nature portfolio

Peer Review File

Polar Ice sheets are decisive contributors to uncertainty in climate tipping projections

Corresponding Author: Mr Jonathan Rosser

Attachments originally supplied by the reviewers can be found at the end of this file, in order by reviewer number and revision.

Version 0:

Decision Letter:

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Dear Mr Rosser,

Your manuscript titled "Cryosphere tipping elements decisive for tipping risks and cascading effects in the Earth system" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers. Please also consider the editorial thresholds below:

*Provide a more thorough discussion of the limitations of this study such as the robustness and validation of the representation of interactive climate tipping points for different timescales, regions, physical processes, and relevant uncertainties inherent in PyCascades model
*Provide further justification and motivation for tipping point parameters in PyCascades model

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

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Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Kyung-Sook Yun, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-9990-3581

Joe Aslin
Deputy Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This study investigates interacting climate subsystems including tipping elements (GIS, WAIS, AMOC and AMAZ), ENZO and ASSI. Two main analysis are presented. First, Sobol analysis is used to infer which input uncertainties (on critical thresholds, link strengths and timescales) contribute most in the outcome uncertainty on the mean number of tipped elements. There, the authors find that the uncertainty on polar ice sheet related critical thresholds have most impact at the lower level of warming used, namely 1.5 deg. C above preindustrial level.

Second, a "leave one out" analysis is used to infer how important it is to represent/include those components in models when aiming to predict potential (cascading) tipping phenomena. There, it is found that discounting or removing a node or link can have major impact on the outcome. Especially, removing the GIS has the biggest impact at lower temperature, while removing the AMOC has the biggest impact at higher temperatures. It also shows the importance of including the AMOC->GIS and GIS->WAIS link.

The motivation is to give directions in where modelling efforts may be the most relevant, by suggesting which parts of the climate must be included or better constrained. As the title suggests, the authors conclude that priority should be given to cryosphere tipping elements.

I kindly ask the authors to address the two following major comments (labelled M1 and M2), without which I cannot support publication in Nature Communications Earth and Environment. I also provide some more specific comments (labelled s1-48).

M1. The PyCascades model was explicitly presented as a conceptualisation of interacting tipping elements. It represents generic bistable systems under linear interaction, as well as their collapse timescale. While I fully agree with the authors that including non-tipping elements is such a study makes sense, ENZO and ASSI are not tipping element (as acknowledged by the authors in e.g. 92-93), and therefore are not reasonably captured in PyCascades. Especially, the motivation for doing so (given in section 1 of the supplementary material) is, in my opinion, insufficient. Indeed, it mainly motivates their inclusion based on the fact that they are important for other tipping elements, but does not justify the use of bistable systems with critical thresholds and hysteresis behaviour to represent them. This challenges the conclusion, as the importance of ENZO or ASSI, and of any other element represented in this study, may strongly depend on this representation.

M2. I find that the results are way more nuanced than the title, abstract and conclusion. In particular, cryosphere tipping elements include GIS and WAIS. While it is true that the importance of GIS is shown through most of the analysis, WAIS only appears relevant sporadically (in the Sobol analysis applied to the 1.5 deg. C warming and through its link to GIS). For the rest of the results, the AMOC is much more pushed forward as a crucial component in itself and for the links it has with other elements.

s1. I would strongly suggest sharing the code that produces the results and figures.

s2. In the title, I would suggest to replace "cryosphere tipping elements" by "polar ice sheets" as this can bring confusion regarding ASSI.

s3. Through the text, I think that "disintegration" should be replaced by "collapse" or something similar. I would only find this term relevant in the case of ice sheets.

s4. 13: This only refers to part of the results, see M2.

s5. 4-6: It feels awkward not to cite the AMOC there as it is one of the most central tipping elements (also in this study).

s6. 8: Stability is quite a precise term in the context of dynamical systems. Maybe replace by "safety" or something similar.

s7. 10: I am not sure of what "structural uncertainties" means here.

s8. 12: I would write "climate components".

s9. 37: This should be reformulated (tipping poses risk, not Earth system components).

s10. 42: Sufficiently would be more just and less trivial.

s11. 46-50: There is a weird punctuation structure ":-;-;-:-." there.

s12. 54: I believe it would be nice to give more context to cascading tipping events, for which some key references are already given in the bibliography (e.g. Dekker, Klose, Wunderling).

s13. 66: "First" is not well announced (what is going to be enumerated?) and is not followed by a "Second" or something similar.

s14. 67: I would remove "point".

s15. 70: The coupling from what?

s16. 72: I am unfamiliar with the "knock-on" term. I believe it would need either a clarification or reference.

s17. 74: This statement would deserve a reference.

