

Peer review process documentation

of the article

**When Environmental Motivation Does and Does Not Cut Emissions: Environmental Motivation and
Students' Cafeteria Food Decisions**

by

Nicolas E. Neef, Ken Wakabayashi, Ann-Kathrin Briem, Dierk Bauknecht & Siegmar Otto

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Handling editor: Florian Lange

Review Round 1

Comments by the editor

1. In line with the reviewers, I see much value in your empirical contribution and think that you are making a compelling argument. A common thread that I see in the reviews is the request for sensitivity analyses/robustness checks/alternative model specifications. Given the lack of a preregistered analysis strategy, I concur that such analyses would be very useful to increase the evidentiary value of your contribution. I see that many of such analyses would be underpowered if considered as hypothesis tests, but I don't think that this should discourage you. Simply demonstrating how much coefficients change when, e.g., considering vegan and vegetarian options separately (or as ordinal outcome?) or when including/excluding additional participants would be very informative.
2. I did not understand why the investigated factors/relationships were framed as "implicit assumptions". I think you claim that researchers examining the correlation between motivation and impact make those assumptions, but you do not demonstrate this nor do I think readers can easily follow this claim. I would think that people who examine this correlation and who talk about a motivation-impact gap would explain low correlations with factors/explanations very similar to the ones you investigated here. Perhaps consider reframing the presentation along the lines of: "Here are three requirements that need to be met for the correlation between EM and emissions to be strong – are they met?" or "Here are three potential factors that may attenuate the correlation between EM and emissions (=that may account for the low correlations previous research has found) – do they?"
3. In section 2.2.3, please check whether it is possible to further clarify for which period participants reported their meal choices. I think they reported retrospectively, on 25, 26, or 27 January, what they ate on 22-26 January. Is that correct? How could people taking the survey on 25 January know what they will have eaten on 26 January? Or am I misunderstanding the procedure? Please add as many details as possible regarding the reporting procedure (and my apologies if I should have overlooked any).
4. H2 postulates the absence of a relationship and the lack of a non-significant coefficient is then taken as evidence for this absence hypothesis. This seems inconsistent. The available data only allows concluding that you did not find evidence for a relationship, but not that you found evidence for the absence of such a hypothesis. You do address this in the discussion section, but that does not seem to solve the issue that H2 is a hypothesis that cannot be tested with the approach you chose. I suggest adjusting the language accordingly, making sure that at no point, you are suggesting that you found support for H2, and discussing the many explanations that may account for the absence of a relationship in your data. Perhaps you can add a disclaimer already when you postulate H2 in the introduction, clarifying that you expect to see a weak relationship in your data, while acknowledging that you are not able to test H2 in the inferential statistical sense.

I inserted some minor formatting/language-related comments in the attached pdf. Please take them into account, but you do not need to respond to those comments in your response letter. I am looking forward to your revision!

Comments by Reviewer A

The manuscript entitled “When Environmental Motivation Does and Does Not Cut Emissions” examines relationships between environmental motivation, food choices, and carbon emissions in a real-world context. It makes a compelling argument that relationships between motivation and impact are moderated by what is practically feasible and that people act on broad categories around sustainability rather than specific CO2 values. The authors transparently described their methods, which made it very easy to follow and will support replications. I think that the paper is a good fit for the journal, but I have some comments about framing and inferences.

1. The abstract states that “*emission differences are neither always acted upon nor even recognized*” (lines 21-22). The results support that emission differences were not acted upon (H2a), but to my understanding, there were no tests of recognition.
 2. The Introduction states that prior research assessing motivation-impact links implicitly assumes sustainable behavior **necessarily** produces lower carbon emissions (lines 113–121), whereas later it states that a motivation-impact link would be expected if vegetarian and vegan meals emit less than omnivorous meals **on average** (lines 208–211). The “necessity” versus “on average” formulations appear to reflect different underlying assumptions, either of which could warrant such research, and I suspect that prior work might have well relied on the latter.
 3. The recommendation to enhance knowledge about carbon emissions (lines 25-26 and lines 43-44) is intriguing. However, it seems to conflict with the results of H2b showing that knowledge didn't affect choices, and (unlike for emission differences between meal choices) no moderation was tested, i.e., whether environmental motivation would be more predictive of impact among people with more knowledge. Also, there seems to be a minor inconsistency between the translations: the English version of the abstract recommends enhancing knowledge at the point of purchase while the German version seems to recommend enhancing knowledge more generally.
 4. Relatedly, the Discussion (lines 497-498) states that: “*Yet such knowledge appears insufficiently precise or salient to impact what meals the participants selected (...). In essence, this means that, even when environmentally motivated, individuals might often lack the precision in knowledge to consistently choose the lowest-emission behaviors*”. I don't think that this claim is supported by the data, because precise knowledge wasn't measured, so it's unclear how pronounced it would be and whether it would affect choice. Please distinguish more clearly between the null-finding (for general knowledge) and speculations about reasons and avenues for future work. I was specifically wondering whether you would recommend others to use the current or a different measure of knowledge in conceptual replications?
 5. Method: Because the exclusions were quite substantial, I recommend reporting sensitivity analyses in the Supplement testing whether the patterns are robust to alternative exclusion criteria.
 6. Avoid the phrasing “*highly insignificant*” (lines 495-496), because it is not a meaningful statistical concept (if the null-hypothesis is true, any p-value is equally likely).
 7. The practical recommendations (lines 529-542) are helpful. The section could be strengthened by more clearly distinguishing implications that follow directly from the design/results from recommendations drawn from the broader literature, with a focus on the former.
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Comments by Reviewer B

Overall, I think this is a very interesting and well thought-out study, which provides an important argument to the literature.

