Response to Reviewers

We would like to thank the editor and all the reviewers for their engagement with our manuscript; we greatly appreciate their constructive feedback. We have significantly revised the manuscript to address all the issues that were raised. Below we provide detailed responses to the reviewers' comments and describe the changes we have made in the manuscript. Our line numbers reflect the location of the change in the clean manuscript.

General & Technical Edits

We have slightly edited the title of the manuscript, replacing "role" with "effect of information about" climate tipping points, as this more accurately describes the phenomenon we investigate.

Reviewer 1

General comments

"[...] it is difficult to fully assess the quality of the manuscript and the underlying research given that critical information on the experimental treatment, questionnaire, codebook, and the data is missing."

Response: The omission of this material in our initial submission was an oversight. We have included this important information in supplementary materials to the revised manuscript, such as the texts used as experimental interventions in our study (translated from Norwegian) in Appendix A, details of the survey questions (translated from Norwegian) in Appendix B, our codebook, and the responses from the open answer questions (translated from Norwegian) in Appendix C, and a supplementary figure from our ANCOVA analysis in Appendix D.

"[...] Some sections of the manuscript seem half-finished (e.g., some headers and sentences appear twice)."

Response: We have corrected the repetitions where headers appear twice and other such errors throughout the manuscript.

Introduction

"Lines 28-30: "Among the many reasons for his [sic!] inadequate response to the climate challenge (Stoddard 2021), public risk perceptions and the corresponding support for climate action have been paramount." First of all, I would not use the term 'paramount' – other factors can be considered as equally important as policy support and risk perceptions.

Response: We agree, and have changed the wording in the introduction, now referring to the "important role" played by risk perceptions in societal responses to climate change (and the lack thereof) (lines 28-29).

Secondly, if the authors wish to invoke the concept of policy support, then I would encourage them to explain the interrelationships between policy adaption, policy support, and public perceptions of climate change more clearly while citing recent sources such as Yeganeh et al. (2020) and Bergquist et al. (2022).”

Response: Thank you for pointing us towards these relevant publications. We have added a sentence clarifying our understanding of the relationships between risk perceptions, policy support and policy adoption, referencing Drews and van den Bergh (2015) in addition to Yeganeh et al. (2020) and Bergquist et al. (2022) (lines 29-31).

“Lines 30-33: “[Lenton et al. (2008] argued that the persistent lack of climate urgency, i.e., insufficiently high-risk perceptions, stems from a “false sense of security” (p. 1792) founded in smooth, gradual projections of climate change.” Lenton et al. (2008) merely say that “Society may [!] be lulled into a false sense of security by smooth projections of global change” (p. 1792). The ‘may’ should be included in the indirect citation as well -> may stem from… Aside from this, I encourage the authors to be more careful when invoking concepts such as risk and urgency (not to equate them). A person who perceives climate change as a significant threat may not necessarily perceive it as an urgent threat. Communicating the risks associated with climate tipping points may affect both types of perceptions. This could be further explored in the introduction or discussion section.”

Response: Good points, and we have edited our reference to Lenton at al. (2008) that “society may be lulled into a false sense of security by smooth projections of global change” as we do not wish to miscommunicate the message (lines 32-33). In our revised version, we have paid careful attention to not equate risk (severity) and urgency, acknowledging that they are related but distinct and may be perceived differently by individuals.

<u>Literature review</u>

“Sections 2.1 and 2.2 contain a lot of information, not all of which is relevant to the focus of the present study. The sections could be shortened to create a more consistent and clear narrative structure. For instance, when the tipping point risk characteristics are explained, the authors should focus on the main points; each of the characteristics could be explained in only three or four sentences. To structure this section, it could also help to first highlight that the reorganization/state shift is the focal process and the remaining characteristics merely describe how this process unfolds (i.e., in a nonlinear, potentially abrupt fashion) and what it entails (i.e., potential for severe, long-term impacts…)”

Response: We agree with this assessment (as do other reviewers) and have accordingly condensed the literature review provided in section 2.1. In 2.2, we have shortened the description of risk-relevant characteristics of tipping processes. However, in some cases, we found it was necessary to add a sentence or two to provide previously missing definitions (e.g., of positive feedback in lines 139-143). We have also reorganized this section, starting with a continuous discussion of the four main characteristics of tipping processes, which is followed by a description of additional features that could affect risk perceptions (e.g., the severity of potential impacts).