s18. 75: Does "Earth system component" refer to the biosphere?

s19. 77: Do you mean lack of meltwater forcing originating from melting ice sheets? Also, isn't "poor dynamical representation" a bit unfair?

s20. The end of the introduction is a bit brutal. It could end on what will be done (like the first paragraph of the "Modelling approach" section).

s21. 83: I would replace "have used" by "use".

s22. 99: Here, I would suggest being more transparent on what the model actually does, as it is important for the framing of the study. Especially, it simplifies tipping elements into linearly interacting, generic bistable systems. This is somehow said in the Methods section but is very relevant at this point.

s23. 107: I would replace by "key climate components" and reserve "element" for tipping elements, for clarity.

s24. 107-108: I don't really get the sentence "... above preindustrial running equilibrium experiment". Is there a "level" missing after "preindustrial"?

s25. The main text should say exactly what the Sobol Variance quantifies (what this percentage means), either in the text or captions of fig 1 and 2.

s26. As I understand from fig 1-2, no timescales uncertainty significantly contributes in the Sobol variance. Does it mean that this uncertainty does not really impact the outcome? I would appreciate a comment on this.

s27. On fig 1-2, the fact that timescales and threshold temperature are represented in red is confusing, I would suggest black or grey.

s28. 152-154: To my understanding, this is not consistent with fig 2, which shows that most important are the GIS critical threshold and the AMOC->GIS link. Then the AMOC threshold, and then the rest on a similar level.

s29. 179-180: While I understand the intention of the authors and do not have a better formulation, I would suggest to be careful with such sentences, as the model does not represent that. This can be confusing for an unwary reader, especially regarding comment s22.

s30. 209: I do not see that on fig 3. Do you mean fig 4?

s31. It feels awkward not to include WAIS in this analysis as it was pushed forward in both the title and Abstract, see M2.

s32. 221-223: "e.g." are here are confusing as the GIS and AMOC case give opposite results.

s33. 242: Tipping of what? of GIS or in the whole system?

s34. 243: So AMOC is not a critical component at those temperatures? This is what fig 4 insists on.

s35. Fig 4 and 6 only provide representative cases of removal. Why those ones? Are other cases negligible?

s36. 251: I would replace "altered" by "decreased".

s37. 278-279: There seems to be a problem with this sentence.

s38. 289-291: "Missing key dynamics can have significant impact" sounds very trivial as a conclusive statement.

s39. 302-303: Again this is given as the conclusion of the results, which are more nuanced. Sobol analysis indeed shows that for low warming, but leave one out only gives the GIS as an important element in low warming, and AMOC as important element for higher warming. I also see that 247-252 motivate the importance of WAIS but only indirectly, as the reader must refer to the supplementary table 3 to see that this limit percentage is in fact given by the WAIS removal case.

s40. What about the 4 deg. C warming experiment in the conclusion?

s41. 307-308: It would be more just to invert this sentence into "Although the network model used in this study is a strongly simplified non process-based model, it has many common features ..." or something similar.

s42. 326-331: This is a quite strong statement made here. Would one prefer quantity over quality when aiming at prediction of the climate? Is many poor representations better than some more accurate ones? I do not have an answer myself, but I do not believe that the results give an answer to this question.

s43. 339: I would replace "if we believe" by "assuming" (as no one really believes that).

s44. 492-493: There is an inconsistency between the ref here and the ones of the table.

s45. 54(sup): Does the climate tipping point report provide number? what is the "(supplementary information)"? To my understanding those numbers have been chosen by the authors informed by the report, not "taken from".

s46. (sup) How was ENZO link chosen, as it did not appear in previous iterations of PyCascades-related publications? The supplementary section 2 should be clearer about what comes exactly from where (each critical thresholds, timescales and link strengths). As of now, it is quite convoluted.

s47. The caption of sup table 1 has a problem in the last reference (1 is Kriegler).

s48. In general, citations in the supplementary material mix text and number citations.

Reviewer #2 (Remarks to the Author):

Please see the attachment

Reviewer #3 (Remarks to the Author):

Review of

“Cryospheric tipping elements decisive for tipping risks and cascading effects in the Earth system”

by Rosser et al.

Summary:
This paper by Rosser et al investigates the interaction of tipping elements in the climate system. The basic idea is to represent interacting tipping elements as Cusp or Hopf bifurcations where an interaction term is added to the right hand side of the normal form. Depending on the strength of the interaction term, bifurcation behaviour is either strongly or weakly coupled. The modelling itself is relatively simplistic, but allows for sampling of a large subset of parameters, and statistical analysis of the results. The model itself has been described in the published literature together with aspects of the components, but the detailed analysis of the model behaviour applied to the climate dynamics is novel. I thought the paper was generally interesting to read, but the figures and analysis are not as clear as they could be.

