Author's response to reviews

Title: Associations between childhood maltreatment and emotion processing biases in major depression: results from a dot-probe task

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Author's response to reviews:

Response to the Reviewers
We are grateful for the Reviewers' interest in our work and appreciate their valuable feedback that improved significantly the quality of our manuscript.

Reviewer #1
General remarks: This paper represents a useful addition to the literature on the complex subject of emotional processing in depression plus childhood trauma.

Comment 1: Given the lack of a healthy (non-depressed) control group with measures of CT the findings cannot be claimed to be specific to CT in depression and the authors could make this limitation clearer. The authors claim that they may have demonstrated the bias in this group because the bias is only demonstrated in conditions such as depressed mood (discussion p13). However, they cannot really claim this without an equivalent non-depressed group.

Our response: We fully agree that the lack of a healthy control group is an important limitation of our study and that our explanations are only speculative. We edited the text to highlight this limitation and underlined the hypothetical character of our argumentation; please see our revised Discussion [p. 14, lines 8-9].

Comment 2: Lack of similar findings in previous studies in non-depressed subjects may relate to lack of power.

Our response: This is an excellent point and we thank the Reviewer for bringing this up. In our revised discussion section we included this interpretation of previous findings [p. 14, lines 11-13].

Comment 3: The study is small and the finding of a relationship between 3 of the subscales but not the other 2 and bias to sad faces may simply relate to power.

Our response: We absolutely agree with the comment that our small sample size is a major limitation and that low statistical power has to be emphasized as a possible reason for non-significant results. Reviewer 2 suggested a conservative significance-level of # = .01 to account for multiple testing. After #-adjustment, we
did not find any significant differences regarding the strength of correlations of CTQ subscales and attentional bias to sad faces [p. 11, lines 22-25]. We acknowledge these points in our revised discussion section [p. 16ff, lines 25ff].

Comment 4: The authors should emphasise further the limitations of a small heterogeneous sample with multiple co-morbidities and various medications.

Our response: Following this important advice, we added statements to our discussion to point out the potential influence of medication on attentional biases and that we did not control for this possible confounding effect [see p. 13, lines 22-25, and limitations section p. 17, lines 8-12]. Additionally, we conducted analyses to compare medicated and non-medicated patients with respect to attentional bias and childhood trauma scores but found no significant differences between groups [p. 11, line 8]. We revised our limitation section and highlighted the fact that our sample was heterogeneous and small [p. 17, lines 4-7].

Comment 5: They should also comment on the relatively low HDRS scores.

Our response: Thank you for raising this question. The treatment program of our department is primarily designated for individuals suffering from rather moderate psychiatric symptoms. Typically, patients have to accept a waiting-period and sign a therapeutic contract assuring the absence of suicidal intentions. Please see our revised discussion section for explanations of rather low HDRS scores in our sample [p. 17ff, lines 25ff].

Reviewer #2

General remarks: I believe this is a well written and timely article; however I have some suggestions to improve the manuscript:

Comment 1: Although the inability to control for medication status is a problem in any cognitive study, I would suggest running some subgroup analysis comparing individuals on vs off antidepressant medication to determine whether this makes any difference to the results. I realise this analysis will be underpowered, but it may still provide an indication of the influence of medications in this sample.

Our response: Following the suggestion of the Reviewer we conducted additional analyses comparing medicated and unmedicated patients with respect to attentional bias and childhood trauma and found no significant differences between groups [p. 11, line 8]. As the Reviewer pointed out, lack of significant findings might be due to underpowered analysis. In our revised paper we acknowledge that ... “it is important to note that the majority of patients received antidepressant medication and we could not control for possible influences of different dosages or types of medication on attentional biases.” [see p. 13, lines 22-25].

Comment 2: Can the authors provide a breakdown of the number of individuals who have experienced past trauma vs those that hadn’t?

Our response: We very much hope that we addressed the Reviewer’s request correctly by inserting information about adult trauma exposure that was documented during the PTBS section of the SCID-I interview [p. 7, lines 20-22]. We administered no other questionnaires assessing recent traumatic experiences. The List of Threatening Experiences Questionnaire (LTE-Q) rather
measures the occurrence of stressful events, such as the loss of significant others, financial problems or unemployment, but not traumatic experiences in a narrower sense. In our revised methods section we clarified this point by adding the sentence “The LTE-Q encompasses life events such as severe illness, loss of close family members, financial problems or unemployment.” [p. 8, lines 24-25]. Moreover, we mentioned the lack of a questionnaire assessing past trauma as a limitation in the revised paper [p. 17, lines 23-25].

Comment 3: The correlational and hierarchical regression analyses should be bonferroni corrected or corrected at least to a conservative level of p<.01 given the number of comparisons.

Our response: We agree with the Reviewer that a more conservative significance level is reasonable given the number of tests. Following the Reviewer’s suggestion we corrected for multiple testing introducing a significance level of p<.01 (please see our revised methods section [p. 11, lines 11-13]). We revised our results section accordingly [p. 11ff] and deleted hierarchical regression analyses for attentional bias to happy faces, since the correlations were only marginally significant after #-adjustment. Hence, we dropped Table 5 from our manuscript. The correlation between attentional bias to sad faces and emotional neglect did not survive correction for multiple testing and we changed our reporting of this finding. In our revised paper we do not report correlations with happy bias in the abstract [p. 3]. However, to keep our results and discussion comparable to those of previous studies in this field, we think it is reasonable to mention the expected (and easily to interpret) negative correlational trends between emotional maltreatment and attentional bias to happy faces in our study [p. 15, lines 16ff].

Comment 4: The frequency breakdown of education should be given in Table 1.

Our response: Following the Reviewer’s suggestion we improved reporting of education information in Table 1 [p. 29].

Comment 5: Is illness duration referring to months since diagnosis or months since symptom onset?

Our response: We thank the Reviewer for drawing attention to a source of possible misunderstanding. In our revised Table 1 we included a more precise labeling (months since symptom onset) [p. 29] to clarify the meaning of the variable.

Comment 6: Theoretical ranges should be listed for the self-report measures.

Our response: This is a good point. We added theoretical ranges of scores for all questionnaires to the methods section [p. 8].

Comment 7: Abbreviations of all scales need to be listed in full beneath each table.

Our response: In our revised Tables 3 [p. 32] and 4 [p. 33] abbreviations are presented in full.

Comment 8: All regressors should be listed in Table 4 and 5 so that readers can view the change in beta.
Our response: We completely agree that detailed information about the beta-values of all regressors can be of interest for the reader. For the sake of clarity and simplicity we refrained from adjusting Table 4 in the manuscript. However, as noted below Table 4 [p. 33], we provided an Additional File 1 with detailed information about beta-coefficients and partial eta-square (see also Comment 9) for all predictors of the model.

Comment 9: Please provide the partial eta square value of each predictor.

Our response: We appreciate the Reviewer’s interest in more detailed reporting of statistical information and included a column for partial eta-square in Table 4 [p. 33].

Comment 10: The lack of control group limits.

Our response: We totally agree with the Reviewer’s statement that the lack of a healthy control group is an important limitation of our study. This limitation is highlighted in our revised discussion where we emphasize the speculative character of our conclusions; please see [p. 14, lines 8-9].