"This brings me to the next point: there is not a clear consensus as to whether 'irreversibility' should be considered a CTP characteristic (e.g., Armstrong McKay et al., 2022). After all, irreversibility is a high standard that is extremely difficult to prove. The authors acknowledge this in some parts of the manuscript when they use alternative terms such as 'limited reversibility' and 'irreversible at human timescales'. For reasons of consistency, I would choose a single term, perhaps limited reversibility (or another term that carries the same meaning). From a risk analysis perspective, this can also be framed as a question of controllability: once a tipping point has been crossed, it becomes much more difficult for humans to control exactly how a natural system operates. This is because the regime shift comes with a certain degree of stability/persistence."

Response: This is a very important observation, in which we fully agree. Also, the issue of reversibility or not is a matter of time, and we highlight this by referring to "timescales that are relevant for humans" on line 157. To address this issue, we are using the term "limited reversibility" throughout the manuscript and specifically refer to human-relevant timescales, and we explain what this means in section 2.2 (lines 152-160).

"Section 2.1: The authors may want to engage with more recent publications from the field of public perceptions of climate change. Importantly, some of these publications show that in many countries climate change is not perceived as a distant risk anymore (e.g., van Valkengoed et al., 2023; I strongly recommend this review)."

Response: We are very thankful to the reviewer for highlighting such relevant literature. We have expanded our section on the field of public perceptions of climate change, specifically adding a section reflecting the decline of temporal distancing in public perceptions of climate change, referencing van Valkengoed et al., 2023 (lines 92-94).

"Sections 2.1-2.3: The summary of the previous literature contains statements that are imprecise and potentially misleading. For instance, the authors state that "[Bellamy and Hulme (2011)] found that climate tipping points increased concern only among participants with an egalitarian value set […]" (lines 196-198). The study that is cited here is a cross-sectional study, not an experimental study. Bellamy and Hulme (2011) merely found that egalitarians were most concerned about climate tipping points. It cannot be concluded that there was an 'increase' in concern or any other variable."

Response: We greatly appreciate the Reviewer's in-depth knowledge regarding the referenced studies. We have made several corrections and clarifications in this section, e.g., stating that Bellamy and Hulme (2011) "found that concern about climate tipping points was higher among participants with an egalitarian value set while also generating a fatalistic narrative among study participants" (lines 207-208).

"In their short summary of the study by van Beek et al. (2022), the authors write that "[van Beek et al. (2022)] observed an increase in concern and perceived seriousness of climate tipping points" (line 190f). However, it should be noted that the changes that van Beek et al. (2022) registered on their quantitative measures of concern and seriousness were nonsignificant. That is, the present summary of van Beek et al. (2022) seems to disregard the quantitative findings and only reports the results from the qualitative analysis."

Response: In our reading of van Beek et al. (2022), they concluded that the qualitative findings outweighed the quantitative ones due to limitations of the quantitative analysis and potential ceiling effects. Hence, our literature review referred to their qualitative findings. However, we have now provided a more detailed report on this paper, which includes a mention of the null-finding in the quantitative component of their analysis (lines 197-202).

"Line 204f: "[…] a study by Formanski et al. (2022) found no difference between climate risk perceptions related to linear versus non-linear climate change". Given that the present study is in many ways similar to the Formanski et al. (2022) study – e.g., in terms of the design and the dependent variables (for RQ-2) – it would make sense to briefly describe the methodological approach and explain the main findings of this previous study. In general, I encourage the authors to pay particular attention to the most recent publications in this field that directly relate to their research questions – e.g., Bellamy (2023, -> RQ-1) and Formanski et al. (2022, -> RQ-2)."

Response: We agree with the Reviewer regarding the relevance of the two studies by Bellamy (2023) and Formanski et al. (2022), and the value of a more detailed comparison of their approach and findings with ours. Here, we have expanded our section detailing the study conducted by Formanski et al. (2022), providing a comparison of key methodological similarities and differences with implications for the interpretation of our findings (lines 210-216). We also added a comment on this in the methods section (lines 287-290).