The obvious issue is the rather small and biased sample, that make generalisation of the effects difficult. But as the measures and methods are really good and the study uses data on specific (self-reported) behaviour instead of intention, I think it's nonetheless a valuable contribution to the literature.

Nonetheless, I think there is one more serious and some minor issues, that should be addressed by the authors before the study is published. I therefore recommend to accept the article after major revisions.

The more serious issue is the dichotomization of the food choice into omnivore vs. sustainable (vegan+vegetarian), while only 1/4 of the sample reports to be omnivore and only about 1/8 of the chosen meals were the non-sustainable option in this categorization. Why do this? This seems inconsistent with the argument about explaining emission and other sustainability-goals given in the introduction, as the vegan option should nearly always be even more sustainable and have even less emissions.

And it probably also makes it harder to find effects in this sample, because statistical power should be higher, if the ratio of the two options is closer to 50-50 (if I am not mistaken). I think it is necessary to either give a good argument in the introduction, why this is a better definition of "the sustainable option" than just the vegan option, and why it is a good idea to merge them into one category. Or to present results on how the models change when you use just the vegan options as 'sustainable behaviour' instead of vegetarian+vegan. I would prefer the latter. (When testing this, because of power, you probably, should either only test vegetarian against vegan, or combine omnivore+vegetarian).

Further comments:

Section 1: You make a really good argument here. IIRC, Simon & Merten (2024) made quite a similar case in their paper on measuring knowledge about impacts, arriving at 3 very similar points. Maybe it's worth referencing them?

Section 1.2: You probably should mention/discuss here a bit more, that being an animal product (after being related to cows) is the most influential factor by quite some amount for the emissions of a food product (see Poore & Nemecek)

So avoiding meat / animal products in general is a very good heuristic or rule of thumb to reduce emission (at least if the product contains similar amounts of it, your point on mixed foods is very valid)

Section 1.3: Should the same not be true also for Knowledge? Why don't you assume an interaction here?

If you take your own argument, should it not be necessary, that the person a) wants to protect the environment AND b) there is a substantial difference in Emissions AND c) the person knows about it?

So an interaction of EM x Emission difference x EK? I know that with your sample size it will probably not be possible to get meaningful results on a 3-way interaction, but I am curious if you had any other reason to not include interactions with knowledge in your model, or what your model would look like, if you included either EK x EM or EK x CO₂-diff.

2.2.1: I think the detailed discussion on the fit-values could be moved to the supplementaries. As you yourself note: The GEB is a well established scale - and the things you report there are not that relevant for your study overall.

Also, the part of the audience that is interested in these details probably already knows the difference between infit and outfit :P

2.2.2/2.3.1/2.3.2: I suggest moving the explanation, that the knowledge-scores were z-transformed to 2.2.2., where you describe your measurement of knowledge. You already need that transformation to calculate the mean rho and the correlation with EM reported in 2.2.2, when I saw no mention of this, my first assumption was you did no z-transformation at all.

Also, you then don't need mention it twice in 2.3.

2.3.1: Your data is not only clustered within participants, but also within the weekdays. You did not mention including a random intercept for the day, probably because you did not do it in any of the models.

Is the reason for this, that it could lead to artefacts, because of the CO₂-difference is directly linked to the weekdays? You could be a bit more explicit here about that.

2.3.2.: I don't think everyone in the audience of this journal is that familiar with GLMMs and will instantly understand what "Gamma distribution and log link to CO₂e" means in this context. Maybe you should explain a bit more about this model and how to interpret its results.

Table 2, Model 3: I think the last OR in this column should be in the cell below, shouldn't it? Model 3 uses knowledge as a predictor according to 2.3.1.

Table 2 & 3: It probably would help readers, if you include a bit more guidance on how to read your tables in the notes (or the corresponding texts). The Tables contain lots of information and GLLMs with Odds-Ratios are probably not that familiar to the audience. E.g: I was quite a bit confused about the relations between the p-values and the CIs in your table and it took me some time to remember that with OR 1 means no effect and values smaller than 1 mean effects in the opposite direction.

Related note: You do not report the rate of participants with a masters and or a bachelors degree in your study. You should, if you include them as predictors!

Table 2 & 3: Is the inclusion of R²c in this table useful for the reader, or does it distract from the more relevant R²m? (especially as it is just the sum of ICC and R²m)

Table 3, Model 5: Why is R²c here not equal to R²m + ICC? Are you sure, these are the correct numbers?

Figure 2: Did I understand correctly, that the red triangles are the white diamonds divided by the bars? Maybe you should make this a bit more clear, especially in the note of the Figure.

4.3: you should probably also mention, that you only have data on 5 specific days with 6 to 7 specific meal options each, so it's possible that some of the effects are confounded with which participants came in on which day or that some days may feature specific meal components that are more often liked/disliked.

4.3: As noted above, I am not really convinced that the merger of vegan and vegetarian did improve either power of interpretability.

Thanks for the interesting read!

