Reviewer’s report

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

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Reviewer: Norman Farb

Reviewer's report:

The authors present a study of whether mindfulness is associated with reduced discrepancy between implicit and explicit attitudes (IED). Mindfulness was not associated with IED. IED was however associated with lower red meat consumption (RMC) and higher intentions to reduce RMC. One facet of mindfulness (acceptance without judgement) interacted with the IED --&gt; intention effect.

This article provoked very mixed feelings. On the one hand, the rigor in which many of the stats are conducted, the description of the measures, and the general project of looking at whether mindfulness is related to IED were all features of the project that I read about with enthusiasm. On the other hand, the article has several major issues that I think make it unsuitable for publication in its current form:

1) Study Rationale. The introduction is very disorganized, often repeating sections, while completely omitting central constructs such as Red Meat Consumption (RMC) or the measurement of intentions, which are the 'meat' of the findings in this study (please excuse the pun). How can the reader be expected to follow the analyses when there is no theoretical framing of these constructs? Why are 5 sub-facets of mindfulness analyzed without any prediction about which facets will be most relevant? Were they all theorized to be important?

The analysis of IED with RMC also left me scratching my head- why would discrepancies between attitude types predict 'better' behavior? There is just no theory provided here, and it looks like the paper is a post hoc attempt to capitalize on a few p values that hit uncorrected significance.

2) No consideration of Type 1 error / power. The study's sample size presents and opportunity to make strong claims about the potential for an IED / mindfulness association, where even null findings may be meaningful if they are shown to be well-powered. However, the authors do not discuss power calculations for the sample, and more importantly, they conduct literally hundreds of statistical tests without any consideration of correction for multiple comparisons. To then focus on a few interactions with mindful acceptance with p-values in the .01-.05 range to me seems like a serious error- there is no way that these effects survive correction for multiple comparisons. I have suggested below that aggregating the intention items into a single measure, and running a multi-level analysis with time as a factor instead of 3 separate analyses for each time point, may help reduce the number of comparisons. The authors would then still need to present a strategy for dealing with a large number of comparisons for these findings to be credible.

3) Double dipping- the most striking findings link intention to RMC- but wasn't this already reported in the main study where this data was collected? Is it okay for the authors to report these findings again without mentioning whether they are novel reports or not?
I believe that points 1 and 2 require major revision to the manuscript to bring it to a publishable standard.

Introduction

- Study rationale. The intro begins relatively well, introducing dual-process models of cognition and attitude expression, and linking incongruence between implicit and explicit attitudes to lower levels of well-being. The critical idea, i.e., that mindfulness may serve as a vehicle for reducing this dissonance, could be more clearly explained. For example, how can mindful attention reveal an unconscious process? How is an unconscious revealed in general, anyway? Through unbiased observation of behaviour? Or do the authors argue that implicit attitudes are themselves directly observable, and if so, can this be made explicit?

- Along these lines, I'm not clear on the difference between self-esteem, reports of emotional experience, and attitudes more broadly defined. The authors argue that attitudes differ from self-related constructs in that the latter are "normally highly emotionally charged." So attitudes are then thought of as not having strong emotional components? What then is the definition of an attitude, if it is not self-relevant and emotionally-laden?

- Given this move to discuss non-self-relevant attitudes, it is then very curious that the next paragraph introduces private self and body consciousness, both of which seem to be extremely self-related. There is nothing wrong with these constructs, but the setup of saying that the paper is focusing on less self-relevant constructs doesn't make sense if this is going to be the next paragraph. Like, where did these constructs come from, given the preceding introduction? Then, given the strong claim that these two constructs "correspond with the mindfulness subskill observing", it would be nice to see either examples of the questions or better yet, some research to substantiate this claim. Otherwise, if the authors are interested in mindfulness, why not just use a mindfulness scale? It feels like this paragraph is just being shoe-horned in- it could be cut or else why these constructs are being mentioned could be better explained.

- The intro then fans out again to define attitudes and how they relate to well-being, which seems like it should have been covered already in the first page or 2 of the paper- could the authors move this discussion of attitudes and well-being to a single section of the intro, I would suggest moving it all to the start? It feels like the intro section begins all over again on page 6, which is confusing to the reader.

- Is mindfulness just the opposite of impulsivity? If not, how does it differ? This is important in justifying how the study is different from the Goldstein study cited in the paragraph discussing impulsivity as a moderator.

- Where does red meat consumption come from as a construct / measure? It is not mentioned anywhere in the introduction (the acronym is not even introduced except for in the abstract)- why was this measure selected? Introducing new concepts in the methods section about the overall study motivation is confusing to the reader.

Methods

- the overall study protocol is pre-registered, but the current mindfulness exploration is not. In fact, the KIMS-E was not mentioned in the main pre-registration, which leads me to question what the full
protocol was- it weakens rather than strengthens this paper to point to a pre-registration that does not include the measures you used! According to that pre-registration, you should not have the data (i.e., the KIMS-E and other measures) that you currently report...

- along these lines, did the authors' ethics review process include this specific set of analyses? If they were omitted from the pre-registration, perhaps they were also not covered by the ethics review process?

- why exclude people who had not consumed red meat in the past few months? doesn't this limit your findings to people trying to reduce red meat consumption, but does not then generalize to people who are attempting to maintain a meat-free diet?

- the IAT design and processing seem appropriate and are well described

- the methods around factor structure, dropout analysis, and IED calculations all seem appropriate and well described

- I was surprised that the 3 intention items were not combined in some way, or at least explored via factor analysis, as the use of all 3 separate items leads to an explosion of comparisons later on that makes it hard to interpret the study findings

- Similarly, I was perplexed that the 3 time points were not analyzed as a level in a multilevel model, as opposed to running 3 separate sets of each analysis that involved time, as again this creates a huge number of separate statistical tests that dramatically increase the chance of type 1 error and lower power

- In general, I didn't understand the theoretical rationale for examining the relationship between IED and intentions- what does this type of analysis mean in terms of theory? That people who are less consistent between the explicit and the implicit attitude are more likely to hold strong explicit intentions around red meat?

- Similarly, why should IED be linked to red meat consumption (why was this tested)? Why would being less consistent in one's two types of attitudes end up predicting behavior?

Results

- the results reporting was well done, and I appreciate the use of confidence intervals in addition to test statistics and p values

Discussion

- the discussion is reasonable in interpreting the results of the statistical tests but doesn't help address the major theory issues outlined above
Minor points:

- Grammar/proofing. The introduction section could use a bit of editing, for example, "Assumingly" is not a word (bottom of page 4), maybe the authors meant 'Presumably'? There are some other grammatical issues that more thorough revision could correct.

- Why are some references blinded when the authors themselves are not blinded from review? It just makes review harder and doesn't protect from reviewer bias.

If the major issues around theoretical framing and correction for multiple comparisons could be addressed, I would be happy to consider this report for publication, but as it stands, I cannot support there being value in the findings as presented.

Best regards,

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Assistant Professor of Psychology
University of Toronto

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.

No

Are the conclusions drawn adequately supported by the data shown?
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No

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