The main take-away message from this paper is that coupled simulations of the cryosphere with other climate systems are necessary for capturing the interactions between different components correctly. I think this point is nicely illustrated by this study, and why this point is not revolutionary in itself, I believe highlighting it clearly might provide extra justification for renewed efforts to develop fully-coupled models. (Of course, these efforts are already on-going, and mostly hampered by technical difficulties, rather than lack of motivation).

Main points:
1. The most important question is how valid the representation of interactive climate components as relatively simply connected bifurcation systems is. While I think this model is very useful to provide insights about possible interactions of the climate system, I would like to encourage the authors to make sure they better discuss and acknowledge limitations. For example, cryospheric systems can react on several different timescales to changes in climate, not only one, with each of these timescales representative for different regions and physical processes (e.g., MISI, MICI, melt-elevation feedback etc.). In the model, there is only one timescale. Moreover, the interaction terms are linear, but nonlinearity might turn out to be essential in determining whether tipping of one element can cause the tipping of another.

2. Representation of Arctic Summer Sea Ice (ASSI) and ENSO as tipping elements: As the authors acknowledge, neither can be considered a tipping element. While they clearly have links to the other elements, they should fundamentally be represented by a different kind of equation. Unless my math is flawed, using the parameters provided, Arctic Summer Sea Ice exhibits a hysteresis behaviour, which it shouldn't. Given the relatively simple form of the ODE's underlying the model, I suggest implementing a monotonically increasing component that can represent ASSI and ENSO more adequately. The ASSI tipping point is also frequently referenced throughout the manuscript (e.g., line 138, line 48 of the supplement), after it is established that it is not in fact a tipping element. Please correct.

Other points:
Figures 1 and 2 are not every informative, in particular the difference between the two is only small (in contrast to what is claimed in lines 150-151). I suggest using a logarithmic colorscale for the Sobol variance importance or some other way to emphasize the difference between them. Also scaling the arrow width with the mean strength of the link between two components would be helpful (or potentially providing this information in a separate figure, rather than only the table in the supplement). The strength of the interaction is, after all, key for understanding the results.

Analysis of leave one out removal studies: I think the authors should have gone more in depth here. In particular, I suggest in figure 4 to not stack the number of different tipping points, but to plot the different components (AMOC, GIS, etc) clustered side-by-side (i.e., individual bars for each component), with the y-axis showing the percentage of simulations which show tipping for each component and scenario. This would help illustrate and identify the strength of the links for the different components better. Same for Figure 6.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Rosser,

Your manuscript titled "Polar ice sheets decisive for tipping risks and cascading effects in a conceptual model of the Earth system" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment provided you explicitly clarify that El Niño or Sea ice are not tipping elements and adapt all language that could mislead in this way throughout the manuscript. If suitable we will publish your manuscript under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Kyung-Sook Yun, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-9990-3581

Joe Aslin
Deputy Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/

Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I appreciate the effort of the authors for answering my previous comments, and especially for adapting the experiments for a better representation of ENSO and ASSI.

I have still one important, general comment labeled M1, and some more specific comments labelled m1-28.

M1: Even though the model has been adapted in a way that ENSO and ASSI are not represented as tipping elements, the terms “tipping”, “tipping threshold”, “tipping point”, and “tipping timescale” or “critical temperature” are still used for them or in sentences that include them. This is incorrect and brings confusion in many places of the manuscript, including text, figure title/captions and table content/captions.

m1 38-49: “tipping point interaction” or “tipping point cascades” is vague as tipping points do not really interact or cascade. I see that this terminology is used in the title of Wunderling & al 2024, but I would find “tipping element interaction” and “tipping cascades” more suitable.

m2 40: I would find “…may occur…” more suitable. In this same line, I guess that “This” refers to the interaction? If yes, “Such interactions” or something similar would be clearer as, for the moment, it seems to refer to “cascading tipping”.
Finally, there are quite some references motivating that interaction between tipping elements can decrease the likelihood of cascading tipping, which can be a counterintuitive idea for some readers but is crucial for this study. For example, Ciemer & al 2021, Sinet & al. 2023 or Nian & al. 2023 may be relevant here.

m3 45: a point is missing before “These”. This sentence is again a bit unclear as “These interactions” refers to both stabilizing and destabilizing interactions. Maybe “In a worst-case scenario, these interactions …”?

m4 51-52: was “Large uncertainties about their existence, interactions, and key uncertainties” intended? Uncertainty about uncertainty is possible but, if so, the formulation could be less confusing.

