Reviewer’s report

Title: Mindfulness is not Associated with Dissonant Attitudes but Enhances the Ability to Cope with them

Version: 2 Date: 13 Dec 2019

Reviewer: Reviewer 2

Reviewer's report:

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses one or several testable research questions? (Brief or other article types: is there a clear objective?)
Yes - there is a clear objective

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?
Yes - the approach is appropriate

EXECUTION - Are the experiments and analyses performed with sufficient technical rigor to allow confidence in the results?
No - there are minor issues

STATISTICS - Is the use of statistics in the manuscript appropriate?
No - there are issues with the statistics in the study

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?
No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Has the author addressed your concerns sufficiently for you to now recommend the work as a technically sound contribution? If not, can further revisions be made to make the work technically sound?
Probably - with minor revisions
GENERAL COMMENTS: This manuscript presents a study regarding the association between mindfulness, discrepancy between implicit and explicit attitudes, and intention/behavior. The authors hypothesized that mindfulness would be negatively associated with dissonance in attitudes and that mindfulness would moderate the association between dissonance and behavior/intention. The authors explore whether distinct facets of mindfulness operate in these ways.

This is my first review of the manuscript and I will focus the majority of my comments on the extent to which the authors have responded adequately to the prior reviews. I also have some additional comments - some that fit with the prior reviews and the authors responses, some that do not.

Overall, I think the authors did an adequate job addressing the reviewer's comments.

REQUESTED REVISIONS:

1. It is very useful that the authors have used the Benjamini -Hochberg test to control for false discovery rate. It is also helpful that the procedure is described on page 18. But this method can produce some unusual "results" with adjusted p -values that are much higher than would typically be thought of as "statistically significant" (e.g., much greater than .05) being called significant. I think this has happened in this study and so the tables in which the authors are referring to statistically significant findings have p-values that are much higher than "typical". It is helpful that the CI are also presented, but it might also be useful to provide the information that was used to calculate the modifications and the cutpoint that was calculated. I think this point regarding the cutpoint was not described on page 18 and that would also be helpful for readers.

2. 5 - facets of mindfulness and exploratory nature of the study. I think the authors argue forcefully for the 5-facet model of mindfulness and the importance of examining how it was important to include distinct facets in the study. The authors seem to indicate on the bottom of page six, however, that there will be one facet that accounts for the findings that mindfulness is associated with IDE. I do think the authors can strengthen the rationale for studying these independently - they provide some rationale, but then seem to drop that to say it is all exploratory.

3. Introduction of RMC as part of the study rationale. This is one point that I do not believe the authors have provided sufficient justification. They note that the study's focus is IED and mindfulness on behavior, but it seems that they authors would not have chosen ANY behavior arbitrarily. Making this justification and introducing the concept of RMC earlier doe not mean that the paper shits its focus to eating behaviors as a central idea, rather it provides a fuller rationale for why RMC (or intention to reduce RMC) is important. If its not important and is just arbitrary, then say that. The reason is, I think, that not all behaviors would show similar associations with IED. It was not surprising to me that findings were "mixed."

4. Causal Language - there are still places in the manuscript that the authors use language that sounds causal (e.g., bottom of page 26 - "mindfulness does not lead to more congruent attitudes". And causal language is used at other points too - "affect", "triggered", "impact" etc..
5. Drop out seems to be a significant issue with only about 32% of the original sample retained at the second followup. The authors note that acceptance without judgment predicted drop out at T1, but no measured variables predicted drop out at T2. It raised two major questions for me - what does it mean that one of the variables that is highlighted in the study findings also predicted drop out. Moreover, loss of 68% of a sample seems problematic in and of itself - even power was sufficient, what unknown variables might have distinguished drop outs from non and how may have they influenced the findings?

6. The intention items. That you use three intention items in your analysis is very confusing. First of all, it is suggested that these are not strongly associated, but are correlated .70-.82, which might suggest that they could reasonably be combined into a composite - conceptually it would make sense too. It is also not clear what distinguishes these items - especially since they are referred to as items 1,2 and 3. If you chose to include all three, I would recommend that you call them something else to distinguish what kind of intention they are measuring. It would be similar to calling the mindfulness facets mindfulness 1,2,3,4 and 5. Something has to make these items distinct. Moreover, if you use all three in the analyses, then you need to be able to explain why associations may be significant with one or two but not the others. The take way message refers only to intention - but not to item 1, 2, or 3.

Not noted by the earlier reviews:

7. I was struck by the inclusion of "intention" as a central outcome variable (or variables) because the introduction had not discussed intention at all - just behaviors. But it is introduced in the research questions. It struck me that this needed to be justified earlier in the introduction. It is evident that this was part of the parent study, but should also be discussed earlier.

8. It seemed odd to me that one criterion was that participants had to indicate that hey consumed meat "regularly" but the authors included a question to ensure that individuals actually consumed meat "at least once a month," which does not seem to be "regular".

9. At the top of page 27, the authors suggest that "the non-acceptance of dissonance had a positive and thereby desirable…..", but I do not think the authors actually measured non-acceptance of dissonance - this a big inference that goes well beyond the data. Likewise, there is a statement in the conclusion paragraph that is similar and indicates a process of managing dissonance that is not clearly tested or indicated in the findings. On the other hand, I think the point of using dissonance to promote behavior change is viable - it one strategy that is used in Motivational Interviewing - identifying discrepancies, which can cause dissonance.

ADDITIONAL REQUESTS/SUGGESTIONS:

No
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

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

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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