**Author’s response to reviews**

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

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**Author’s response to reviews:**

Reviewer reports:

Adrian Wan, Ph.D. (Reviewer 1): The study attempted to explore whether mindfulness sub-skills associate with decreased implicit-explicit discrepancy (IED); and if mindfulness sub-skills moderate the relationship between IED and intention/ behavior related to red meat consumption (RMC). The authors proposed a longitudinal study with 3 follow-up time points utilizing quantitative data acquired from subjective reports of the relevant constructs. It was claimed that mindfulness sub-skills were not associated with decreased IED as hypothesized; and that acceptance buffered the effect of IED on intention.

Given the popularity of mindfulness practice, and its revival in empirical research, the scope of the study is interesting and relevant to the development of mindfulness-based intervention as a potential pathway for behavioral change and cognitive change by way of working through psychological dissonance. To this ends, the rationale of the study were explained and discussed in details, and the key concepts were also adequately explained. The study design is appropriate to answer the proposed research questions, and the methodology of the research was also clearly articulated in the manuscript.

Before considering for publication, please consider the following queries:

1. Typos were found in the manuscript, such as "We programmed *he* SC-IAT by means of the software Inquisit by Millisecond…” (p. 10, Line 20), and "…participants has to press *i* on their keyboard." [italic is missing] (Line 10, p. 51).
   --&gt; Thank you for this feedback. We corrected the typos.

2. Ethical approval number is missing/ incomplete (p.8, Line 33)
   --&gt; Thank you for this feedback. We added this information.

3. The sample was recruited from an online sample; will the recruitment strategy imposed potential limitations / confounding to the findings?
   --&gt; Thank you for this question. We added the following answer to this question in the limitation section of the discussion (page 29).
Third, the study was conducted online, thus participants were at home in an uncontrolled environment. This appears to be an environment more prone for distractions, which, one might expect, could especially affect the SC-IAT and its outcomes. However, Houben and Wiers (2008) investigated whether an Internet-delivered IAT would yield different results compared to an IAT performed in a lab and detected no significant differences. Importantly, they even found that the IAT performed at home was more strongly related to other explicitly assessed measures as well as to the target behavior (drinking behavior) than the lab-based IAT. This supported our choice to apply this method to our sample and setting as well. Additionally, we asked participants directly after having performed the SC-IAT whether they were distracted during the task. If this was the case, their data were excluded from the analyses.

4. It is unclear (or not explicitly stated) why d-score was adopted for data analysis (p. 12, Line 4) --&gt; The following information is now mentioned explicitly on page 12.

The d-score represents the strength of an association between concepts, which is measured by the standardized mean difference score of the 'hypothesis-inconsistent' pairings and 'hypothesis-consistent' pairings and is expressed in milliseconds [49].

5. It was mentioned that three items were adopted to measure intention to reduce RMC (p. 12, Line 29), can you specify how these three items were generated?

--&gt; On page 11, we explained that all explicit cognitions as well as the intention items are generated as suggested by the I-change model. Relevant references can also be found on this page.

6. Can you explain why using the KIMS-E as the measurement for mindfulness in your study?

--&gt; The following rationale for choosing the KIMS-E is now explained on page 15.

To assess mindfulness, a Dutch translation of the KIMS-E [17, 18] was administered. In contrast to earlier studies that suggested mindfulness to be a unidimensional construct [59, 60], a more recent factor analysis regarding various mindfulness questionnaires identified five different domains of trait mindfulness [17]. Given that the original KIMS [25] only consists of four facets, we decided to use the KIMS-E, which entails the fifth facet. Also the KIMS-E has demonstrated good psychometric properties [18].

7. In the methodology section, can you briefly explain the rationale for your sample calculation? And I am aware that the dropout rate between Baseline and One-month follow-up is about 33.6%, and only 32% were maintained at the third follow-up time points. How would you comment on this reduction in maintenance rate? And would such attrition impact on the results of the findings, or the research study as a whole? The number of attrition appeared to be quite a lot.