m5 59-61: commas concluded by “and” would be more suitable in my opinion. Also, I do not understand how polar ice sheets have “structural uncertainties” in sea level rise impacts.

m6 62-63: the example of marine ice cliff instability is a good example but is given as an impact in this sentence, not as an uncertainty - this number should be compared to the one without MICI, clearly stating the modelling of the MICI is a still ongoing challenge.

m7 89: if it applies, I would write “important biases” to make this statement less trivial (as all models have biases).

m8 128: note that every linear function is monotonic. In any case, this formulation seems incorrect to me, as there is no response function used here – the internal dynamics of these elements is represented linearly, while forcing is additive.

m9 138: do you mean the uncertainty on the tipping behaviour?

m10 141-144: I believe that this part should be in the discussion, and could be less defensive – these temperatures have been used for the given reasons, but further temperature scenarios would be interesting to investigate.

m11 149: do you mean the uncertainty on the number of tipping elements?

m12 149-163: can we then trust the relative values? If yes, this part would really gain from stating it clearly.

m13 Fig. 1: I had a previous comment on the color used in Fig 1 and 2, which might have been misunderstood. The color scale used is very much adequate, but the representation of the timescale and temperature threshold icon in the legend is represented in red. By consistency with the other items in the legend, these should be black.

m14 191: do you mean “below or at 1.5”?

m15 193-195: this sentence involves to much information which do not bind together. In particular, the structure “It is unclear whether … as well as the probability of tipping …” does not make sense.

m16 258: “impacts of knock-on impacts” simplifies to “knock-on impacts”.

m17 297: on -> of.

m18 306: impact on the locations of tipping does not really work, do you mean impact on which element tips or not?

m19 316 and 321: “GIS tipping is higher“ or “decrease in the tipping of the GIS” is a wrong formulation. Should be something

like “GIS tips more often” and “decrease in the number of GIS tipping events”.

m20 401: if I understand, not all timescales were taken from Armstrong McKay & al, only those for GIS, WAIS, AMOC and AMAZ. Also, which “behaviours” did you take from this reference?

m21 405-407: partly because of what was stated before, this sentence is not correct. Yes, the model emulates key behaviours, but is not sufficient to explore the space of potential transitions and interactions (which are taken linear).

m22 410: this sentence is unnecessary in my oppinion.

m23 413: I would argue that “possibly” is not necessary here.

m24 420-449: I would be much more careful with these conclusions and frame them as key messages rather than key suggestions, especially for the first one.
The first message would be that, depending on the problem, the quality of models can become secondary when missing components have such a great impact. It is shown by this study and is a good message, but I do not agree that inclusion of more components is clearly a priority. For example, when considering the AMOC, better ocean and atmosphere models may result in a much closer tipping point of the AMOC. Is such a change more or less important than, for example, an interactive West Antarctica Ice Sheet?

m25 658: should the “x” in “Nxd” not be a multiplication symbol?

m26 table T1: in relation with M1, the caption clearly says “… for the tipping elements in this study”.

m27 table T2: as for T1, I believe values are taken from or informed by these references. Kriegler & al gives some number for ENSO interactions but to the best of my knowledge, none of those references quantify ASSI interactions.

m28 table T3: again related to M1– tipping risk should not relate to a climate component which is not a tipping element.

Reviewer #2 (Remarks to the Author):

This is a much improved paper compared to the one originally submitted, reflecting comments made by all three reviewers. I didn’t comment too much about specifics in the earlier reviews because I thought the larger issues needed attention, so apologies for what might seem like trivial comments but these small things matter in the long run.

Kudos for the work done in revision. If the bulk of these comments were adressed, I would be happy to recommend its publication.

In the manuscript could you be clear about what you consider a component and what is an element. In the abstract, the distinction is quite clear, but in the paper, this is less clear, partly because the study assesses six components, four of which are elements. It would be best not to use these terms interchangeably. Their usage throughout the text seems to have no underlying rationale (the supplementary material is quite precise).

Could you make the usage consistent, establishing that usage up front?

Introduction

Some general comments about the introduction, which may help the reader if you choose to tighten up the structure a little. As you have written it out, there are two main areas of uncertainty with respect to modelling. One is how the individual components are represented in models, stand alone and ESM. The other is within the ESMs themselves, which represents the interactions between components.

The main reason as to why the estimates of tipping point temperature have become lower is because of better dedicated models. There is a delay in representing these improved understandings in ESMs. The other is that the interactions within the broader climate network can only be represented thermodynamically within ESMs. How these interactions may play out within the network is the largest uncertainty at play here – not the capacity of the models to simulate individual variables into the future. It is likely that specific aspects of each tipping point will come into play here.

