Author’s response to reviews

Title: Attentional Avoidance in Peer Victimized Individuals with and without Psychiatric Disorders

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Author’s response to reviews:

Dear Dr. Navarro,

Thank you very much for your evaluation of our manuscript and for sending the comments. We have edited the manuscript accordingly. The changes are marked as bold text in the revised manuscript.

We hope that the changes meet your expectations.

Yours sincerely, on behalf of the authors,

Benjamin Iffland

Reviewer reports:

Andrea Pozza (Reviewer 3): I believe that the statistical analysis is described clearly and well-established. However, I highlight some important points which the authors should address to make clearer the methods and to improve the consistency between methods and objectives.

→ Thank you very much for the evaluation of our manuscript. Please find below our responses to your concerns/comments.

Methods
Page 12 row 25. I suggest to remove the exclusion criterion (b) related to "evidence of current psychosis" for the clinical group, as it is not consistent with the group characteristics: 4 participants had schizophrenia paranoid subtype or schizoaffective disorder (see Table 1).

→ Since the presentation of the exclusion criteria was not entirely clear, it was revised.

Page 10, lines 10ff: “Exclusion criteria for the clinical sample included (a) evidence of a current substance abuse or dependence, (b) evidence of current active-phase symptoms of psychosis as delusions, and hallucinations, and (c) evidence of acute suicide intention or ideation.”

Page 17 row 5-10. Please, provide a statistical test value supporting the following sentence: "Error rates did not differ between the two samples. Out of 240 trials, participants indicated between 0 and 17 wrong colors (clinical sample: M = 5.65, SD = 4.17; control sample: M = 6.29, SD = 4.26)."

→ statistical test values were added.

Page 15, lines 3ff: „Error rates did not differ between the two samples. Out of 240 trials, participants indicated between 0 and 17 wrong colors (clinical sample: M = 5.65, SD = 4.17; control sample: M = 6.29, SD = 4.26; t(57) = .58, p = .562).”

Page 18 row 32-9. Please, add a statistical test value justifying the following statement: "Error rates did not differ between the two samples. Out of 480 trials, participants indicated between 0 and 38 wrong locations (clinical sample: M = 10.52, SD = 10.12; control sample: M = 8.13, SD = 5.71)."

→ statistical test values were added.

Page 16, lines 15ff: „Error rates did not differ between the two samples. Out of 480 trials, participants indicated between 0 and 38 wrong locations (clinical sample: M = 10.52, SD = 10.12; control sample: M = 8.13, SD = 5.71; t(56) = 1.11, p = .273).”

Page 19 row 24-9. I suggest to explain more thoroughly the rationale why "To be included in the high peer victimization group, participants had to score higher than the median (FBS total > 11; n = 28) on the FBS [89]". Why was this score chosen?

→ Unfortunately, the FBS lacks a representative norm sample and validated cutoff scores for peer victimization. Therefore, using a median-split of the FBS was a first approach to categorize samples into high vs low peer victimized participants. However, with this approach we were able to indicate peer victimization dependent differences in psychopathology and psychophysiology in previous studies (Iffland et al., 2014, Frontiers in Psychiatry; Sansen et al., 2015, Psychophysiology; Iffland et al., 2018, Biological Psychology). Within the current dataset, we
conducted additional ANCOVAs using the FBS as a continuous variable. As the patterns of results did not change, we decided to keep the 2 x 2 ANOVA design suggesting that the influence of peer victimization on attention was easier to reconstruct in the depiction of two groups.

However, we added this information to the manuscript to explain the rationale for the use of the median split.

Page 17, lines 12ff: „To date, the FBS lacks a representative norm sample and validated cutoff scores for peer victimization. In line with previous studies (81,91,92), therefore, a median-split of the FBS was used to categorize the samples into high vs low peer victimized participants.“

The Eta squared index (η²) was used as a measure of effect size but the criteria to interpret it are not mentioned in the Methods. In the Discussion section page 24 row 26-8, the authors state: "effect sizes of the significant effects were medium". I suggest to mention the criteria to interpret its value in the Data reduction and Statistical Analyses paragraph. For example, see Olejnik and Algina (2003).

→ we added the interpretation criteria suggested by Cohen (1988) to the Statistical Analyses section.

Page 18, lines 1ff: „For the ANCOVAs, partial eta-squared (η²) values were reported to demonstrate the size of effects such that 0.01 represents a small effect, 0.06 a medium effect, and 0.14 a large effect [107].“

Results

- Page 20 row 7. Please avoid sentences like "Table 1 presents participants' means [...]" and replace all of them with "Participants' means are presented in Table n" throughout the manuscript.