--&gt; The power calculation is now added on page 8 and 9. Although the drop-out was quite high, we almost approach the required sample size at the last follow-up as calculated by the power calculation (467 instead of 488). In other words, attrition rates were taken into account during the study development already. Therefore, we think this warrants to draw valid conclusions based on the current sample.

8. The second limitations you have mentioned in Discussions (p. 24, Line 44-49) is important. Since it is a known issue based on existing research (reference #66), so have you considered any other possible options in minimizing the conceptual discrepancies between "implicit attitudes towards red meat" vs. "explicit attitudes towards the *consumption of red meat*?

--&gt; Thank you for your comment. On page 28 and 29 we added the following paragraph in which we discuss two possible ways on how to minimize this incongruence.

One possible way to minimize the incongruity between the explicit attitude and the implicit attitude
could be to assess both attitudes towards the object, hence red meat. However, a pitfall would be that explicit attitudes are not congruent with the other explicit constructs social norms, social modeling, and self-efficacy anymore, which are always assessed towards a behavior. Another way could be the usage of stimuli in the implicit task that depict the consumption of red meat more clearly (e.g. a fork with a piece of red meat pointed towards the mouth). However, it might be questionable whether the subjects are able to recognize these stimuli clearly as the consumption of red meat as other factors might be disturbing (e.g. the fork, the mouth, the face(s) of people, etc.). Future research should test the best way to minimize the incongruity between the measured attitudes.

9. What other alternative hypotheses you can come up with in the context of the study? Any other confounding variables that might help making sense of the results reported in this study? I think it is also interesting to explore the role of compassion/ self-compassion as a mediator of change, or as a potential buffer against psychological discrepancies and dissonance. This is because core to all mindfulness practices are the cultivation of compassion towards the self and the others. Maybe, further research can go towards that line of inquiry.

--&gt; Thank you for this input. We added the following paragraph discussing the possible role of self-compassion in the context of attitudinal dissonance on page 27 and 28 of the discussion section.

‘Another possible avenue for future studies could be the investigation of self-compassion in the context of discrepancies. Self-compassion is an even broader construct than mindfulness as it includes the components of mindfulness, self-kindness, and common humanity. It is defined as a kind and understanding outlook toward one's personal disappointments and struggles [79, 80]. A study on body appreciation concluded that self-compassion might work as a buffer against harshly judging discrepancies regarding body-related comparisons. Whether self-compassion might also prevent a judgmental view against attitudinal discrepancies could be investigated in the future.’

I reckon that this scope of the study will shed lights on the potential role of mindfulness play in changing our intention and behaviors, which will further contributions to the development of mindfulness-based interventions, and change-related framework that would guide mindfulness practitioners in their work. It is also uncommon to have research papers with null findings being published, yet I do recognize the contributions of null findings in empirical/ evidence-based practice research. Therefore, please consider to give the above questions some thoughts, so that the community can learn more from your meaningful work on mindfulness practice, and its potential mechanism of psychological/ cognitive-behavioral change.

Finally, I wish you all the best in your revision, and further research!

&gt;&gt; Thank you very much – also for your constructive feedback.

Bas Verplanken (Reviewer 2): This manuscript addresses the interesting issue of discrepancies between implicit and explicit attitudes, and the relationships of such discrepancies with intentions and behaviour. The authors hypothesised that individual differences in mindfulness plays a role in these relationships.

This certainly was an interesting endeavour. The study was well conducted and the paper is well written. I do have a number of concerns about assumptions, hypotheses and measures. Firstly, the authors seem to entertain two 'hidden' assumptions. The first is that individuals are motivated to bring their explicit attitudes in line with their implicit attitudes. While in certain domains it might be the case that implicit-explicit discrepancies are experienced as uncomfortable, or may lead to suboptimal behaviour, I am not convinced that there is an innate tendency to bring the two in line: most people live
happily with many sorts of dissonances and inconsistencies. A second 'hidden' assumption seems that implicit attitudes are "correct". Thus, once a mindful person recognises the discrepancy, they are then inclined to adapt their explicit attitude accordingly. While the authors do not explicitly state these two assumptions, the least I would like to see is that these issues are clarified.