<u>Methodology</u>

"In part 1a, all participants were asked a series of questions about their climate change risk perceptions" (line 256f). The authors need to provide a list of all questions that were asked here, as well as an explanation for how the responses to these questions were processed/aggregated. The authors also need to disclose all questions that were part of the "tipping point knowledge test".

Response: All survey questions about risk perceptions can now be found in the supplementary material in Appendix B - questions 1 - 3 pre-intervention and questions 6 - 8 post-intervention. The questions in the climate tipping points knowledge test have now been included in the supplementary material in Appendix B question 4. A description of how the survey responses were processed has now been added (lines 294-295).

In the results section, the authors present statistics about which tipping point characteristics were most frequently identified. However, from the method section, it is not clear which question stimulated these answers. Section 3.1 only states that participants were asked to name an example of a CTP."

Response: We have now clarified in section 3.2 (Analysis) that the responses to the prompt to provide an example of a climate tipping point were coded to distinguish groups of participants with different levels of knowledge (lines 322-325). We expand that some participants provided examples of climate tipping elements while others provided characteristics and why we found this meaningful data despite characteristics not being prompted by our question (lines 325-327).

“Appendix A – which is said to present the stimulus materials (i.e., information packages) – is missing in the document.”

Response: We apologize for this oversight. The stimulus materials have now been included in the supplementary materials to the revised submission in Appendix A.

“Sample composition: “A nationally representative sample was recruited […]” (line 275f). The authors need to specify in which sense the sample is ‘representative’. To me, it looks like the sample is a quota sample, not a probabilistic sample. That would, however, mean that the sample is only representative of the Norwegian general population in terms of selected demographic characteristics (e.g., sex, age, region). If that is the case, the authors need to state this explicitly and provide the quota plan.”

Response: Our survey was performed in close collaboration with a professional polling company which use standard criteria for representative recruitment. We have however expanded the description of the study participants to reflect that our sample was a quota sample to recognise there are members of the Norwegian public that may be overlooked in our study (lines 301-305). We hope that our description now more clearly communicates the selected demographic characteristics.

“Data analysis (knowledge): Is a qualitative categorization necessary? The authors could just present the familiarity ratings and the frequencies for correct/incorrect CTP examples – and then they could probably draw the same conclusions from this data. Yet, this would not require the presentation of a category system. However, if the authors still wish to use a categorization procedure, then I would advise them to avoid the category label “no knowledge” and to use “no demonstrated knowledge” (see Figure 3, p. 13) instead.”

Response: While a categorization might not be necessary, we believe it is more insightful and better reflects the complex cognitive reality of ‘knowing’ than merely providing frequencies for correct and incorrect examples. However, we have adjusted the first category label to “no demonstrated knowledge” since we cannot conclusively state that those participants have “no knowledge” and this is now consistent throughout the manuscript. We have also expanded that there are likely several possible explanations for why they did not provide written evidence of their potential familiarity with the concept (lines 315-318).

“Data analysis (risk perception): Instead of conducting separate independent-sample t-tests on the test scores for t1 and t2, it would be better to conduct an ANCOVA on the post-test scores, with the pre-test scores as a covariate. This is an elegant way to test whether there are differences between the experimental groups at the post-test stage while taking the pre-test scores into account. The paired-sample t-tests (from line 383 onwards) can then be presented as simple ‘follow-up analyses’.”

Response: We conducted an ANCOVA analysis on the post-test scores in order to strengthen the validity of our findings in the risk perception analysis and have included the results in this section. We have also included standard deviations for each experimental condition, as well as exact p-values and standardized effect sizes in the manuscript (lines 408-418).

Results

"Section 4.1, 4.4: The authors should try to meet the journal article reporting standards for empirical research articles in psychology and social sciences. That is, mean values and standard deviations should be reported for each experimental condition (e.g., in a table), as well as exact p-values and standardized effect sizes."

Response: We have included a table which includes the mean values and standard deviations should be reported for each experimental condition and the exact *p*-values for the ANCOVA and t-tests are now included in the text (lines 408-418).