→ this sentence was edited accordingly.

Page 19, lines 4f: „Participants’ means on the assessments and diagnoses are presented in table 1.”

- Page 20 row 17-33. The authors have conducted a series of repeated-measure ANCOVAs testing main or interaction effects of several predictors. However, some of these interaction effects have not been justified sufficiently in the Objectives section (e.g., Valence x Group x Peer victimization interaction effect is not mentioned). Please, provide a rationale in the Objectives section for all the effects analysed by the ANCOVAs or remove those which are not mentioned in the Objectives or those not based on the literature.
Admittedly, the analyses mentioned by the reviewer (i.e., Valence x Group x Peer victimization interaction effect in the initial omnibus ANCOVAs) were not reasoned in the objectives. However, these omnibus ANCOVAs were explicitly requested in a previous review of the current manuscript. Therefore, for now we decided to keep the analyses. However, if the editor decides that the manuscript would benefit from removing the analyses, we will address this in a next step.

- It should be noted the small sample size of the study; the analysis appears under-powered here. It seems that they did not conduct an a-priori power calculation to determine the requested sample size. In addition, the analysis appears weak because the control group was not matched on age against the clinical group (i.e., the two groups were significantly different on age); so the effect of age had to be controlled for by the analysis. This should be highlighted as a limitation.

Indeed, the study is limited by the small sample size and sample composition. We added this limitations to the manuscript.

Page 27, lines 4ff: “Additionally, the current study is limited by the small sample size and sample composition. With a total of 61 participants, the analyses may have been underpowered to reveal potential effects. Next, the clinical sample and the healthy control sample differed in age which had to be controlled for in the analyses again weakening the power of analyses. Moreover, the sample was relatively young, with subjects who are predominantly single. Limitations in sample size and composition should be addressed in future studies using larger and more representative samples.”

- It seems that childhood maltreatment and trait anxiety have been tested as covariates, then removed from the analysis, but they do not seem to be mentioned in the Objectives. Please, clarify.

We added a rationale for running ANCOVAs with childhood maltreatment serving as covariate to the Statistical Analyses Section:

Page 17, lines 21ff: “Additionally, because the clinical sample and the healthy control sample differed significantly on some subscales of the CTQ, all ANOVAs were carried out as analyses of covariance (ANCOVAs) with age and the CTQ sum score serving as covariates to control for the influence of childhood maltreatment within the family.”

We removed the ANCOVAs with trait anxiety serving as covariates and deleted the following: “Similarly, all ANOVAs were carried out as analyses of covariance (ANCOVAs) with age and the trait anxiety score serving as covariates to control for a general effect of anxiety on attentional processes.”

- In addition, as the authors tested many effects, I suggest to provide a summary of all the ANCOVAs effects in a Table. The Beta coefficients of each tested effect are not given
and they should be provided in the Table to make the reader more familiar with the effect direction.

→ As requested, we added tables to summarize all effects of the ANCOVAs at a glance. The results section of the manuscript was edited accordingly. However, for some reasons we decided not to report Beta coefficients. First, we think direction of effects is best depicted in figures and tables reporting mean RTs of various sub-samples (tables 5 & 7). Second, in our opinion, reporting of betas is not suited for categorical variables because the direction of beta strongly depends on scoring of categories (i.e., direction changes if clinical sample is scored by 2 instead of 0, while control group is scored by 1). Lastly, because age was not significantly associated with bias scores in any of the ANCOVAs (all p’s > .05) reporting of the direction of this null-effects is no longer relevant. We hope you can follow our reasoning. Of course, if the editor decides that the manuscript would benefit from reporting beta coefficients we will add them in a next step.

Conclusion

- I suggest the authors to mention that the sample size was small for the type of analysis

→ see above

- Page 27, row 50-5. Please remove or revise the following sentence: "With respect to the results of the present study, it may be suggested that peer victimization in and of itself is associated with a risk for developing biases in emotion processing", as statistical analysis/research design do not allow to draw this conclusion about the risk developing biases.

→ we edited this sentence accordingly:

Pages 27ff, lines 26ff: „With respect to the results of the present study, peer victimization in and of itself is associated with biases in emotion processing.”

Table 1

- Please, report the p-value when it is not significant instead of "n.s."

- Please, add the statistic test value for each comparison (e.g., independent Student t test)

→ the requested values and information were added to the table
Fernando Marmolejo-Ramos (Reviewer 4): as per requested by prof navarro, the authors have added details. as a minor detail, footnote 'a' in the table 1 should read 'chi-squared test' having said this, a final proofread wouldn't come amiss

→ Thank you for this note. We edited the footnote and proofread the manuscript.