Author Response to the Reviewers Round 1

EDITOR	
<p>In line with the reviewers, I see much value in your empirical contribution and think that you are making a compelling argument. A common thread that I see in the reviews is the request for sensitivity analyses/robustness checks/alternative model specifications. Given the lack of a preregistered analysis strategy, I concur that such analyses would be very useful to increase the evidentiary value of your contribution. I see that many of such analyses would be underpowered if considered as hypothesis tests, but I don't think that this should discourage you. Simply demonstrating how much coefficients change when, e.g., considering vegan and vegetarian options separately (or as ordinal outcome?) or when including/excluding additional participants would be very informative.</p>	<p>We want to thank you, the editor, and reviewers for your time. We really appreciate the deep engagement. We also wish to highlight that the constructive tone of both the reviews and the editor's decision letter was especially welcome.</p> <p>All changes concerning your comments have been marked in <i>red</i> font in the manuscript.</p> <p>We have decided to do three additional sensitivity analyses.</p> <p>Sensitivity A) In accordance with the recommendation of Reviewer B, a different specification for "sustainable" was applied. Instead of collapsing vegetarian and vegan meals into one category, we contrasted vegan vs. vegetarian + omnivore. We applied this to the original models with the data reported in the first manuscript. The reason why we did not opt for an ordinal regression is that we did not know how to add the variable "objective mean CO2 difference" to the model. This variable would be differently defined, depending on the contrast within the ordinal model.</p> <p>Sensitivity B) We have applied a different, less strict data exclusion. Specifically, we removed only participants who did not consent or understand the instructions (for ethical reasons), non-student participants (because they were such a small minority), participants who ate NO meal at all (not at least 2, as in the prior version), and participants who recorded more than 30% (in contrast to 20%) missing values.</p>

	<p>We did NOT exclude people who incorrectly answered the bogus item or said they had carelessly answered the questionnaire (or did not answer that question at all). The direction and significance of the results remained mainly the same, although with different magnitudes of the effects (this refers mostly to Reviewer A)</p> <p>Sensitivity C) We applied the new analysis logic from A) to the models with the data points from sensitivity analysis B).</p> <p>We report all the sensitivity analysis in three new sections in the Supplementary Files.</p>
<p>I did not understand why the investigated factors/relationships were framed as “implicit assumptions”. I think you claim that researchers examining the correlation between motivation and impact make those assumptions, but you do not demonstrate this nor do I think readers can easily follow this claim. I would think that people who examine this correlation and who talk about a motivation-impact gap would explain low correlations with factors/explanations very similar to the ones you investigated here. Perhaps consider reframing the presentation along the lines of: “Here are three requirements that need to be met for the correlation between EM and emissions to be strong – are they met?” or “Here are three potential factors that may attenuate the correlation between EM and emissions (=that may account for the low correlations previous research has found) – do they?”</p>	<p>Thank you for pointing this out. We agree that framing this as an exploration of whether these requirements are met is the better framing for this study. We have adapted the Introduction and section 4.1 accordingly.</p> <p>In addition, we have clarified that our analyses address these requirements at the level of individual behaviors and their associated emissions, rather than directly at the level of aggregated emissions.</p>
<p>In section 2.2.3, please check whether it is possible to further clarify for which period participants reported their meal choices. I think they reported retrospectively, on 25, 26, or 27 January, what they ate on 22-26 January. Is that correct? How could</p>	<p>Thank you very much for pointing this out. Indeed, there was a mistake with the dates. Participation was possible from the 26th to the 28th of January (so Friday to Sunday). No participant accessed the survey earlier than the 26th</p>

<p>people taking the survey on 25 January know what they will have eaten on 26 January? Or am I misunderstanding the procedure? Please add as many details as possible regarding the reporting procedure (and my apologies if I should have overlooked any).</p>	<p>of January, and no participant accessed it later than the 28th of January.</p> <p>We updated section 2.1 accordingly (and added a short sentence to 2.2.3). We also added the exact participation date and time to the updated data file (data.2) we uploaded to the OSF.</p>
<p>H2 postulates the absence of a relationship, and the lack of a non-significant coefficient is then taken as evidence for this absence hypothesis. This seems inconsistent. The available data only allows concluding that you did not find evidence for a relationship, but not that you found evidence for the absence of such a hypothesis. You do address this in the discussion section, but that does not seem to solve the issue that H2 is a hypothesis that cannot be tested with the approach you chose. I suggest adjusting the language accordingly, making sure that at no point, you are suggesting that you found support for H2, and discussing the many explanations that may account for the absence of a relationship in your data. Perhaps you can add a disclaimer already when you postulate H2 in the introduction, clarifying that you expect to see a weak relationship in your data, while acknowledging that you are not able to test H2 in the inferential statistical sense.</p>	<p>To address this, we have revised the manuscript in three ways.</p> <p>First, we added an explicit disclaimer as a footnote immediately following H2a/b in section 1.2. Here, we clarify that these hypotheses are not intended as strict absence hypotheses in the inferential statistical sense. Please let us know if the footnote is what in line with your expectation.</p> <p>Second, we revised section 3.1 to avoid any phrasing that could be interpreted as “support” for H2a/b in an inferential sense. We now consistently describe these findings as an absence of evidence for associations, rather than as evidence of absence, and explicitly link this interpretation to the disclaimer/footnote in section 1.2.</p> <p>Third, we have added alternative explanations to section 4.</p> <p>In addition, we have also adjusted the abstracts. We no longer interpret these results in the abstracts. We only name them.</p>
<p>I inserted some minor formatting/language-related comments in the attached pdf. Please take them into account, but you do not need to respond to those comments in your response letter. I am looking forward to your revision!</p>	<p>Thank you for the comments. We have addressed them accordingly.</p>
<p>REVIEWER A</p>	

