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?

**Version:** 0 **Date:** 30 Jul 2018

**Reviewer:** Angela Fang

**Reviewer’s report:**

This paper assessed whether certain cognitive maintaining factors of social anxiety disorder (self-focused attention, self-efficacy, and social costs) predicted response to two formats of exposure-based therapy (traditional in-vivo exposure and virtual reality). The authors tested each of the predictors in separate models, as well as in a final model which included significant predictors from the previous analyses, and reported that changes in social costs were the only significant predictor of social anxiety outcome beyond the other factors. I understand this is a secondary analysis from a randomized controlled trial testing the efficacy of virtual reality exposure therapy for social anxiety disorder and consider this novel treatment to be a strength of the study. However, there are a few methodological issues I have concerns about, which I describe below.

There seems to be no valid basis for combining all patients who were randomized to begin with waitlist with the other patients who received an active treatment first. Differences between these groups need to be checked beforehand. The most important confound here is whether waitlisted patients already improved (or even worsened) in their social anxiety symptoms prior to beginning treatment, in which case these patients cannot be treated the same as those who began treatment right away. This confound needs to be explicitly addressed.

Relatedly, the rationale given for combining patients from both active treatment conditions is due to power concerns (p.10). However, it seems worth clarifying that the purpose of the study was to assess cognitive predictors of pure exposure-based therapies, rather than whether cognitive predictors differentially predict outcomes from these two types of interventions. It is an advantage that treatment condition was included as a moderator in the regression analyses to additionally test whether there were differential effects based on treatment, although this could also be more clearly articulated in the analysis section too.

If patients were assessed before and after the waitlist period, the true test of the specificity of cognitive predictors for impacting social anxiety symptoms during exposure-based treatments and not during the waitlist period, would be the best evidence from these data that cognitive factors predict outcome in exposure therapy. Can this be looked at?

Did the authors include patients with past experience with CBT (not just within the past year)? If so, is it possible that symptom improvement varied differentially based on past CBT treatment,
and thus changes in social costs were more predictive of outcome for patients who had received CBT in the past?

Although reference to the original randomized controlled trial was made in the methods section, it may be worth repeating for ease of interpretation that both exposure therapies performed better than waitlist in reducing social anxiety symptoms.

Internal consistency for each of the cognitive predictor measures should be given based on the current sample, particularly because subscales were often used.

There is relevant literature missing from the introduction. In particular, Hofmann (2000) reported changes in self-focused attention after exposure therapy for social anxiety disorder:


References were reported somewhat haphazardly in the text. These should be revised to be numbered, in square brackets, in the order in which they are cited in the text, consistent with the format of BMC Psychiatry.

Are the methods appropriate and well described? If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls? If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown? If not, please explain in your comments to the authors.

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Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review? If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

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