When discussing uncertainty, the temptation is to use uncertain language, but the opposite is needed (this advice goes back to some of the foundational work in this area – Morgan and Henrion, Schneider and Moss). There is also a temptation to use emotive language about uncertainty to raise concern about future risks. Try and be as precise and as clinical as possible. This is why I have made what seems like a series of trivial edits in the introduction.

Specific comments

Lines 23–30: Could you be even more explicit about the component/element issue here to help the reader?

Line 23: key components become critical organs on Line 25, then key Earth system components on line 27. Suggest

removing the critical organs and staying with components (ENSO and ASSI also not organs).

Lines 30–32 Suggest While combinations of different drivers (rainfall, wind patterns, local temperatures) are decisive for the state of the key Earth system components, research has been able to trace many drivers back to the global mean surface temperature.

This is not a perfect suggestion either, because to my knowledge, wind patterns do not scale in this way – there are often multiple alternatives that are not linearly scalable.

Lines 32–33: of them – of components (otherwise ambiguous)

Lines 34–36: suggest new sentence and reword to 2.2–3.4°C by 2100 under current policies and actions.

Lines 38–50: Are ASSI and ENSO included in this sequence? The term climate element seems uncertain on

line 40. Is it a tipping element or climate component?

Line 51: existence? Is this really an existential point or do you mean fundamental nature or intrinsic nature? It seems to be odd to be assessing the risk of something that may not be real. Especially when you know all these things have changed in the past.

Line 54: this means that – not needed

Line 56: dangerously – word choice – Concerningly?

Line 57: greatly – repeatedly readjusted and reassessed?

Line 64: many of these – most?

Line 72: can – redundant

Line 74: recent progress means that – redundant

Lines 76–78: known to still be missing key elements of the dynamics – what does this mean (an element of the dynamic?) Are elements missing or aspects, or are they insufficiently expressed?

Also, the Abramowitz et al. review is pre-CMIP6, more about CMIP5 and they are on about model dependency and the current suite not representing the full range of uncertainty. You might need to tighten this up.

Lines 149–163: Is this really overcounting? Isn't it a network multiplier effect?

Lines 164–166: where one element or interaction is removed from the system in order to analyse the system's response to this and understand the importance of the removed element for the overall behaviour of the system. Suggest: where one element or interaction is removed and the system response analysed to understand the importance of the removed component for overall system behaviour.

Lines 171–173: this first sentence should directly refer to the parameter ranges in Table S1 and not be a general statement. This could apply to whole para – it should jump straight into the Sobol analysis and explain how the parameters influence the results. Perhaps the 1.3 °C comment should be in the intro (around Line 35).

Lines 180–181: word choice, variance or variation but not variability.

Line 183: No secondly, so no firstly.

Line 201: critical – most important? (because critical is used in a different sense in the following para)

Lines 236–247: A little uneasy about the label real climate models on line 236, true climate on line 238 and the claim of full representation following this. This framing belies the caveats given earlier. Philosophically, I don't think truth claims can be attached to models, so a little judicious editing might be needed. Just refer to their respective strengths in representation. No problem with the underlying logic.

Line 251: catastrophic – word choice

Lines 413–414: possibly always

Line 531: ESD upper case

Supplementary

Line 11: which – each.

Reviewer #3 (Remarks to the Author):

Review of the revised manuscript "Polar ice sheets decisive for tipping risks and cascading effects in a conceptual model of the Earth system"

I think the authors have generally addressed my reviewer comments (reviewer 3) well, and the manuscript is much improved. I have a few minor comments, which I think should be addressed to improve clarity.

Line 237 and beyond (Leave one out analysis) I find the difference between “Node Discounted” and “Node Removed” confusing. Can you give an explicit example what that means in terms of interactions etc?

When interpreting fig 4 in particular, it would be useful to know what the likelihood of each individual component is to tip in the absence of any links. For example, WAIS always collapses for 4 degrees C warming. This is a very important assumption and hence piece of information for interpretation of these results. Could that information be added as a panel in a & b?

Since we have established that ASSI and ENSO are not tipping elements, what does fraction tipped mean for these in fig 4 and 5?

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Cryosphere tipping elements decisive for tipping risks and cascading effects in the Earth system

General comments

Many of these comments reference the whole of the tipping point literature, with this article being a case in point, but not unique.

Starting with the title, I thought this point was well known to anyone who has studied past, present and future climate in any detail. From the abstract: *Cryosphere tipping elements (Greenland and West Antarctica ice sheets) are most decisive for tipping risks and cascading effects* ***within our model***.

This is a paper about a specific model, PyCascades and how it represents tipping risks and cascades. So, as a reviewer, I might ask "How well does this model represent the climate system?" An associated question is "How well do the CMIP6 models represent the climate system?" The fact that CMIP6 models have improved but not enough to diagnose the interactions between tipping points is clearly stated in the article.

