Reviewer’s report

Title: Internet-delivered attention training in individuals with social anxiety disorder - a double blind randomized controlled trial

Version: 1 Date: 29 January 2012

Reviewer: Jenny Yiend

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This is an important paper presenting an RCT reported in line with CONSORT guidelines, comparing the effects of internet delivered CBM attention training for social anxiety with a computerised control. The CBM community has been calling for such studies reported in this way. To date most are from the experimental tradition where the focus of interest has been basic research and mechanistic questions, rather than clinical efficacy. This is therefore a very welcome example of true translational research. Furthermore communication and dissemination of the disappointing findings is not only important, but I would argue, essential, to the field.

Major Compulsory Revisions

Several substantial points of improvement to the manuscript:

- The authors should conduct and report one or more post hoc analyses in addition to the intent to treat main analysis. This is an accepted addition that can provide more illuminating information for future studies, around factors that may improve chances of later success. As a minimum they should look at a) the dose-response relationship – do those receiving most training trials improve most? (clearly there is considerable information on exact number of trials received as well as sessions) - and b) predictors of success – eg starting attentional bias, or some proxy for it and perhaps other individual differences (eg continuous measures of severity) . Although both are mentioned in the text, they should be examined quantitatively.

- More information should be contained in the introduction regarding the cognitive mechanistic rationale behind CBM (attention) and reference made to the large body of experimental manipulation studies that have led to it, and before that the decades of experimental work examining the naturally occurring prevalence of attentional biases. Demonstrating this linkage between basic research and their translational application is contextually important and of benefit to both domains of research.

- The analysis of change in attention bias (p10) is helpful and important, however the authors should also add results and discussion of correlations involving outcome measures and a) change in bias (ie pre to post bias scores or some proxy for it) and b) starting bias (see first point, above). Just looking at post intervention bias leaves a lot unexamined.

- Consider the implications of the fact that the intervention (and those before it)
only train attention away from negativity. Consider added benefits - in terms of efficacy - of additionally training attention towards positivity. Rather than training what is essentially avoidance, would it be an improvement to train active selection of positive mood-supporting information? This much-discussed point has received little coverage in the literature, but deserves it.

Minor Essential Revisions

Paragraph 2 page 3 line 5 refers to "subcortical processes" giving a misleading impression - there is no clear evidence that CBM is targeting subcortical processes. The fMRI attentional training study of Harmer and colleagues that the authors may be thinking of has many flaws, in the opinion of some. This reference to neural substrates should simply be removed.

P13 5 lines from bottom – the reference to disengagement should be removed since to date it is far from clear whether this effect replicates in clinical disorders. If the authors strongly disagree then strong evidence should be cited – ie reports of disengagement effects in clinical (not subclinical) samples. It is my understanding that there is only 1 such study at present together with unpublished replication failures. The point is not necessary for the present paper.

P14 last line. Rather than ‘to be altered’ which implies the authors are problematically concluding on the null hypothesis (rather than simply reporting their data), they might say ‘further investigation’

P12 line 4 – trails -> trials

P9 – please explain what a ‘first order autoregressive covariance structure’ is and what benefit it conveys for the analysis. How does it handle missing data and why is that better than other methods?

P8 – by whom was the follow up conducted?

P3 last line criteria of -> criteria for

In line with good practice in psychiatric journals, always report absolute Ns alongside %s

P3 para 2 – it is an overstatement that ABM is a computerised ‘treatment’ – it is arguably not established as such. Add a qualifier such as ‘potential’ or ‘putative’.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.