	<p>Thank you for your engagement with our work. We sincerely appreciate your input. Please find below our responses to your comments. To make it easier for you to follow the changes we made in response to your comments, we have assigned the color <i>green</i> to you. All changes referring to your comments in the text are in green font.</p>
<p>The abstract states that "<i>emission differences are neither always acted upon nor even recognized</i>" (lines 21-22). The results support that emission differences were not acted upon (H2a), but to my understanding, there were no tests of recognition.</p>	<p>We have adjusted the abstracts accordingly.</p>
<p>The Introduction states that prior research assessing motivation-impact links implicitly assumes sustainable behavior necessarily produces lower carbon emissions (lines 113–121), whereas later it states that a motivation-impact link would be expected if vegetarian and vegan meals emit less than omnivorous meals on average (lines 208–211). The "necessity" versus "on average" formulations appear to reflect different underlying assumptions, either of which could warrant such research, and I suspect that prior work might have well relied on the latter.</p>	<p>Thank you for this helpful comment. We agree that most prior studies likely relied on the assumption that, on average, more sustainable behavior is associated with lower emissions, and that this assumption is generally reasonable. Accordingly, we have revised the Introduction to avoid contrasting "necessary" versus "on average" formulations.</p> <p>Instead, we now state more explicitly that the magnitude of the motivation–emissions link at the behavioral level depends on the size of the actual emission-difference between behavioral options.</p> <p>This revised framing is also more consistent with our statistical approach, as our analyses focus on behavior-level choices rather than aggregate emissions (i.e., individual-level emissions).</p> <p>In addition, following the editor's recommendation, we have adjusted the wording in this paragraph in the Introduction to avoid characterizing prior work as relying on "implicit assumptions". We now frame these points as conditions or requirements that should be met to observe a strong relationship between environmental motivation and emissions.</p>

<p>The recommendation to enhance knowledge about carbon emissions (lines 25-26 and lines 43-44) is intriguing. However, it seems to conflict with the results of H2b showing that knowledge didn't affect choices, and (unlike for emission differences between meal choices) no moderation was tested, i.e., whether environmental motivation would be more predictive of impact among people with more knowledge. Also, there seems to be a minor inconsistency between the translations: the English version of the abstract recommends enhancing knowledge at the point of purchase while the German version seems to recommend enhancing knowledge more generally.</p>	<p>Thank you for pointing this out. Indeed, the recommendation was going beyond what our data can support. It was meant in a more general way, based on previous studies by other researchers (others have shown that providing participants with knowledge about emissions at the time of purchase can increase the uptake of less emitting meals). In other words, if you provide people with knowledge (that they previously might not have themselves), this can be effective. Yet, we have decided to omit this here. We have also decided to adjust the language in the discussion accordingly and frame this not as something directly following from our data. Please see below.</p>
<p>Relatedly, the Discussion (lines 497-498) states that: "<i>Yet such knowledge appears insufficiently precise or salient to impact what meals the participants selected (...). In essence, this means that, even when environmentally motivated, individuals might often lack the precision in knowledge to consistently choose the lowest-emission behaviors</i>". I don't think that this claim is supported by the data, because precise knowledge wasn't measured, so it's unclear how pronounced it would be and whether it would affect choice. Please distinguish more clearly between the null-finding (for general knowledge) and speculations about reasons and avenues for future work. I was specifically wondering whether you would recommend others to use the current or a different measure of knowledge in conceptual replications?</p>	<p>We now state only that our general measure of food-related emission knowledge was not significantly related to meal choice. However, we avoid implying that participants' knowledge was "insufficiently precise," since, indeed, we did not measure more specific knowledge. Thus, we omitted this sentence entirely (section 4)</p> <p>At the same time, we clarify that this null finding does not rule out the possibility that some participants may have actually had more specific knowledge about the emissions of the particular meals offered on that day, which could have influenced their choices. Further, we explicitly frame this issue as an open question and potentially a promising direction for future research (section 4).</p> <p>With regard to measurement, we believe that our scale may be appropriate for assessing general food-related emission knowledge. However, we agree that future conceptual replications would benefit from using a more behavior-specific knowledge measure, tailored to the exact choice context</p>

