**Reviewer’s report**

**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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**Reviewer:** Amanda Morrison

**Reviewer’s report:**

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Review of "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?"

The authors report one study in which 60 participants with SAD completed either in vivo or virtual reality exposure therapy. Three types of cognition were assessed before (or at session 3) and after the first six sessions of therapy and change in these cognitions were used to predict post-treatment social anxiety (after controlling for pre-treatment anxiety). The cognitions included self-reported self-focused attention, self-efficacy in social situations, and estimated social costs. Although change in each cognition predicted treatment outcome if alone in the model, only changes in estimated social costs predicted treatment outcome above-and-beyond other predictors.

1. The paper would benefit from a sharpening and strengthening of the motivation for the analyses. There are a few issues. The first has to do with the idea of "prediction" which is how the analyses are framed. This seems slightly inaccurate, as the "predictors" are measured in an overlapping time frame as the outcome (although the outcome is post-treatment LSAS, pre-treatment LSAS is controlled for, therefore making the outcome akin to change in LSAS from pre- to post-treatment). If the analyses were framed as "association between early changes in [variables] and treatment outcome" then it would be more accurate.

2. The second issue with the motivation for the analyses is that it is unclear why the authors motivate their analyses by referring to tests of mediation (eg, Hofmann et al). Tests of mediation are conceptually and statistically different from what the authors do in the paper, but this is not made clear in the Intro or Discussion.

3. Related to #2, it is unclear why the authors do not test mediation, which seems to be a stronger approach to test what the authors are after. If the authors test mediation, then the current framing of the rationale for the study (ie, as focused on extending prior research
to the context of individual (vs group) therapy and to individuals with varied social fears (vs just public speaking fears) would fit better. If the authors maintain the focus on association/prediction, then it seems the primary rationale for the study should be that this hasn't been examined yet using these variables, in addition to the fact that the therapy is an exposure-only treatment not explicitly targeting cognitive change.

4. A final note related to framing in the Intro has to do with why the authors looked at change in cognitive variables as a predictor, rather than, for example, baseline cognitive variables as a predictor. It is currently unclear why the authors chose this approach and what it might offer readers (eg, could there be clinical utility to being able to predict treatment outcome based on degree of change in estimated social costs by mid-treatment?).

5. For the regression analyses, when an interaction term is included, all lower order effects involved in the interaction must be included, and these analyses appear to forget to include the effect of treatment. Because of that, both the interaction and the effect of cognitive change in the context of the interaction are not interpretable (and the lack of the treatment term might explain why the cognitive change effects become non-significant when the interaction term is included). As such, I cannot evaluate the results or conclusions related to moderation by treatment type.

6. The justification for choosing just before Session 7 as Time 2 is unclear. The authors explain that this was chosen because a treatment completer was defined as someone who completed at least six sessions, but that doesn't seem relevant since someone could have completed two sessions prior to Session 7 and then completed Sessions 7, 8, 9, and 10 and be considered a completer. So, by Session 7, this person would have completed perhaps one or no exposures, whereas a different person might have completed four exposures (Sessions 3-6). It seems more accurate to say that Session 7 was chosen because this marked a certain point in the treatment (it's just past halfway so I'm not sure what mark it would be) or a theoretically or practically important number of exposure sessions. Consistent with the latter option, the authors could decide to allow Time 2 to be individually-varying across participants, e.g., if the cutoff was three exposures, that could be prior to Session 7 for a number of folks or Sessions 8, 9, or 10 for others. At minimum, further explanation of this choice of Time 2 would be helpful.

7. For Table 2, are the presented intercorrelations of the Time 1 measures? If so, I'm unclear as to the purpose of providing these intercorrelations. It seems the authors may want to rule-out multicollinearity among predictors so they can conduct the multiple regression, but if so, intercorrelations should be between the change scores.

8. For the Participants section, it would be helpful to include basic demographics and comorbidities so the reader doesn't have to refer to another paper.

Minor points
9. There were a few sentences that were difficult to understand and would benefit from refinement. The two that stood out the most were the following:

a. P 4 unclear sentence: "For cognitive behavioural therapy, changes during treatment regarding self-efficacy in social situations and in the therapy context are associated with treatment outcome"

b. Discussion: second sentence, clarify that change during first 6 sessions in these cognitive variables was used as predictor (it wasn't that these variables themselves were used as predictors)

10. Intro P 4: it would help to spell out what the "contradicting results" were since this is central to the paper; why would one think that group format or public speaking anxiety would function differently in terms of predictors (or mediators) than individual therapy or other social fears?

11. Throughout the paper, some references are typed out rather than numbered

12. For the organization of predictors in the Method section, it would be helpful to use parallel structure as the Intro (self-focused attention first, then self-efficacy, then social costs)

13. Would be helpful to provide internal reliability coefficients of your measures in your sample.

14. It seems unnecessary to include the final sentence of the Method ("Furthermore, analyses were based on the aggregated sample...") since this is assumed by the fact that there's a moderator of treatment condition in the model.

15. In the first paragraph of the Discussion and in the Conclusion it seems that it should be emphasized that each variable predicted treatment outcome alone, but that only change in estimated social costs predicted when all three change scores were in the model. Right now, it sounds like the other variables didn't predict at all.

16. Table 3 note - it would be helpful to explain in the table note what the delta symbol stands for.

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If not, please specify what is required in your comments to the authors.

Yes

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If not, please specify which controls are required in your comments to the authors.

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