"Section 4.1: The authors find that only a few survey respondents rated the information on climate tipping points as "new to them" (see lines 339-341). It is concluded that this indicates socially desirable responding. However, the authors should keep in mind that the free recall of memorized information is generally more difficult than the recognition of memorized information. It is possible that many survey respondents had previously heard of specific tipping elements but were unable to recall that information during the free recall task ("name an example…"). The authors should therefore not simply dismiss their findings on this measure as socially desirable responding. While social desirability could certainly play a role, the present findings could also be an indication that laypeople may be more aware of CTPs and the consequences of unmitigated climate change than researchers often assume (even though that CTP knowledge might not be highly accessible, as it was only activated through the confrontation with specific stimulus materials). The authors should also consider the possibility that many laypeople may be aware of the catastrophic consequences of unmitigated climate change, but they may not associate these impacts with the concept of tipping points or simply do not know the relevant terminology. This tends to be a principal weakness of studies that only ask people 'whether they have heard of climate tipping points'. Thus, the present results give us limited insights into laypeople's expectations about how climate change will unfold in the future. This is yet another reason why the authors need to be cautious in their interpretation of the present results."

Response: The Reviewer makes an important argument, which we now discuss in section 4.1 (lines 373-379). Our methodological approach differs from Bellamy's design regarding the role of free recall, and this deserved some attention.
The Reviewer's comments regarding laypeople's underexplored general awareness of the potentially catastrophic impacts of unmitigated climate change are apt. However, we did not seek to investigate these general beliefs about the nature of future climate impacts; we were interested specifically in the effect of knowledge about tipping points on climate risk perceptions. While we do not discuss this issue in our manuscript, future research should explore this phenomenon.

Discussion

"Lines 418-421: "Our results contrast with recent findings by Formanski et al. (2022) […]. One explanation for this difference might be that Formanski et al focused on a single characteristic of tipping points (non-linearity), which might not be the feature that generates most concern". The treatment materials by Formanski et al. (2022) also highlighted another feature – severe impacts. However, it is true that other features such as the limited reversibility of the impacts were not explicitly mentioned, which is a valuable observation. At

the same time, the authors could also consider the fact that there were other differences in terms of the specificity, scope, and length of the materials - Formanski et al. (2022) acknowledged that the length and simplicity of the materials could explain their null findings. A simple reframing of climate change as a dynamic phenomenon (with the help of the CTP concept) may not be enough to increase concern; yet the current study indicates that a more elaborate discussion of CTPs and the associated risk characteristics could help!"

Response: We have expanded our discussion of why our results differ from Formanski et al. (2022), why this might be, and how future research could address this more thoroughly. As mentioned above (lines 477-485), we also provide more detail on how this study compares to ours in the methods section.

"Lines 426-429: "We did not observe any effect of information of climate tipping points on beliefs about whether or not it is too late to act on climate change. This could be attributed to the public's tendency to downplay the seriousness of these risks due to certain cognitive biases, and that systematic risk associated with climate tipping points pose unique learning challenges that is not easily grasped by participants". First of all, the authors could point out that the null finding on this item is consistent with the results that Formanski et al. (2022) report for the dependent variable 'efficacy beliefs'. Secondly, the explanation that the authors offer for the null finding would only seem plausible to me if the text presented to participants had, in some way, suggested that the crossing of multiple CTPs is inevitable – why else would they be prompted to believe that it is too late to act on climate change?"

Response: The Reviewer's point made us reconsider the inclusion of this finding and argument. We decided to remove this section, and no longer present our results on our participants' response to "whether or not it is too late to act on climate change".

Conclusion

"The excurse on social tipping points (from line 475 onwards) comes a bit out of nowhere. The authors may want to consider deleting this part or embed it more deliberately, so that it fits into the conclusion."

Response: We have removed the discussion of social tipping points from our conclusion. While we find them relevant to the conversation on climate tipping points and public risk perceptions of climate change, this was not discussed or elaborated on in the body of our work, therefore does not have an obvious place in the conclusion.

Technical corrections

"Stoddard (2021)" is missing in the references"

Response: we have added Stoddard (2021) and checked (and corrected) other errors with regards to the references.

"Some publications that are listed in the references section are not cited in the text (e.g., Trope & Liberman, 2010)"

Response: We have screened the manuscript for errors relating the references.