	(instead of using a more general measure). We have added this to Section 4.3.
Method: Because the exclusions were quite substantial, I recommend reporting sensitivity analyses in the Supplement testing whether the patterns are robust to alternative exclusion criteria.	Please see our comment on the Editor's first recommendation. We adhered to your valid comment and tested a different, less conservative exclusion criterion. We report this analysis in the Supplementary Material.
Avoid the phrasing " <i>highly insignificant</i> " (lines 495-496), because it is not a meaningful statistical concept (if the null-hypothesis is true, any p-value is equally likely).	Please see our response to the comment before.
The practical recommendations (lines 529-542) are helpful. The section could be strengthened by more clearly distinguishing implications that follow directly from the design/results from recommendations drawn from the broader literature, with a focus on the former.	We have restructured this section. In the first paragraph, we highlight implications that follow from our data, while in the second paragraph, we highlight implications that stem from prior research (e.g., providing knowledge at the time of purchase). Again, we hope it is clear enough that this is not deducted from our data, but a more general recommendation based on other studies.
REVIEWER B	
	Thank you for your engagement with our work and for all your comments. We really value your input. Please find below our responses to your comments. To make it easier for you to follow the changes we made in response to your comments, we have assigned the color <i>purple</i> to you. All changes that are in response to your comments are in purple font.
The more serious issue is the dichotimization of the food choice into omnivore vs. sustainable (vegan+vegetarian), while only 1/4 of the sample reports to be omnivore and only about 1/8 of the chosen meals were the non-sustainable option in this categorization. Why do this? This seems inconsistent with the argument about explaining emission and other sustainability-goals given in the	Thank you very much for this comment. Please allow us to explain the rationale behind the dichotomization. It is primarily methodological. Before conducting our analyses, we computed difference scores between sustainable and non-sustainable options for emissions (and prize) and then used these differences as predictors in the regression models. Because the possible contrasts

<p>introduction, as the vegan option should nearly always be even more sustainable and have even less emissions.</p> <p>And it probably also makes it harder to find effects in this sample, because statistical power should be higher, if the ratio of the two options is closer to 50-50 (if I am not mistaken). I think it is necessary to either give a good argument in the introduction, why this is a better definition of "the sustainable option" than just the vegan option, and why it is a good idea to merge them into one category. Or to present results on how the models change when you use just the vegan options as 'sustainable behaviour' instead of vegetarian+vegan. I would prefer the latter.</p> <p>(When testing this, because of power, you probably, should either only test vegetarian against vegan, or combine omnivore+vegetarian).</p>	<p>(e.g., vegan vs. omnivore, vegan vs. vegetarian, vegetarian vs. omnivore) are not statistically independent, it is not feasible to include multiple such difference scores simultaneously within the same model without introducing redundancy and almost perfect multicollinearity. For this reason, we opted for a single, clearly defined dichotomy.</p> <p>Nevertheless, we fully agree with your and the editor's comment that an alternative categorization, contrasting vegan options against a combined vegetarian and omnivore category, is equally justifiable. Since we did not preregister either, we agree that full transparency is important here and testing other models is a good strategy to show the robustness of the results.</p> <p>We therefore tested this alternative specification as well (two times; once with the same data and once with less strictly excluded data). As reported in our response to the editor's comment, the significance of the results remains unchanged across these model variants.</p> <p>We have also revised the language in the manuscript to better reflect the underlying logic of this analysis. We argue that the magnitude of the emission difference between available options is the key determinant of observed effects, rather than motivational factors alone. It is not really important whether it is true that vegan options are always the least emitting. What is important is that the emission difference varies.</p> <p>We hope this clarification and the additional robustness checks address your concerns.</p>
<p>Section 1: You make a really good argument here. IIRC, Simon & Merten (2024) made quite a similar case in their paper on measuring knowledge about</p>	<p>Thank you for this comment. We have carefully read the article, but see the overlap to be quite limited. However, we have introduced a reference to the limitation section, as we believe the scale</p>

<p>impacts, arriving at 3 very similar points. Maybe its worth referencing them?</p>	<p>developed by Simon and Merten could be a more useful measure of emission knowledge than the one we applied and could be used in future studies instead. Especially if one is interested in a broad knowledge construct.</p>
<p>Section 1.2: You probably should mention/discuss here a bit more that being an animal product (after being related to cows) is the most influential factor by quite some amount for the emissions of a food product (see Poore & Nemecek). So avoiding meat/animal products in general is a very good heuristic or rule of thumb to reduce emissions (at least if the product contains similar amounts of it, your point on mixed foods is very valid)</p>	<p>Thank you, we agree that this has been underdeveloped in the previous version. We have added a short paragraph highlighting why judging emissions is especially difficult for composite meals—while vegan/vegetarian/omnivorous can be a valid heuristic for food items.</p>
<p>Section 1.3: Should the same not be true also for Knowledge? Why dont you assume an interaction here?</p> <p>If you take your own argument, should it not be necessary, that the person a) wants to protect the environment AND b) there is a substantial difference in Emissions AND c) the person knows about it?</p> <p>So an interaction of EM x Emission difference x EK? I know that with your sample size it will probably not possible to get meaningful results on a 3-way interaction, but I am curious if you had any other reason to not include interactions with knowledge in your model, or what your model would look like, if you included either EK x EM or EK x CO₂-diff.</p>	<p>Thank you for this comment. There are several reasons why we did not include an interaction EM x EK (x CO₂e-difference).</p> <p>Regarding the three-way interaction: We agree that such a model would be severely underpowered in our sample. Thus, we did not include it also for this reason. However, more importantly, as outlined below, we also lack a theoretical rationale for specifying this interaction as part of our model(s).</p> <p>Regarding the two-way interaction EM x EK, prior studies often report additive rather than significant multiplicative relations between knowledge and motivation (e.g., Simon & Merten, 2024). While a moderating role of knowledge is conceivable, this remains an open empirical question, and we did not want to extend the scope of the paper by engaging in this debate without a real reason for it.</p> <p>The EK x CO₂e difference interaction is the most interesting, we agree. However, it is important to note that emissions are not <i>chosen</i> outcomes but a direct consequence of the chosen behavior. Knowledge can therefore only affect emissions</p>