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Thank you for your thought-provoking remarks regarding our manuscript. We do not fully agree with the statement that “people live happily with many sorts of dissonances”. According to Festinger (1957), people are motivated to resolve dissonance, as this is associated with negative feelings. IED has been shown to affect behavior (e.g. Rydell et al., 2008; Muschalik et al., 2019; Goldstein et al., 2014; Maliszewski, 2011), which presumably is such an attempt to resolve dissonance. Furthermore, we do not assume that implicit attitudes are more ‘correct’ than explicit attitudes, but only that mindfulness might help to get in touch with a person’s implicit attitude. This might result in expressing one’s implicit attitude also in an explicit way. We added the following paragraph to page 4 to prevent a possible confusion about hidden assumptions.

‘In sum, mindfulness involves the pure perception of one’s internal and external experiences in an objective way. Since this mindful processing enhances the clarity of one’s thoughts, feelings, behaviors, and sensations [19], it may allow intuitive, implicitly represented information about objects (i.e., implicit attitudes) to become incorporated into a person’s conscious experience (i.e., explicit attitude). In addition, mindfulness facilitates the ability to observe one’s (inner) experiences without judgment or attempts to change those. Hence, mindful individuals may also be more accepting of their implicit attitudes once they become aware of them. We do not assume that implicit attitudes are more ‘correct’ than explicit attitudes, but that mindfulness might help to get in touch with a person’s implicit attitude. This might result in expressing one’s implicit attitude also in an explicit way. Thereby, discrepancy between a person’s implicit and explicit attitude might be decreased.’

The first hypothesis states that "mindfulness leads to more congruent implicit and explicit attitudes (...)" (p.7, RQ1). The first thing to mention is that the present study is unable to establish causality. Having three measurements over time does not salvage this problem, as it remains to be a correlational design. The best one may expect in this case might be from cross-lagged panel analyses, although these have their problems and can neither establish causality, but such an analysis was not conducted.

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Thank you for this remark. We reformulated the hypotheses into “related to” to prevent that causality is implied.

Apart from the causality issue, I am not convinced of the hypothesis that mindfulness should be associated with more congruency between implicit and explicit attitudes. The cited work by Koole and collaborators is somewhat different in that they focused on self-related constructs. Many attitudes are not self-related, and, again, people happily live with all sorts of inconsistencies. It is not unreasonable to suggest that mindful persons may be more aware of what is denoted as an implicit attitude. The authors provide such an account in the discussion section, when they suggest that "(...) although mindfulness does not lead to more congruent attitudes, it seems to offer a skill to deal with dissonant attitudes" (p.23). This seems much more plausible: why would a mindful person - accepting "things as they are" - be motivated to change anything, unless they see good reasons to do so? Incidentally, in my book this means that the distinction between implicit and explicit systems is blurred at best, and may actually not really exist at all.

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It is absolutely true that the work by Koole et al. focused on self-related constructs. This was exactly the reason, why we came up with the research question whether the same relationships can be found for less self-relevant constructs, such as attitudes (see explanation on the beginning of page 5). As mentioned in our reply to an earlier comment, we do not fully agree with the statement that “people happily live with all sorts of inconsistencies”. According to Festinger (1957), people are motivated to resolve dissonance, as this is associated with negative feelings. Moreover, IED has been shown to
affect behavior (e.g. Rydell et al., 2008; Muschalik et al., 2019; Goldstein et al, 2014; Maliszewski, 2011). Hence, this might be an indicator for the existence of IED. In order to illustrate the existence of IED in our study, we added a graph on page 18, which depicts the distribution of IED in our sample.

