Author's response to reviews

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

Authors:

Patrick J. F. Clarke (patrick.clarke@uwa.edu.au)
Lies Notebaert (lies.notebaert@uwa.edu.au)
Colin MacLeod (colin.macleod@uwa.edu.au)

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Author's response to reviews: see over
Dear Dr Murray

Re: Manuscript number 7715253259079615

Title: Absence of evidence or evidence of absence: Reflecting on therapeutic implementations of attention bias modification; Patrick J. F. Clarke, Lies Notebaert, and Colin MacLeod

We were very pleased by your invitation to revise and resubmit this manuscript taking account of the comments and suggestions of the Reviewer (Dr Thomas Ehring). We appreciate the thoughtful feedback and we have been happy to follow the advice given to further improve the manuscript.

We were encouraged that the Reviewer shares our assessment that the content of this paper adds a valuable perspective to the discussion on the potential merits and limitations of Attention Bias Modification. As per the Reviewer’s suggestion we would only be too happy to have this included as part of a commentary series alongside a reply by Emmelkamp. In line with this we have sought to more clearly highlight the links between arguments made in the current paper and some of the assertions in Emmelkamp’s original commentary. We believe that this helps increase the clarity of the arguments being presented in the paper.

Please find below a detailed summary of the amendments that have been made in light of this very welcome feedback.

Reviewer Comments/Compulsory revision:

1. Expansion of literature review and number of studies with significant clinical findings
The Reviewer indicates that the literature review appears somewhat selective and it would be of interest to indicate the numbers of studies that have/have not found significant effects of ABM. The literature in the original manuscript was indeed somewhat selective as we had initially restricted our focus to the small number of studies also considered by Emmelkamp’s commentary. We are very happy to expand upon this to other, and more recent, findings that bear on the issues under consideration and we have done so in the revised paper. As to the question of the number of studies that have demonstrated significant effects of ABM; while a more systematic review of the available literature is obviously beyond the scope of this brief commentary, we have sought to compile a table of the studies that have implemented ABM with clinical, sub-clinical, and non-clinical populations that summarises whether the modification procedure was successful in achieving attentional change, and whether there was a significant impact on anxiety/emotional symptoms. This therefore permits a brief numerical breakdown of successful and unsuccessful ABM studies across clinical and non-clinical populations. This table also assists in illustrating one of our central points, in that when an ABM task succeeds in modifying biased attention for threat it also succeeds in modifying emotional vulnerability (and vice versa). This table is referred to in the section ‘What do recent failures suggest about the potential effectiveness of ABM?’. Given the size of this table we might suggest that this is best published as supplementary online material, however we are happy to be guided by your judgment on this.

2. Discussion of meta-analytic findings
The reviewer suggests it would be beneficial to refer to findings from meta-analyses which can represent the pooled evidence in relation to ABM. We agree that this would be a useful addition to the paper and we have added a subsection to address this. In describing the findings of these meta-analyses we have also sought to highlight their limitations. Specifically, the benefit of such meta-analyses is that they seek to be broadly inclusive of the available research, however, as with all meta-analysis, their limitation is that they may fail to make critical distinctions that are relevant. For
example, the Hallion and Ruscio study (2011) clustered both attention bias modification and interpretive bias modification within the same meta-analysis and do not reporting effects for ABM alone. Furthermore, it did not seek to sub-divide the various ABM studies in ways that would be informative. In this regard the meta-analysis by Hakamata, et al (2010) is somewhat superior in addressing questions relating to attention bias modification as it does divide ABM studies according to a number of meaningful criteria (e.g. effect sizes for clinical vs. non-clinical populations, and across variants of ABM tasks). As such we have reported the valuable insights that can be gained from these meta-analyses in relation to ABM while also acknowledging some of the limitations of exclusively relying on such techniques.

3. Addressing specific criticisms regarding ABM
We entirely agree with the Reviewer that the promising nature of ABM and the need for further investigation should not be a controversial issue. However, the motivation to produce this commentary was in part driven by the suggestion in the original Emmelkamp (2012) review that there is both a lack of robust evidence for attentional bias modification and that the current state of research into ABM suggests that there is no need to investigate this as an online treatment approach. Thus, a number of points in this paper are designed to address the alternative perspective on these issues. We agree also that there are a number of additional substantive issues that need to be addressed within ABM research and we have sought to acknowledge these in greater detail in the revised manuscript. Below is a detailed response to each of the issues raised for consideration.

a. Adherence of ABM research to standards for clinical trials
It is indeed the case that the far majority of ABM research does not meet the strict criteria for the standards of randomised clinical trials. Part of the reason for this is that ABM has emerged from a field of experimental research in psychology and is only more recently being incorporated into trials. We have now sought to highlight this problem in the section that considers the results of recent meta-analytic studies.

b. Comparison effect sizes for ABM vs. CBT
When describing the results of recent meta-analyses we have reported the estimates of effect sizes for ABM studies in both clinical and non-clinical populations. We now also include reference to how these compare to effect sizes for psychological interventions such as CBT. While these preliminary ABM studies compare favourably with effect sizes from traditional psychotherapeutic interventions, we also acknowledge that the computation of average effect sizes for ABM is currently based on a small number of studies (that mostly do not meet criteria for clinical trials) and should therefore be interpreted with caution.

c. Treatment benefits of ABM when focusing on clinical vs. non/sub-clinical samples
Wherever possible we have sought to highlight the distinction between the pattern of findings and effect sizes between clinical and non-clinical populations in the revised manuscript. What is consistent is the observation that when an ABM procedure succeeds in modifying selective attention, it also succeeds in modifying emotional vulnerability and this is equally true in clinical and non-clinical populations. What is inconsistent is the ability of existing ABM procedures to reliably produce such attentional change. We have also sought to clarify that our intention is not to argue that ABM currently represents a superior treatment approach, simply that present data provide strong support for the potential of this as a future intervention.

d. Adoption of ABM as an online treatment
We absolutely agree that it would be premature to adopt ABM as an internet-based treatment as there are other types of internet based treatments that are currently delivering better and more reliable results. This, and the fact that current ABM tasks do not yet achieve reliable change in
attentional bias across studies, means that at this stage, we would advise against the large scale adoption of ABM as an internet-based treatment and we have made this explicit when discussing the need for further research. We have also sought to explicitly outline our alternative position to Emmelkamp. In particular we describe our objection to the manner in which the original commentary represents current evidence for ABM and the suggestion that this provides a basis not to further investigate ABM as a viable online intervention. We have therefore amended sections dealing with the Background, Future research, and ‘Is there any need for attention bias modification as an intervention’. We believe that the result is a clearer communication of the arguments presented in the paper and the reasons for making them.

Once again, we warmly thank the Reviewer and yourself for your assistance in revising this manuscript. We have appreciated the quality and constructiveness of the feedback provided which we hope you agree has contributed to a higher quality final manuscript.

Kind regards

Patrick Clarke