	<p>indirectly (just as motivation), that is, only through its influence on behavior upstream. Consequently, interaction in the emissions model would only be <i>interpretable</i> if knowledge (or its interaction with emission differences) also affected meal choices. Knowledge alone did not affect behavior, as evident in our models reported.</p> <p>Still, we tested the EK x difference interaction in the behavioral choice models. This interaction was quite far from being statistically significant ($p = .96$). Given the absence of any indication of an interaction here (and no direct knowledge effect), we do not have a strong basis for specifying the corresponding interaction in the emissions model.</p> <p>That said, we fully agree that if future studies were to find a robust influence of knowledge on behavior (either through a main effect or an interaction), then modeling EK x difference would be theoretically and numerically well motivated.</p>
<p>2.2.1: I think the detailed discussion on the fit-values could be moved to the supplementaries. As you yourself note: The GEB is a well established scale - and the things you report there are not that relevant for your study overall.</p> <p>Also, the part of the audience that is interested in these details probably already knows the difference between infit and outfit.</p>	<p>We have moved everything but the explanation of the dichotomization and the reliability to the Supplementary Material.</p>
<p>2.2.2/2.3.1/2.3.2: I suggest moving the explanation, that the knowledge-scores were z-transformed to 2.2.2., where you describe your measurement of knowledge. You already need that transformation to calculate the mean rho and the correlation with EM reported in 2.2.2, when I saw no mention of this, my first assumption was you did no z-transformation at all.</p> <p>Also, you then dont need mention it twice in 2.3.</p>	<p>We have added a sentence in section 2.2.2 and removed it from both sections 2.3.1 and 2.3.2.</p>

<p>2.3.1: Your data is not only clustered within participants, but also within the weekdays. You did not mention including a random intercept for the day, probably because you did not do it in any of the models.</p> <p>Is the reason for this, that it could lead to artefacts, because of the CO₂-difference is directly linked to the weekdays? You could be a bit more explicit here about that.</p>	<p>You are correct that observations are clustered within weekdays, in addition to participants. However, we didn't include a random intercept for weekday because, in the present design, weekday is not an independent clustering factor but is directly tied to the meal offerings and their associated emission differences. Including an additional random intercept for weekday would thus absorb the same variance and effectively control for the contextual differences we aim to examine by our fixed effect.</p> <p>We added a sentence to both 2.3.1 and 2.3.2 to explain this.</p>
<p>2.3.2.: I dont think everyone in the audience of this journal is that familiar with GLMMs and will instantly understand what "Gamma distribution and log link to CO₂e" means in this context. Maybe you should explain a bit more about this model and how to interpret its results.</p>	<p>We added an explanation of the Gamma distribution and the log-link and explained why it is the valid choice for our data.</p>
<p>Table 2, Model 3: I think the last OR in this column should be in the cell below, shouldnt it? Model3 uses knowledge as a predictor accorting to 2.3.1.</p>	<p>Indeed, thank you for pointing this out.</p>
<p>Table 2 & 3: It probaly would help readers, if you include a bit more guidance on how to read your tables in the notes (or the corresponding texts). The Tables contain lots of information and GLLMs with Odds-Ratios are prbably not that familiar to the audience. E.g: I was quite a bit confused about the realtions between the p-values and the CIs in your table and it took me some time to remember that with OR 1 means no effect and values smaller than 1 mean effects in the opposite direction.</p>	<p>We have added a short paragraph to sections 3.1 and 3.2 explaining the tables in more detail.</p>
<p>Related note: You do not report the rate of participants with a masters and or a bachelors</p>	<p>We have added this to the sample description.</p>

<p>degree in your study. You should, if you include them as predictors!</p>	
<p>Table 2 & 3: Is the inclusion of R^2c in this table useful for the reader, or does it distract from the more relevant R^2m? (especially as it is just the sum of ICC and R^2m)</p>	<p>We have omitted the ICC and instead reported only the R^2m and R^2c. We hope this is also in your interest.</p>
<p>Table 3, Model 5: Why is R^2c here not equal to $R^2m + ICC$? Are you sure, these are the correct numbers?</p>	<p>The reason why the numbers do not always match is that the ICC is adjusted while the R^2c (and R^2m) is (are) not.</p>
<p>Figure 2: Did I understand correctly, that the red triangles are the white diamonds divided by the bars? Maybe you should make this a bit more clear, especially in the note of the Figure.</p>	<p>Yes, your understanding is correct. We have adjusted the caption of the figure to explain this in more detail.</p>
<p>4.3: you should probably also mention, that you only have data on 5 specific days with 6 to 7 specific meal options each, so it's possible that some of the effects are confounded with which participants came in on which day or that some days may feature specific meal components that are more often liked/disliked.</p>	<p>Thank you for your comment. The study is indeed based on a limited set of five specific cafeteria days with a fixed set of meal options, and that day-specific characteristics (e.g., particular meals that are more or less liked) could, in principle, have influenced participants' choices and/or emissions. However, we believe this would be more concerning if we were to argue that the magnitude of our effects can be generalized beyond the study's context. Yet the magnitude of the effects is less of a concern to our conceptual argument here. Nevertheless, we have included this as a potential limitation to prevent others from reading this as a generalizable statement on the magnitude of the effects.</p>
<p>4.3: As noted above, I am not really convinced that the merger of vegan and vegetarian did improve either power of interpretability.</p>	<p>We have omitted this statement here. Please see our response to the editor's and your prior comments, and our sensitivity analysis.</p>

Review Round 2

Comments by Reviewer A

Thank you very much for your thoughtful engagement with the comments. I believe all of them have been addressed well, like through the addition of the sensitivity analyses, the clear distinction between the current findings and directions for future research, and the revised wording around the knowledge findings. I also believe that changing implicit assumptions to requirements/conditions improves the clarity of the argument. It was great to read the manuscript again and I would be excited to see it published!

