The article provides a very interesting example of translational research, taking interesting findings from experimental psychology and undertaking an evaluation of the transferability into clinical practices. The authors have approached this task of translation in a commendable method utilising structured and largely reproducible processes. With a few further pointers, I believe that this article could merit publication, more for the exemplar of structured translation than for the impact of findings (which do not yet convince me of a strong likelihood of meaningful clinical benefit for MeST). The intervention described will require further evaluation of efficacy in improving participants’ subjective wellbeing before it could stand as genuine process for widespread implementation in healthcare services; however, taken as an early stage, this study demonstrates some promise for the process. Further elements that I would suggest the authors consider include:

* Whilst the authors have not overstated the case for their own findings in this paper, I do find that some of the rationale explored makes overstatements of the potential in this way, for example:
  
  o The opening statement should incorporate more caution; e.g. "…are considered by some to be small…” as it is not a widely-held view that the effects of psychotherapies are overestimated.
  o The statement that "rAMS is considered to be an enduring trait of depression" is also somewhat overstated, and not represented by the single citation provided to an old unsystematic review of research in my opinion.
  o The evidence cited for the closing statement of the second paragraph also fails to truly support the confident statement that MeST has demonstrable impact in the client group in question. Some cited studies have non-active placebos, others explore PTSD rather than depression, and the one study [18] with an active control showed improvement in memory specificity but not depressive symptomatology. This statement therefore also needs more caution injected.

* Much of the rest of the introduction provides a strong rationale for the importance of structured approaches to translational research, and it is important that these elements are maintained.

* There was no mention in the methods of the way in which the authors collected unstructured data; as such, it was inappropriate to describe "qualitative findings", which offers a suggestion of some level of systematic process. Authors should either report how
they undertook the qualitative element, or they should report these as anecdotal observations rather than qualitative findings.

* The analysis in additional file 2 explores whether change in depressive symptoms explain change in memory specificity. I was somewhat confused by this approach, given that the argument for exploring intervention for such a specific symptom as memory specificity is that it can have an effect on depressive symptoms, rather than the other way around. I am therefore left wondering why there was not a similar statistical analysis of any effect in this other direction (i.e. did change in memory specificity explain change in depression) when they come to report the data for depressive symptoms. Any such impact would surely be the argument for any further investment in formal trialling, and so the lack of such analysis and any subsequent critique - particularly when they have done such an analysis for the relationship in the alternative direction - appears to be remiss.

* I find it somewhat disingenuous that the analysis for the one setting where only MeST was undertaken is not reported in the main paper, given that no significant impact on any outcome measure was found. Whilst the sample in this one setting was very small, there needs to be at least some commentary on this I think.

* I am surprised that the authors do not offer up the possibility that an RCT of MeST in RCPs would be a clear next step in a journey towards full implementation, given that some healthcare economies (and international guidelines such as Cochrane) require such evidence to consider the kinds of changes in approach that these authors would require for the broader acceptance and delivery of this intervention.

* On exploring the tables, I noted that in Table 5, the scores for AMT were considerably different at pre-assessment - this seemed worthy of potential comment.

**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?**
If not, please specify which controls are required in your comments to the authors.

Unable to assess

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

Yes

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