This raises the question as to when will they be able to do so? If we look at the history of development of Earth System models, through CMIP3, CMIP and CMIP6, they will not be able to represent the complex interactions between tipping points with sufficient confidence to make predictions within the time limit of a 1.5 °C overshoot. In fact, such changes are more likely to occur in the real world before they occur in the fully coupled models, just because of sheer amount of development needed.

Hence the need for models that explicitly represent these interactions. I would prefer the paper to cut straight to this point. The other introductory points have been made many times.

Another aspect of the broader literature is that it deals with two aspects of tipping points: the scientific understanding of them in general and of specific examples (e.g., the six in this paper), and the risk they pose.

PyCascades is a model designed to test specific interactions between tipping points. It is a model for testing uncertainties, and, given sufficient confidence in the science, calculating risk. It relies on scientific understanding, and that understanding is crucial for characterising and translating risk.

It should not be the role of this paper to advocate for any particular position, but it does so in a couple of ways. The abstract warns against neglecting the large cryosphere tipping elements in a really confusing way. What does "can reduce the mean number of disintegrated Earth system components by as much as 56%" actually mean? The we see the statement 'Overshooting 1.5 °C is becoming inevitable while ESMs lack dynamic ice sheets. Including them is necessary as is meeting stringent targets.

Here, you are advocating two quite different actions based on a confusing double negative. Tipping point risks are being used to advocate for stringent targets, and this advocacy has been going on for some time. The scientist's warnings literature is an example of this, and in my view, this market is saturated. Everyone who is going to get the message has got the message.

Policy makers know this. They need research that is policy relevant, not policy prescriptive.

Mitigation targets and model development are two different things and should not be in the same sentence. They run on two different timetables, which need to be taken into account when making recommendations. Cutting through this Gordion knot cannot be a topic of this paper.

My really big concern is that climate models are much less sensitive to forcing than the real world when the hydrological cycle and regime shifts are taken into account. The ensemble method of reducing everything to the global trend removes even more of this sensitivity. The 'truth' element of some distributions may be appropriate (e.g., temperature) but for others, it is not (e.g., relative humidity). There is a lot of information in so-called statistical noise that is being discarded.

The details of how observations and models could be developed to try and overcome this is beyond the scope of this paper. This comment is partially aimed at the Discussion, which advocates a particular view of the authors. Parts of it read like a marketing pitch for PyCascades and how it fits into the broader modelling ecosystem. It's a paper, not a press release.

My view is that this paper should focus on the analyses carried out, emphasising how the variance and leave one out analyses build on the previous findings of the PyCascades group (which should be summarised in a little more detail – you don't want to be seen as paper milling). PyCascades could well be useful – it is your task to illustrate how using the results in this paper and perhaps those that precede it.

While the large uncertainties inherent in the process are mentioned, more time should be spent on how those uncertainties influence the interpretation of the results. The word disintegration is used a few times, but that is not really appropriate when all of the different processes are taken into account. At best it might be part of some processes.

The language in the tipping point literature becomes a problem when translated into everyday parlance. Even the term tipping point as popularised by Gladwell has its own connotations in every day language, while the definition in the literature has become far more nuanced. Words may have multiple meanings. For example, a mathematician may refer to the collapse of a function, and a physical collapse that follows can be very drawn out, but that's not how the person on the street understands the process; more like a building or bridge collapsing in a pile of dust and smoke.

For these reasons, the communication of high-stakes, highly uncertain risks and its translation from the formal literature is actually a highly-skilled task in its own right.

The tone in this paper could follow that in the Global Tipping Points Report 2023, which some of this paper's authors worked on.

Given the large uncertainties articulated in the paper, how can a particular percentage result be relied upon (i.e., the 56% after element removal)? Some of the issues that arise from this type of analysis are: when is a tipping point initiated, how does the rate of change proceed after initiation, what (if any) is the window where overshoot and recovery is possible and what is the point of no return? In a previous paper discussing the individual elements this information is provided for the GIS, for example, but not here.

It also follows that these factors will interact beyond pairs, but across a network, which is not in the scope of PyCascades, or at least the version used here.

In my view the results as presented here are interesting and useful, but they are only a step in a process. PyCascades is a toy model when compared with the complexity of the whole system

(nothing wrong with that), and starting off with paired analyses in a small network is sensible. However, the functions, such as those for ENSO are very crude (nothing wrong with that, either).

If the paper concentrated on the results of the analysis, how they can inform, what are the key caveats, what findings are robust and those aspect that may be modified with new information, it could be considered as publishable. The discussion should be a very clear guide as to how the results can be interpreted as informed by this and related research.