“Authors names -> correct spelling errors (e.g., Russil -> Russill)”

Response: (Line 188) We have corrected this error and screeded the manuscript for other such errors.

“Throughout the manuscript the authors frequently use phrases such as “could be affected by climate tipping points” (line 58). And when they introduce the concept in line 35, they state that “Climate tipping points refer to dynamics in the Earth system […]”. Technically, tipping points are not ‘dynamics’ within the climate system – they are thresholds; hence, tipping points do not ‘affect’ countries – only the impacts that the crossing of tipping points has can affect countries. I presume that the authors mean the processes that are initiated by tipping events, not the ‘tipping point’ itself?”

Response: We have been clearer in our terminology with regards to the difference between a tipping point and the change process associated with the passing of a tipping point throughout the manuscript. We have also corrected the statement that climate tipping points are not dynamics, but thresholds in the Earth system in order to be as clear as possible when distinguishing climate tipping points from the impacts and processes they set in motion.

“The authors use formulations such as “the effects of climate tipping points on” (line 70). I presume that the authors mean the effects of the ‘presentation of’ or ‘exposure to’ information on climate tipping points?”

Response: We do indeed refer to the effects of the exposure to knowledge or information about climate tipping points; we have corrected this instance (line 70) and other such instances in the manuscript.

“Line 323: “50% indicated little or no familiarity” – in case two categories were combined here, the authors should provide frequencies for each category (“little familiarity” and “no familiarity”)”

Response: The frequencies have now been provided for each category (lines 352-354).

Reviewer 2

“The paper provides excellent background on risk perceptions of climate change in general and climate tipping points, as well as on the Norwegian context for the study. The section on risk-relevant characteristics of tipping points, however, seems a little too long. The main points could be made more succinctly.”

Response: We acknowledge this very positive judgement of our manuscript. The section on risk-relevant characteristics was also highlighted by Reviewer 1, and we have accordingly now significantly reduced the amount of information in section 2.2 in order to focus more clearly on the key messages.

“At the same time, this section also seems a little too prescriptive. It is rightly noted that definitions of climate tipping points are varied, but it would be worth unpacking the divergences in more detail and adopting either a more flexible understanding of the concept,

or alternatively being prescriptive about only those features that are common to all climate tipping points. For example, irreversibility is identified as one of the key features (and by the public themselves), but this is in fact not a feature of many climate tipping points, e.g., Arctic sea ice, Atlantic Meridional Overturning Circulation, ocean acidification, etc. So, it can be contested as to whether identifying 'irreversibility' would contribute towards a 'correct' understanding of climate tipping points. In other words, it would be good to see some reflection on the problematic nature of 'knowledge' about climate tipping points, and what the implications are for the present (and future) research."

Response: This is a core point, also commented on by Reviewer 1, and the issue of irreversibility (now referred to limited reversibility) has been elaborated more (lines 152-157). This qualification no doubt also depends on the post-tipping time horizon. It was not our intent to come across as prescriptive in this section, but to identify the risk-relevant features common to climate tipping points in the literature (and based on emerging agreement on tipping point definitions in recent reviews such as Armstrong McKay et al., 2022 and Milkoreit et al., 2018 which both identify limited reversibility as a feature of climate tipping points). We have been more specific that we have chosen to focus on only certain characteristics, recognising the divergencies while stating our focus (lines 126-129).

"A more minor point is that in some places, notably lines 100 to 106, there are repetitions of the text."

Response: We have corrected this error in the text and removed the repetitions.

Reviewer 3

"With regards to the introduction, it is quite hard to discern what is the research question that the study is exploring. It only comes until line 45-55 (4th and 5th paragraph) where readers finally understand the intention of the manuscript. The introduction would significantly benefit if the research question were brought earlier on in the first paragraph and then provide information on what has been done and why it is important to explore climate risk perceptions of tipping points from the public in Norway."

Response: We agree that it is important to arrive at the research questions early on and have revised the text accordingly by the research question earlier into the introduction (lines 36-37).

"There are three issues I would like to raise on the literature review on climate risk perceptions (section 2). First, I would like to suggest including a brief overview of the literature that studies affective dimensions of climate risk perceptions, as this is another growing field in the literature that significantly influences how individuals perceive climate risks."