The authors distinguish the five facets of mindfulness, but do not provide hypotheses on what each of these facets may do differently. Each facet is tested separately, but these analyses are fishing expeditions, rather than based on theory-driven expectations. Can 'observing' expected to do the same thing as 'non-reactivity'? This relates to a more fundamental issue with respect to the mindfulness construct. One may indeed take a reductionist stance, and analyse the five facets as if these were independent variables. However, there are also those who take a more holistic view on mindfulness, and would argue that "mindfulness is more than the sum of its parts". The authors need to make up their minds on this, and act accordingly. If they wish to analysed the five facets independently, there need to be hypotheses on differential effects.

Thank you for stressing this. The reason to assess the mindfulness subscales separately was to get an indication on which specific sub-scale/ability is related to (less) dissonance between attitudes. This was not done before and therefore also one of the novel aspects of the study. Former authors (e.g. Koole et al., 2009; Hyde et al., 2010) assumed that the ability to have insight into one’s inner processes (i.e. subskill observing) was responsible for reduced dissonance between self-related constructs. However, the different subskills were not tested in these studies (explained on page 6). As a result of that, we decided to explore the sub-skills separately. Moreover, the factor analysis performed by Bear et al. (2006) showed that mindfulness is not a unidimensional, but a multidimensional construct. This supports our approach of looking at the mindfulness sub-skills separately. The relevant reference is added on page 14 and 15. Adding hypotheses at this point in time would be post-hoc assumptions. Therefore, we kept the research question exploratory as originally intended and stress this in the manuscript (page 6 and page 8).

I would have liked to see evidence of the five factors, for instance in a confirmatory factor analysis. In addition, in testing the five facets separately, the authors should have applied Bonferroni corrections in order to protect from Type 1 error. Thus, instead of alphas of .05 and .001 they should adopt .01 and .0002 respectively.

A factor analysis by Baer et al. (2006) showed that mindfulness is not a unidimensional but a multidimensional construct, consisting of the five factors named in the present article. We added this explanation and reference on page 14 and 15. Also we performed exploratory factor analyses for all five factors and removed items with low factor loadings (explained on page 16). Moreover, omega was calculated for each of the five facets and were estimated as sufficient (explained in the method section). Therefore, we regard an additional confirmatory factor analysis as superfluous.

We now applied the Benjamini-Hochberg procedure to protect from Type 1 error. This method is regarded as more powerful and less conservative than the Bonferroni procedure. The method is explained on page 18. The Benjamini-Hochberg p-values are now reported for all regression models (see Table 3 and Table 4).

I was puzzled by the dissonance index, IED, which is a key variable. It was calculated as the absolute value of the difference between implicit and explicit attitude z-scores. The underlying assumption, then, is that all types of dissonance are equal. For instance, a positive explicit attitude and a negative implicit attitude (e.g., +1.00 and -1.00, respectively) receive the same IED score as a negative explicit
and a positive implicit attitude (-1.00 and +1.00, respectively). A neutral explicit attitude and a negative implicit attitude (0.00 and -2.00) receive the same score as a neutral explicit a positive implicit attitude (0.00 and +2.00), and so on. The assumption that these dissonances are all the same is rather speculative at best.

The method used to compute discrepancy scores is based on former studies, such as


We performed the same calculations as done in the studies cited above and we regard this a suitable method to calculate the discrepancy for the purposes of the current study. Moreover, we do not claim that these dissonances are all the same, but we investigated how the construct dissonance in general relates to mindfulness.

Null results are always difficult to interpret, and there may be (but not always!) good reasons to publish them. The authors did a good job in discussing these null results. However, there is still room for doubts, for instance about the reliability and validity of the measures. This holds especially for the implicit attitude measure and the associated IED scores. For instance, we do not see the correlations between the implicit attitude measures on the three occasions. Reasonable test-retest correlations would provide supporting evidence for the reliability of the measure. The implicit attitude did not seem to correlate with any other variables, which may feed the suspicion of invalidity.

Thank you for stressing the importance of publishing null results and acknowledging the difficulties to interpret them. In line with your suggestion, we calculated the test-retest correlations between implicit attitudes assessed at all measurement points. This is now reported on page 13 of the method section. As the correlations were relatively low, we elaborated on this with the following paragraph in the discussion section (page 29).

