Reviewer's report

Title: Absence of evidence or evidence of absence: Reflecting on therapeutic implementations of attention bias modification

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Reviewer: Thomas Ehring

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The manuscript comprises a commentary on (1) an article published in BMC Psychiatry reporting on the results of an internet-delivered attention modification training (Carlbring et al., 2012) and (2) a commentary related to this article published in BMC Medicine (Emmelkamp, 2012).

In my opinion, the manuscript adds an interesting perspective to the discussion opened by Emmelkamp (2012). In contrast to Emmelkamp's earlier conclusions, the authors argue that ABM is a promising intervention that merits further investigation. They present a number of thoughtful and convincing arguments, which makes it an interesting manuscript to read.

On a general level, I think that a discussion of the promises and limitations of ABM is timely. I am therefore wondering whether it would make sense to publish the current manuscript in BMC Psychiatry alongside a reply by Emmelkamp (and maybe another reply by the authors of the current manuscript). Such a series of commentaries may help to further elucidate for the readers, which arguments are presented by both sides.

I have a number of suggestions on how I think the current manuscript may be further improved.

Major compulsory revision

(1) The literature reviewed by the authors appears somewhat selective. There are some studies that appear relevant but are not mentioned by the authors (e.g., Neubauer et al., 2013). It would be interesting to know what the exact number of clinical studies is showing significant vs. non-significant effects of ABM.

(2) On a related note, it may be preferable referring to meta-analyses reporting on the effects of ABM on symptoms of anxiety (e.g., Hallion & Ruscio, 2011) as they can be expected to better represent the overall evidence rather than selected single studies. Importantly, the results of these meta-analyses show significant but only modest overall effects of ABM on anxiety. This should be taken into account when discussing the clinical implications.

(3) In my opinion, the authors make a very convincing case that ABM is a promising intervention and that there is a clear need to further investigate it from an applied perspective. However, at such a general level this may not be such a controversial issue at all. In my reading of the critical commentaries on ABM,
there are more specific criticisms and doubts that I think the authors should focus more on:

(a) Do existing studies on ABM really meet the current standard for clinical trials (e.g. CONSORT criteria)?

(b) How do effects of ABM compare to those of existing treatments for CBT? Is there any evidence that they are superior to traditional CBT or at least not inferior?

(c) Are the conclusions regarding clinical implications drawn by ABM proponents really backed up by the data, when only focusing on studies including clinical samples (given that the large majority of ABM research has focused on non-clinical or sub-clinical samples)?

(d) One of the main issues raised by Emmelkamp (2012) was that it may be premature to adapt ABM for “internet-based treatment” and that other types of internet-based treatment for anxiety shows more promising results.

References


Emmelkamp, P. M.G. (2012). Attention bias modification: the Emperor’s new suit?. BMC Medicine, 10: 63.


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.

Declaration of competing interests:

I declare that I have no competing interests.