Dear Reviewers,

We have now revised the manuscript in line with the comments of the three reviewers. In line with reviewers' comments we have reperformed the analysis updating the dynamics of some (tipping) elements to better represent the most recent literature. We have also revised the framing and discussion to make the key results clearer as well as emphasising the limitations of our model. All this led to the following major revisions:

1. To account for the **El Niño Southern Oscillation (ENSO) and Arctic Summer Sea Ice (ASSI)** being viewed as **unlikely tipping points or threshold-free feedbacks** in the literature, we reperformed our analysis with these elements as monotonic linear responding elements (see Eq. 2 of our Methods section). This analysis (requiring an additional ~32000 runs) is now reflected in all figures in the main body of the manuscript, with the previous work included in the Supplement for reference. This reanalysis did not change the qualitative result of our work: the uncertainties in the polar ice sheets remain critical for tipping risks and cascading effects. The main alteration in the new results section is the increased significance of the ENSO at 4° C with the linear representation leading to a response of this element to forcing without tipping. This allowed for greater variation in the ENSO and so greater influence on other elements (compare Fig. 2 and supp. Fig. 2).
2. As the reviewers rightly point out, **the conceptual model used in this analysis is clearly limited and is a notable simplification** (such as timescales or the tipping point representations) compared to both a global climate model and the true climate system. **We clearly point this out now**, starting from the title, introduction and discussion. We argue that, given the model simplifications, we still find robust results such as the importance of the polar ice sheets for tipping risks and cascading effects.
3. The focus on cryosphere tipping elements has been refocused to improve clarity and emphasis. **The Greenland Ice Sheet (GIS) and West Antarctic Ice Sheet (WAIS) have the greatest impact on tipping risks at 1.5° C**, with the GIS continuing to have large impacts at 4° C and both elements showing some of the largest impacts on the system in the Leave One Out analysis. Therefore, we **changed the title** to " *Polar ice sheets decisive for tipping risks and cascading effects in a conceptual model of the Earth system*" to showcase this importance. We also updated our figures accordingly including GIS and WAIS. The importance of other elements such as AMOC and ENSO, while lower in general, has also been noted in the discussion and raised in prominence compared to earlier revisions to ensure a balanced discussion of the key elements.

Large and critical uncertainties remain in the climate system and particularly in climate tipping points. To better understand these uncertainties and their impacts on tipping risks and cascading effects, we argue that it is key to study how these uncertainties propagate through the system of interacting (tipping) elements. As global climate models do not fully include some of these climate tipping elements yet, especially in coupled models enabling interactions between them, this study presents an initial attempt to characterise this uncertainty and propagate it through the system. Despite limitations of our conceptual model and analysis, we

believe that our findings are a valuable contribution to the characterisation of climate tipping point uncertainty as well as providing insight on the use of climate models for further analysis of climate tipping points. As work to further understand climate tipping points and their key uncertainties ramps up in this decade, we believe it is crucial to publish these results to help guide and inform these future studies.

We are confident that we are resubmitting a notably strengthened manuscript by comprehensively addressing the reviewer comments. We greatly appreciate the consideration of our revised manuscript, thank the reviewers for their reports, and have provided a point-by-point response below. We have added a track changes version to the submission where all changes are highlighted in blue.

With kind regards,

Jonathan Rosser, Ricarda Winkelmann and Nico Wunderling

Reviewer 1:

This study investigates interacting climate subsystems including tipping elements (GIS, WAIS, AMOC and AMAZ), ENZO and ASSI. Two main analysis are presented. First, Sobol analysis is used to infer which input uncertainties (on critical thresholds, link strengths and timescales) contribute most in the outcome uncertainty on the mean number of tipped elements. There, the authors find that the uncertainty on polar ice sheet related critical thresholds have most impact at the lower level of warming used, namely 1.5 deg. C above preindustrial level.

Second, a "leave one out" analysis is used to infer how important it is to represent/include those components in models when aiming to predict potential (cascading) tipping phenomena. There, it is found that discounting or removing a node or link can have major impact on the outcome. Especially, removing the GIS has the biggest impact at lower temperature, while removing the AMOC has the biggest impact at higher temperatures. It also shows the importance of including the AMOC->GIS and GIS->WAIS link.

The motivation is to give directions in where modelling efforts may be the most relevant, by suggesting which parts of the climate must be included or better constrained. As the title suggests, the authors conclude that priority should be given to cryosphere tipping elements.

We thank the reviewer for their clear summary and appreciation of the key aspects of the study. We have undertaken significant revisions to our manuscript and hope you will find they have strengthened our results and their presentation.