Comments by Reviewer B

The manuscript has much improved since the first version, and nearly all of my comments were adequately addressed.

I really appreciate your update with the robustness and sensitivity analysis. I was really surprised that the point of dichotomisation had such little effect!

There are two comments left however, where I do not think that the authors answers are completely valid. As the second one in its current form in the manuscript might leave a misleading impression about methods in the literature, I would suggest some clarifications in the manuscript here. But as both issues are nitpicky details that do not change the overall interpretation of the results, I would also be fine with the editor deciding to advance the publication process to not cause any further delays.

Regarding the EK x CO_{2e} interaction:

I don't think your argument against testing the EK x CO_{2e} interaction is completely valid.

The problem here is, that the direction of the effect of EK is potentially different depending on the differences in CO_{2e}:

A person with high knowledge about impacts should not only know on which days choosing the sustainable choice matters.

They should also know on which days it does not matter. And knowing that the difference does not matter today (or that one of the 'sustainable options' is even worse than the omnivore option) should (in theory) decrease the likelihood of choosing sustainable. Hence, whether the effect of EK is positive or negative on the sustainable choice may depend on the day/difference.

When you only test for an EK-main effect on the meal choice, you only test for an average effect over all days - and this averaging over positive and negative effects can result in non existing overall effects. Therefore, not finding a main effect from EK on meal choice might be a consequence of an interaction effect (and not an hindrance to its interpretation).

But probably the reviewer comments of your article are not the best place for this kind of in-depth discussion and I should just finally get around to write an article/comment about this...

As you already tested the effect and reported an absence of it in the answer to my comment, you might consider mentioning it briefly somewhere in the results/discussion of your paper, as I think this is an empirical finding with some potential implications for theory (as argued above).

Regarding the random intercept for weekdays:

Your argument here confuses me, and I also do not think the added half sentence in the manuscript adds sufficient clarity here, yet. While all variance of the CO₂-difference is explained by the weekday, not all variance in the weekday will be explained by the CO₂-difference. Most obvious example: The ratio of available sustainable meals to omnivore meals differs between days, this probably explains some variance in the likelihood of sus. meals chosen. I thought you could use random effects as a 'catch all' for these other sources and that variance for fixed intercepts would be extracted before modeling of the random effects in LMMs (but this may depend on model specification?) I am no expert in (G)LMMs, so maybe I missed something very basic, but what confuses me with your argument here is, that it seems at odds with your own model:

You use a random effect for participant and still include fixed effects directly tied to the person in your model (EM, EK, socio-demographics...). So this does not seem to be a fundamental problem for the model. So is it a problem for model convergence (or model identification), when you include both? For power? Because you have way more degrees of freedom on the participants than on the days? And could including a random intercept for persons, but not weekdays, potentially bias variance allocation towards effects CO₂e diff and/or the interaction?

From a practical standpoint regarding the interpretation of your results this probably makes no difference. But I think for our discipline in general it would be good to be more specific and clear here to avoid a potential spread of misconceptions about methods!

Author Response to the Reviewers Round 2

REVIEWER A	
<p>Thank you very much for your thoughtful engagement with the comments. I believe all of them have been addressed well, like through the addition of the sensitivity analyses, the clear distinction between the current findings and directions for future research, and the revised wording around the knowledge findings. I also believe that changing implicit assumptions to requirements/conditions improves the clarity of the argument. It was great to read the manuscript again and I would be excited to see it published!</p>	<p>Dear Reviewer A,</p> <p>We sincerely thank you for your constructive comments that helped develop this manuscript. We thank you for the time and effort you have put into your reviews.</p> <p>Kind regards, The authors</p>