‘Fourth, the SC-IAT showed low test-retest stability, which is a general issue faced by researchers using implicit measures. This might be due to sensitive systematic error (e.g., learning effects or situational cues [85, 86]) or might simply demonstrate a low validity of these measures. At this point in time it is unclear if any of these two occurrences happened and were also responsible for the null-findings. These questions are an avenue for future studies. For example, after having performed the implicit measure multiple times, participants could get asked whether they noticed a learning effect, in order to control for possible learning effects.’

The authors do not seem to take notice of effect sizes. For instance, the finding that "IED was related to behavior" was based on a .09 correlation between IED and RMC at T0 (I could not detect the other correlations). This cannot be considered as a meaningful relation.
The finding that IED was related to behavior was not based on the correlation but on the findings of the regression models reported on page 20 and 21. In all three regression models, IED was significantly related to RMC.

In all, although I appreciate the high quality of the study as it was conducted, the problems outlined above, and the series of null effects, have not convinced me that this paper makes a significant contribution.

We hope our reply to your valuable feedback takes away any concerns in terms of the contributions of this manuscript, despite the null results.

Luca Ostacoli, M.D. (Reviewer 3): The present study focuses on the relationship between mindfulness trait and dissonant attitudes towards red meat consumption. The article is worthy of publication as it is an exploration of how mindfulness and acceptance could have a buffering effect on reducing/dealing with dissonant attitudes, also considering possible interesting repercussions in terms of promoting healthy behaviors. Moreover, the article is very well written and easy to read. Introduction is complete and aims are clearly explained. Methodology is adequate and discussions are supported by results. I have only brief comments for the authors:

1) check for typos throughout the text (line 57 page 7, RMC is not previously explained; line 20, page 10, "the") --&gt;
We corrected the typos and took out the term “RMC” to avoid confusion. We replaced it with “behavior”.

2) please specify the amount of monetary reward (line 9, page 9).
--&gt; We specified the amount of monetary reward on page 9.

Norman Farb, PhD (Reviewer 4): 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:

--&gt;&gt; Thank you for your constructive feedback. We hope that the way in which we dealt with the feedback takes away any remaining concerns.

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?

--&gt; It is correct that we do not introduce RMC in the introduction. The reason is, that the focus of this article is on IED and mindfulness and their relationship with behavior. RMC is chosen as an example behavior and is not the focus of the study. Therefore, we only introduce it in the method
section to keep the focus clear. In other words, RMC is the topic that the current study uses as a way to explore IED and mindfulness and their relationship with behavior.

The theoretical reasoning why we also looked at IED and intention is explained more explicitly in the following paragraph on page 8.

‘According to the Reasoned-Action Approach [39] or the Integrated-Change Model [40], intention is the most proximate antecedent for behavior. It is unclear, whether IED affects behavior only or also its most proximate antecedent and whether this relationship might be moderated by the mindfulness subskill.’

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?

We performed explanatory analyses. Therefore, we did not make any predictions nor formulated hypotheses. The reason to assess the mindfulness subscales separately was to get an indication on which specific sub-scale/ability is related to (less) dissonance between attitudes. This was not done before and therefore also one of the novel aspects of the study. Former authors (e.g. Koole et al., 2009; Hyde et al., 2010) assumed that the ability to have insight into one’s inner processes (i.e. subskill observing) was responsible for reduced dissonance between self-related constructs. However, the different subskills were not tested in these studies (explained on page 6). As a result of that, we decided to explore the sub-skills separately. Moreover, the factor analysis performed by Bear et al. (2006) showed that mindfulness is not a unidimensional, but a multidimensional construct. This supports our approach of looking at the mindfulness sub-skills separately. The relevant reference is added on page 14 and 15. Adding hypotheses at this point in time would be post-hoc assumptions. Therefore, we kept the research question exploratory as originally intended and stress this in the manuscript. This is now mentioned explicitly on page 6 and page 8.

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.

