Author’s response to reviews

Title: Cognitive predictors of treatment outcome for exposure therapy: Do changes in self-efficacy, self-focused attention, and estimated social costs predict symptom improvement in social anxiety disorder?

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Author’s response to reviews:

Dear editor and reviewers,

Thank you for the opportunity to revise our manuscript. Below you will find our detailed response to each comment.

On behalf of all co-authors,

Isabel Kampmann

Angela Fang (Reviewer 1): Although the authors have addressed my previous concerns, there is new information in the manuscript that are unfortunately raising some new concerns about the findings. I hope the following suggestions improve the interpretation of the findings and the clarity of the implications.

Re: treatment completers, it seems arbitrary to define treatment completers as completing at least six sessions in a ten-session protocol. I think there still needs to be a clearer rationale for this, as Reviewer 2 recommended. In addition, this definition would be more justifiable if there was no difference in outcomes between those who completed 3 exposure sessions compared to those who completed only 4. Please assess whether there is a difference based on number of exposure sessions completed.

Our response:

We thank the reviewer for her comments. In response to the reviewer’s comment, we report now on Page 6 “We defined treatment drop-out as terminating therapy before at least 50% of the
exposure sessions (i.e., at least six sessions) are reached. In literature, different definitions of treatment drop-out are used (Connell et al., 2006) and our decision was based on the expectation that some patients might not need to attend all ten sessions. This procedure has been also applied in other trials (A-Tjak et al., 2018; Masi et al., 2003).” (p.10-11, line 256-261)

Due to the small number of participants who finished exactly 6 sessions, an analysis comparing completers with only 3 exposure sessions with completers who completed 4 exposure sessions would be underpowered. However, as reported in Kampmann et al. [25], we conducted both completer analyses and intent-to-treat analyses. We found that the completer sample did not significantly differ in treatment efficacy from the intent-to-treat sample and we therefore reported the (more conservative) intent-to-treat analyses. In response to the reviewer’s comment, we now report in the manuscript: “As reported in Kampmann et. al [25], treatment completers and dropouts did not significantly differ on demographic characteristics or outcome measures at preassessment. Furthermore, the completer sample did not significantly differed from the intent-to-treat sample in treatment efficacy.”(p. 10, line 261-264)

References:


Many of the results reported within the text are redundant with the text reported in the new Table 2. I don't think it's necessary to repeat the statistics within the text in the Results section.

Our response:

In response to the reviewer’s comment, we deleted the reported statistics from the results section of the manuscript and now refer to Table 2 for the regression equations: “See Table 1 for means and standard deviations of all predictor variables and Table 2 for regression equations.”

I agree with Reviewer 2's concern that the treatment term needed to be included in the model when assessing the treatment x change in cognitions effect on LSAS scores at post-treatment.
However, with the product term in the model, the main effect of treatment in these models become conditional effects and cannot be interpreted as significant main effects, as described on p.11-12. As such, attempts to interpret the significant main effects in these models as demonstrating an effect of treatment type on LSAS scores need to be removed from the Results and Discussion sections (+ limitations paragraph on p.15).

Our response:

As suggested by the reviewer, we now refrain from interpreting the main effects of treatment and deleted related sentences from the abstract, results and discussion section in the manuscript. (p.2,11,12, 15)

Because the treatment term cannot be interpreted as main effects in Table 2a-2c, it's not justified to include treatment as a predictor in the "final" regression model in Table 2d (nor is it necessary given that this wasn't a primary research question of the study).

Our response:

In response to the reviewer’s comment, we conducted the regression analysis for the final model excluding the treatment term. Accordingly, we now report the results of this analysis in Table 2.

Therapy "rationale" is spelled incorrectly at the bottom of p.7. Also, "self-focused" attention is spelled incorrectly on p.12.

Our response:

We thank the reviewer for reading the manuscript so carefully; we corrected the two typing errors on page 7 and 12.

In Table 2, I would not highlight a p value of .051 as being statistically significant.

Our response:

As suggested by the reviewer, we removed the bold-face type for the p-value of .051 in Table 2.

Amanda Morrison (Reviewer 2): The authors were thorough and attentive to their edits and I believe the paper is much improved. However, I have one significant reservation which is based on the revealed internal consistency statistics for the predictor variables. For the SESS, there was questionable internal consistency (Cronbach's alpha = .69) and for the SCQ there was poor internal consistency (Cronbach's alphas = .56 and .51). Given that reliability for difference scores is typically worse than for the component scores (e.g., Edwards, 2001), I have significant concerns about the reliability of these two change scores, particularly given that the most powerful predictor of treatment outcome was change in SCQ. I have two sub-questions/comments:
1) Given that internal consistency was questionable to poor for these scales, I believe readers would be interested to know the internal consistency coefficients for the Time 2 assessment points (I assume the reported coefficients are for the pre-treatment assessment point). If Time 2 internal reliability is similarly questionable to poor, my concerns about the reliability of these difference scores remain.

Our response:

In response to the reviewer’s comment, we calculated the internal consistency for Time 2. In the process, we detected an unfortunate error in the previous calculation of internal consistency. Therefore, we recalculated the internal consistency for all measures and now report the correct values for both Time 1 and Time 2 in the manuscript (Cronbach’s α FAQ=.76-.81; SESS= 75-.81; SCQ=.81-.93) (p. 6-7). Given the rather high internal consistencies for all three measures, we believe that we could resolve the reviewer’s concerns about the reliability of the change scores.

2) I am also a little confused as to why the authors calculated two alphas for SCQ but used only one SCQ score. It seems the two alphas correspond with 10 items each for the performance and nonperformance items of the 20-item SCQ, but the authors state that they used a "mean score of performance and nonperformance items" for this measure. Therefore, it seems that instead of calculating separate alphas for items that fall into these two categories, the authors should just report a single alpha of all items pooled.

Our response:

As suggested by the reviewer, we now report the internal consistency across subscales (Cronbach’s α =.81-.93) (p.7, line 169-170).