REVIEWER B	
<p>The manuscript has much improved since the first version, and nearly all of my comments were adequately addressed.</p> <p>I really appreciate your update with the robustness and sensitivity analysis. I was really surprised that the point of dichotomisation had such little effect! There are two comments left, however, where I do not think that the author's answers are completely valid.</p> <p>As the second one in its current form in the manuscript might leave a misleading impression about methods in the literature, I would suggest some clarifications in the manuscript here. But as both issues are nitpicky details that do not change the overall interpretation of the results, I would also be fine with the editor deciding to advance the publication process to not cause any further delays.</p>	<p>Dear Reviewer B,</p> <p>We thank you very much for your engagement. Your comments have been very helpful, and with your feedback, we hope to have constructed an engaging argument.</p> <p>Thank you for your time and effort. We have addressed your comments below.</p> <p>Kind regards, The authors</p>
<p>Regarding the EK x CO_{2e} interaction: I don't think your argument against testing the EK x CO_{2e} interaction is completely valid.</p> <p>The problem here is, that the direction of the effect of EK is potentially different depending on the differences in CO_{2e}:</p> <p>A person with high knowledge about impacts should not only know on which days choosing the sustainable choice matters.</p> <p>They should also know on which days it does not matter. And knowing that the difference does not matter today (or that one of the 'sustainable options' is even worse than the omnivore option) should (in theory) decrease the likelihood of choosing sustainable. Hence, whether the effect of EK is positive or negative on the sustainable choice may depend on the day/difference.</p> <p>When you only test for an EK-main effect on the meal choice, you only test for an average effect over all days - and this averaging over positive and negative effects can result in non existing overall effects. Therefore, not finding a main effect from</p>	<p>Thank you for this comment. Genuinely, we truly believe you may be correct here with your general claim. We have added a footnote (2) in section 3.1 where we briefly mention that we have tested it, and that it was non-significant in our current sample. We hope this acknowledges the fact enough for the current manuscript.</p> <p>That said, and regardless of our manuscript, I believe that one should and potentially can test the idea you outline.</p> <p>Probably the most difficult issue is that motivation/attitude or whatever construct one might opt for measuring people's commitment to environmental protection, will at least moderately correlate with knowledge. This will, of course, lead to multicollinearity and suppression effects, and simple multiple regression models, such as those used by us, will have difficulties cleanly partitioning the variance between knowledge and, say, attitude.</p>

<p>EK on meal choice might be a consequence of an interaction effect (and not an hindrance to its interpretation).</p> <p>But probably the reviewer comments of your article are not the best place for this kind of in-depth discussion and I should just finally get around to write an article/comment about this...</p> <p>As you already tested the effect and reported an absence of it in the answer to my comment, you might consider mentioning it briefly somewhere in the results/discussion of your paper, as I think this is an empirical finding with some potential implications for theory (as argued above).</p>	<p>This is especially problematic for knowledge, if, and as is mostly the case, attitude and related constructs are the <i>stronger</i> predictors of PEB. In this case, knowledge will always struggle in explaining much unique variance (as indeed found, e.g., by Simon & Merten, 2024).</p> <p>That said, with increasingly advanced statistical possibilities (such as bifactor models, specifically bifactor-(S-1) models [Eid et al., 2017]), one might be able to partition variance more cleanly already at a latent level, given a theory about the latent relation, and not let only regressions decide where and how variance is attributed to correlated predictors. Our current sample is too small for such analysis, though.</p> <p>We would love to see such an exploration.</p>
<p>Regarding the random intercept for weekdays: Your argument here confuses me, and I also do not think the added half sentence in the manuscript adds sufficient clarity here, yet. While all variance of the CO₂-difference is explained by the weekday, not all variance in the weekday will be explained by the CO₂-difference. Most obvious example: The ratio of available sustainable meals to omnivore meals differs between days, this probably explains some variance in the likelihood of sus. meals chosen.</p> <p>I thought you could use random effects as a 'catch all' for these other sources and that variance for fixed intercepts would be extracted before modeling of the random effects in LMMs (but this may depend on model specification?)</p> <p>I am no expert in (G)LMMs, so maybe I missed something very basic, but what confuses me with your argument here is, that it seems at odds with your own model:</p> <p>You use a random effect for participant and still include fixed effects directly tied to the person in</p>	<p>Thank you for pushing back on this point. In hindsight, our original response and the slight changes in the manuscript were indeed imprecisely worded, and we appreciate your persistence in flagging this.</p> <p>In our previous response, we argued that including a random intercept for weekdays “would absorb the same variance” as the CO₂e emission difference fixed effect. This was implying it was a choice between two equivalent and redundant options. You are right to find this not fully convincing. Indeed, weekday and CO₂e emission differences are not 100% the same, since other day-level factors (such as you point out, e.g., the ratio of sustainable to omnivorous options available) also vary across days and are not perfectly captured by CO₂e emission differences alone. That said, in principle, your intuition is correct.</p> <p>The reason why we did not include the random intercept is more specific to our data. In our</p>

<p>your model (EM, EK, socio-demographics...). So this does not seem to be a fundamental problem for the model. So is it a problem for model convergence (or model identification), when you include both? For power? Because you have way more degrees of freedom on the participants than on the days? And could including a random intercept for persons, but not weekdays, potentially bias variance allocation towards effects CO₂e diff and/or the interaction?</p> <p>From a practical standpoint regarding the interpretation of your results this probably makes no difference. But I think for our discipline in general it would be good to be more specific and clear here to avoid a potential spread of misconceptions about methods!</p>	<p>dataset, each of the five weekdays corresponds to exactly one unique CO₂e emission difference value (with a larger sample of days, this would look different). Then, knowing the CO₂e emission difference tells you exactly which weekday it is, and vice versa. Thus, there is not a lot of variance left for a random effect to capture with 5 observations at the day-level. Therefore, and also because we are interested in the fixed effect of the emissions, not the random effect of the days, we included the former. Nevertheless, we also tested, adding the random intercept (and found a worse model fit [higher AIC and BIC], with, however, very similar results in general)</p> <p>We have revised Section 2.4.1 to make the argument behind our modelling choice explicit from the outset, replacing the previous imprecise wording (i.e., the half sentence). We hope this now provides the methodological clarity you were asking for.</p>
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