On page 6, we explain that earlier studies have found that IED has an impact on behavior and name relevant references and explain their findings. These findings are mixed, in the sense that IED was associated with an increase of certain behaviors (e.g. in Goldstein et al. 2014; Maliszewski et al, 2011) or with a decrease of other behaviors (e.g. Muschalik et al. 2019). We do not claim that IED does predict behavior better, but as a result of these mixed findings, the question arises, whether the relationship between IED and behavior might be moderated by a third variable, in this case mindfulness. This reasoning is explained on page 7.

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.

It was not possible to combine the three intention items, as factor saturation was insufficient. This is reported on page 14. However, we now applied the Benjamini-Hochberg procedure to deal with
possible Type 1 errors. This method is regarded as more powerful and less conservative than the Bonferroni procedure. The method is explained on page 18. The Benjamini-Hochberg p-values are now reported for the regression models.

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?

---&gt; The main finding of the present article is that the mindfulness subskill acceptance moderates the relationship between IED and the intention to reduce RMC. This question was not addressed in the earlier study. Rather, the previous study looked at how explicit cognitions (explicit attitudes, social norms, social modeling, self-efficacy) and implicit attitudes jointly predict intention to reduce RMC as well as RMC itself. IED and mindfulness were not part of the earlier study. This is now mentioned explicitly on page 9.

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?

---&gt; Thank you for this comment. On page 4, we added the following paragraph, which summarizes how mindfulness might function in revealing unconscious processes/implicit attitudes.

‘In sum, mindfulness involves the pure perception of one’s internal and external experiences in an objective way. Since this mindful processing enhances the clarity of one’s thoughts, feelings, behaviors, and sensations [19], it may allow intuitive, implicitly represented information about objects (i.e., implicit attitudes) to become incorporated into a person’s conscious experience (i.e., explicit attitude). In addition, mindfulness facilitates the ability to observe one’s (inner) experiences without judgment or attempts to change those. Hence, mindful individuals may also be more accepting of their implicit attitudes once they become aware of them. We do not assume that implicit attitudes are more ‘correct’ than explicit attitudes, but that mindfulness might help to get in touch with a person’s implicit attitude. This might result in expressing one’s implicit attitude also in an explicit way. Thereby, discrepancy between a person’s implicit and explicit attitude might be decreased.’

- 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?
Thank you for your comment. We added a definition of self-esteem as well as a definition of attitude on page 5, to make the difference between the two concepts clearer. Also, the following paragraph was rewritten in order to make the novelty of the research question even more explicit.

'Self-esteem, for example, is defined as a “favourable or unfavourable attitude toward the self” [20]. Hence, the self is in the focus of the evaluation. It is unclear, whether mindfulness can also help to decrease the level of dissonance between implicit and explicit attitudes – constructs that are, although also to some degree, less self-relevant and less experiential, but more actionable. An attitude is defined as “a psychological tendency that is expressed by evaluating a particular entity with some degree of favor or disfavor” [21]. Hence, not the self is the focus of the evaluation but another entity. To our knowledge, the question whether mindfulness can decrease dissonance between implicit and explicit attitudes regarding entities other than the self has been unaddressed and builds, therefore, the focus of this study.'

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.

Thank you for your suggestion. We did not move the attitude section as we regard the introduction section as well organized, the way it is at the moment. The introduction starts broadly with dual-process models and the two types of evaluations (impulsive and reflective). We then mention that dissonance between both types of evaluations can occur and that mindfulness has been shown to diminish dissonance between implicit and explicit self-related constructs. With this as a basis, we then introduce attitude and come up with our research questions, whether the findings reported earlier can be applied to attitudes as well. Hence, we move from the current broad knowledge in this area to the more
specific subject of the present paper. Moving the attitude section to the start would, in our opinion, disturb this funnel shaped structure.

- 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.

--&gt; We added another reference according to which impulsivity and mindfulness are defined as ‘natural reciprocals’ (i.e. Murphy & MacKillop, 2012). Additionally, we rewrote the paragraph and added some more information to specify the overlap and differences of mindfulness and impulsivity in more detail. The following new paragraph can now be found on page 8.

