Reviewer’s report

Title: Effects of Attentional Bias Modification on Residual Symptoms in depression. A Randomized Controlled Trial.

Version: 0 Date: 12 Oct 2018

Reviewer: Colin MacLeod

Reviewer’s report:

This manuscript reports a potentially influential study that addresses an important issue. The introduction convincingly conveys the scale of the problem, by reviewing the evidence that a substantial majority of patients treated for major depression continue to display residual depressive symptomatology following intervention. The study conducted by the investigators, to evaluate their intervention approach designed to attenuate residual depression, is impressive in scale, involving over 300 participants. Their candidate intervention is theoretically driven, targets a key attentional mechanism implicated in influential models of depression, and draws upon an approach that has been shown to be capable of modifying this attentional factor. Moreover, the intervention is low cost, and highly scalable, being delivered remotely to patients within their own homes. The findings provide encouraging support for the therapeutic value of this intervention approach. Consequently, I believe that this well-written manuscript warrants publications. My few concerns, which can readily be addressed through revision, are listed below.

i. It was unclear whether the 37 participants who were erroneously recruited despite meeting the exclusion criteria of having a current major depressive event were included in the data analyses reported in the manuscript. They should not have been included, but the final analysis presently reported in the Results section (on page 8) suggests that they might have been, as this analysis distinguishes between participants with and without major depressive disorder (MDD). It may be that this distinction refers to presence or absence of MDD subsequent to the intervention, but this is confusing. The final analysis is confusing for other reasons also, not the least of which is that the DV is not clearly specified, which leads me to suggest that it be excluded.

ii. The description of the task could perhaps be tightened a little. Referring to the probes as appearing "behind" faces is misleading, and potentially confusing. A probe that appeared behind a face would not be visible. Instead, I suspect that the probes actually appeared in the same screen location where a face was previously exposed, and I suggest that this be conveyed to the reader. The use of the terms "valid" and "invalid", to describe probes that respectively appeared in the locations where faces displaying positive or negative expression had just been shown, also invites misunderstanding, given that probes appearing in these two locations do not differ in terms of their validity. Instead, it would be more accurate (and informative) to distinguish these two types of probe condition by referring directly to their location, rather than to their validity (i.e. Probe Location: locus of the more positive/less negative face vs locus of the more negative/less positive face).
iii. I suggest that the information provided in the "measurement of attentional bias" section be slightly amended, by removing the statement that these 96 trials were "the same trial types as used in the ABM procedure", given that the mix of trial types within this AB assessment task was very different from the mix of trial types presented within the ABM procedure. In this same section, the provided equation would be easier to understand if the labels employed were more fully informative. I suspect readers may struggle to understand that "probe down" actually means "probe in lower screen position" or that "positive up" actually means "face in upper position displayed more positive emotion". Also, it's probably not helpful to refer to AB as having been measured "by a single session of the placebo training task", down in the "study outcome" section, which challenges the reader to understand how a placebo training task can also be an AB assessment task. Potential confusion would be reduced by describing the "AB assessment task" in the "measurement of attentional bias" section, and subsequently referring to AB having been measured by the "AB assessment task" in later sections also.

iv. I recommend that the authors clearly indicate within the manuscript the precise timing of the AB assessment task delivered at baseline (i.e., how long before the first ABM/placebo session?) and of the final AB assessment (i.e., how long after the final ABM/placebo session?).

v. No analysis presently is provided that informs the reader whether the AB measure was changed by exposure to the ABM condition (i.e. increased, as intended), and whether this change was greater in the ABM condition that was the case in the placebo control condition (also as intended). This analysis should be included. I note the abstract states that "ABM induced a change of AB towards relatively more positive stimuli", despite the fact that the analysis needed to justify this claim is not reported. Hence I anticipate that this analysis will confirm that AB was differential changed, as intended, by exposure to the ABM and placebo conditions.

Though I believe that addressing these few above issues could further enhance the report, I would repeat that I consider this to be an impressive study. The manuscript reports important findings, and the authors discuss the implications of these findings in a balanced and thoughtful manner. I am confident that this work will be of very considerable interest to many investigators, and will have a substantial impact on the field.

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

Yes

 Does the work include the necessary controls?
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