Response: We have thoroughly considered this, but decided not include a section addressing the literature that studies affective dimensions of climate risk perceptions, as our study does not explore this, and the text is already quite extensive. As the Reviewer notes, the literature review should focus on the specific topic of the research, and we did not include affective or emotional variables in our work. However, we understand that it is an

important area of research and should be considered in future research when investigating risk perceptions of climate change.

"Second, I don't understand the difference between section 2.1 and 2.2 that both talk about risk perceptions (it may be a typo). This needs attention.

Response: Apologies and yes, this error in the headlines has been addressed in the manuscript and corrected. Section 2.1 is about general climate risk perception scholarship; section 2.1 concerns the risk-relevant characteristics of climate tipping points.

"Three, section 2.2 "Risk-relevant Characteristics of Climate Tipping Points" and 2.3 "Perceptions of Climate Tipping Point Risk" are far too long and can be summarised. I think it's important to remember that the study is about the public in Norway and how they perceive climate risks about tipping points. The literature review and background information should provide enough context for this, so this section can significantly be summarised to be more specific about what readers need to know (think about the audience for this piece and what they may already know)."

Response: This is an important observation made my all the reviewers. We have shortened sections 2.2 and 2.3 in order to communicate the information more concisely, which is followed section 2.3 which discusses the significance of Norway as a case study.

"With regards to the description of the study participants, it would be interesting to provide more information (if possible) to know which audience may have not been able to be represented with this method. It is important to be clear about the limitations of our studies, and this is one where it is likely that some publics within Norway remained invisible (e.g., immigrants, international students, refugees, people with some disability, non-binary, etc.). Also, what is the level of study from study participants?"

Response: This is a very important comment which should be considered in all studies that seek to provide a nationally representative sample of a population. We have been more transparent on the limitations of our study where it comes to accurate representation of the Norwegian population (lines 301-305). It is important to recognise members within a population that remain invisible using our methodology and a limitation within our study (lines 304-305).

"Where the results of the t-tests independently verified? And if so, how?"

Response: We used a *t*-test for independent means which has been clarified in the text. We cannot state that our results were verified by an unbiased and impartial third party, however, we conducted two statistical tests and yielded statistically significant results in both tests (ANCOVA as recommended by Reviewer 1 and *t*-test for independent means). However, this is a limitation in our study and is now declared in our manuscript as such (lines 480-481).

"Finally, on the conclusions, the manuscript fails to provide meaningful recommendations to other potential audience of this study (e.g., government and academia). If the authors believe that the public in Norway need to know about tipping points, then it would be important to explain this. In other words, the conclusion should describe why this knowledge

is necessary and what is needed or recommended to disseminate this knowledge. For example, if people in Norway understand the risks of tipping points, could we expect some sort of change? In what way?

Response: We agree and have included recommendations in sections 5 and 6 in the latest manuscript submission which we believe elaborates on recommendations and expanded on why knowledge on climate tipping points may be important to the public.

"[…] This takes me to my next comment. The manuscript should provide future research opportunities and also be clear about the limitations of the study."

Response: We have expanded upon future research opportunities in the discussion and conclusion (sections 5 and 6), and also been clearer about the limitations of our study.

Minor comments

"Page 1 line 25: Incorrect factual statements are made in the introduction when referring to "internationally agreed-upon targets". Nationally determined contributions are the opposite of internationally agreed. The Paris Agreement is based on a bottom-up self-determined approach."

Response: Thank you for highlighting this error in our wording regarding the Paris Agreement. We have changed the phrasing (Line 26) to reflect the fact that the Paris Agreement contains global goals that have been internationally agreed, but no (emission) targets for individual countries.

All of these changes to the manuscript can be found in the marked-up copy which has been included with our resubmission.

I sincerely hope that these modifications align with the expectations of the reviewers and the editor. I am confident that the revisions have strengthened the overall quality of the manuscript.

On behalf of myself and my fellow authors, I thank you again for considering our manuscript and look forward to your feedback and decision regarding our research paper. Please do not hesitate to contact me if further clarification or additional information is required.

Kind regards,

Christina Nadeau