‘According to Murphy and MacKillop [35] “impulsivity and mindfulness are natural reciprocals” (page 528). Although both constructs entail a focus on the present moment, the present moment is approached differently. That is, impulsivity entails an overemphasis on the present moment, without an adequate reflection on the future or on consequences of one’s action [36]. Mindfulness also entails a focus on the present moment, however, by noticing and experiencing it fully. Hence, it occurs without judgment and reactivity. Therefore, impulsivity reflects a greater likelihood to act on an impulse, and mindfulness reflects a decreased likelihood to do so, which stems from the acknowledgement of impermanence (i.e. everything comes and goes) [e.g. 37].’

- 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.

--&gt; Thank you for this comment. It is correct that we do not introduce RMC in the introduction. The reason is, that the focus of this article is on IED and mindfulness and their relationship with behavior. RMC is chosen as an example behavior and is not the focus of the study. Therefore, we only introduce it in the method section, to keep the focus clear. In other words, RMC is the topic that the current study uses as a way to explore IED and mindfulness and their relationship with behavior (which is the focus of the manuscript at hand).

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...

--&gt; Thank you for your comment. The pre-registration refers to the previous study. We replaced the link. In this study, mindfulness was not investigated and therefore not mentioned in the pre-registration. In order to prevent any confusion, we made more explicit that the pre-registration refers to the previous study and that the present study is a follow-up (page 8). The reference to the pre-registration is still valuable, in our perspective, because it explains other aspects of the study that were planned beforehand (e.g. other measures included, recruitment procedures).
- 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?

--&gt; Yes, the ethics review process included the questions addressed in this study. The form can be found on OSF at https://mfr.osf.io/render?url=https://osf.io/d2746/?action=download%26mode=render

- 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?

--&gt; It is correct, that the results are not generalizable to people who are attempting to maintain a meat-free diet. However, reducing meat consumption and maintaining a meat-free diets are two different types of behaviors, which are likely associated with different constructs and concepts. Therefore, we made the conscious choice to only focus on those people, who are currently eating meat.

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

--&gt; Thank you.

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

--&gt; Thank you.

- 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

--&gt; Thank you for your comment. We conducted a factor analysis as well as calculated omega as an indicator of internal structure for all constructs (the explanation can be found on page 15). Regarding the intention items, the factor saturation of the standardized sum scores was estimated as insufficient ($\Omega = .07$), therefore intention items were entered separately in the analyses as. This is reported on page 13.

- 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

--&gt; Thank you for this suggestion. As we were interested in cross-sectional as well as longitudinal relationships, we decided to run 3 separate sets of analyses. Moreover, we now applied the Benjamini-Hochberg procedure to deal with possible Type 1 errors. This method is regarded as more powerful and less conservative than the Bonferroni procedure. The method is explained on page 18. The Benjamini-Hochberg p-values are now reported for the regression models.

- 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?

--&gt; We examined the relationship in order to see whether dissonance also has an effect on intention. It is known that dissonant attitudes can have an impact on behavior (i.e. people increased or decreased the behavior at hand, which is probably done in order to decrease dissonance). This has been shown in earlier studies and is explained on page 7. As intention is regarded to be the most proximal determinant
for behavior, we examined whether dissonance also affects intention and whether this is moderated by mindfulness skills. More precisely, we wanted to investigate whether intention can be increased or decreased in order to dissolve dissonance.

- 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?

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Thank you for this question. In the introduction, we explain that former studies have found that IED either had an increasing or decreasing effect on behavior (page 6). This is related to dissonance and the attempt to resolve dissonance. In the present study, we wanted to investigate how IED and RMC are also related to each other and whether this relationship is moderated by mindfulness. This reasoning is explained on page 7.

Results

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

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Thank you.

Discussion

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

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Thank you. We addressed the issues you mentioned above as explained in our reply to the specific comments.

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.

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Thank you. We replaced “Assumingly” with “Presumably” and also checked the grammar by means of a computer program.

- 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.

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Thank you. We removed the blinded references and added the relevant reference.

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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