as one of the many fit indices to demonstrate model fitness. While they claim their models indicate adequate fit, results from the SB chi-square show otherwise. To claim model fitness, the chi-square statistic need NOT be significant. This means a failure to reject the null hypothesis of no difference between the predicted and estimated models. With a significant chi-square, they reject the hypothesis of no difference and claim there is a difference between their predicted and estimated models. This is an indication of lack of fitness (see page 12). Their sample size is not too large to claim, this statistic was affected by the size of the sample."

- We do acknowledge the that the $\chi^2$ goodness of fit assesses the extend of discrepancy between the sample and fitted covariance matrices [5], and as such should be considered a measure of perfect fit. As Prudon [6] expresses it “… $\chi^2$ test, the assumption is that, on the population level, the approximation discrepancy is zero (the prediction is perfect)”. However, and given that this statistics (calculated as the product of $(N-1)$ and minimum fitting function) it is also most sensitive to increasing sample sizes.

We are sure the reviewer agrees that there is a great debate on SEM communities about the appropriateness of $\chi^2$ statistics for analysis with increasing sample sizes. To cite David Kenny [7] “For models with about 75 to 200 cases, the chi square test is generally a reasonable measure of fit. But for models with more cases (400 or more), the chi square is almost always statistically significant.” This issue is addressed and debated in a number of papers e.g. [8].

We do not aspire or are able to settle this ongoing statistical debate but can only state our position in this regard. In fact, in reviewing the 20 most recent published BMC Medical research Methodology and BMC Public Health papers which have reported the fit indices of CFA analyses in the main results, 16 studies did not report $\chi^2$ or its associated p-values [9-24], 3 studies reported $\chi^2$ (2 of which significant) but did not interpret it as indication of misfit and clearly stated its sensitivity to sample size [25-27] . Only one study [28] reported a non-significant $\chi^2$ and used it as means of evaluation of the model. This study, however, had a small sample size of n=107.

In this regard, and given our sample size that is approximately 3 to 4 times larger than has been suggested for CFAs (e.g. [29] ) and given the excellent other fit indices for both perfect fit (RMSEA) and Incremental fit (CFI), we hope the reviewer finds it satisfactory that we address
the debate about the Absolute vs Incremental fit indices in discussion section, and that we only claim adequate fit and not perfect or excellent fit to the model.

"Why are the authors silent on the other variables introduced as controls in their models?"

-Apart from gender, which has been used as a moderator, the cohabitation status has been used as a formative indicator for social support rather than a control variable. Our only non-addressed control variable is age in years. As the previous research has been clear about the associations between age and PTSD [30], and to limit the scope of the results reporting that is already fairly extensive, we have decided not to address this finding more extensively.

References