I kindly ask the authors to address the two following major comments (labelled M1 and M2), without which I cannot support publication in Nature Communications Earth and Environment. I also provide some more specific comments (labelled s1-48).

M1. The PyCascades model was explicitly presented as a conceptualisation of interacting tipping elements. It represents generic bistable systems under linear interaction, as well as their collapse timescale. While I fully agree with the authors that including non-tipping elements is such a study makes sense, ENZO and ASSI are not tipping element (as acknowledged by the authors in e.g. 92-93), and therefore are not reasonably captured in PyCascades. Especially, the motivation for doing so (given in section 1 of the supplementary material) is, in my opinion, insufficient. Indeed, it mainly motivates their inclusion based on the fact that they are important for other tipping elements, but does not justify the use of bistable systems with critical thresholds and hysteresis behaviour to represent them. This challenges the conclusion, as the importance of ENZO or ASSI, and of any other element represented in this study, may strongly depend on this representation.

We thank the reviewer for their thoughtful consideration of the representation of ENSO and ASSI in this study. Their representation as bistable systems is indeed weakly justified in the literature, which primarily shows them to be either unlikely tipping points or threshold-free

feedbacks. With this in mind, we have added the ability to PyCascades to represent monotonic linear elements responding to forcing. This is now described in the methods and supplement in more detail and is used to represent ENSO and ASSI (lines 126-132, 614-620, s51-53, s59-62, s88-100). The updated results (with an additional 32312 runs of the model) are now included in the main body of the text with the original figures and data with the variables as bistable systems are included for reference and comparison in the supplementary information (Supp Figs 1-4). The updated runs maintain the importance of the polar ice sheets, suggesting some resilience to the specific representation of these elements, as well as continuing to demonstrate the impact of discounting or removing a link is large across different dynamical representations. The main alteration was in the significance of the ENSO for uncertainty at 4° C with the linear representation allowing the element to respond to forcing without tipping, unlike the bifurcation case (discussed lines 380-386). This allowed for greater variation in the ENSO and so greater influence on other elements and the overall uncertainty.

M2. I find that the results are way more nuanced than the title, abstract and conclusion. In particular, cryosphere tipping elements include GIS and WAIS. While it is true that the importance of GIS is shown through most of the analysis, WAIS only appears relevant sporadically (in the Sobol analysis applied to the 1.5 deg. C warming and through its link to GIS). For the rest of the results, the AMOC is much more pushed forward as a crucial component in itself and for the links it has with other elements.

We thank the reviewer for their comment and agree that additional analyses were necessary to corroborate our results. With the linear representations of ENSO and ASSI, the results are qualitatively robust but support the focus on the polar ice sheets somewhat more strongly as the WAIS is shown to be more relevant under these conditions (e.g. Figs 1 & 4). At 1.5°C, the two most important elements in the SOBOL analysis are the GIS and WAIS though the AMOC is also important at this temperature (see Fig. 1). At 4.0°C WAIS is less important but GIS and links to it still dominate (with an increased dependence on ENSO due to its high threshold and cascading links to other variables (See Fig. 2). In the Leave One Out analysis the WAIS is consistently the most influential node due to its strong cascading links and significant amount of tipping.

In the former original manuscript, the importance of the WAIS in the Leave One Out analysis was smaller and not included as a key case study in the figures as the AMOC gave more dynamically interesting results, albeit with a lower overall impact. The significance of the WAIS in the Leave One Out analysis has been more clearly shown in the updated manuscript through its inclusion in Figure 4 (and following analysis) demonstrating its large impact on the system.

s1. I would strongly suggest sharing the code that produces the results and figures.

We agree and have uploaded code to produce the results and figures to the github address provided within the manuscript, see Code and Data availability section

(https://github.com/JonathanRosser/Cryosphere-tipping-elements-decisive-for-tipping-risks-and-cascading-effects-in-the-Earth-system)

s2. In the title, I would suggest to replace "cryosphere tipping elements" by "polar ice sheets" as this can bring confusion regarding ASSI.

We thank the reviewer for this recommendation and the new title now reads “Polar ice sheets decisive for tipping risks and cascading effects in a conceptual model of the Earth system”, also taking into account additional comments from the other reviewers.

s3. Through the text, I think that "disintegration" should be replaced by "collapse" or something similar. I would only find this term relevant in the case of ice sheets.

We thank the reviewer for pointing out this potential source of confusion and have replaced all instances of the word “disintegration” with terms more suitable to the situation, such as “collapse” or “tipping” (e.g. line 133) . This has been accompanied by changes throughout the manuscript (particularly in the introduction and discussion) to assist in the interpretation and understanding of results, to prevent additional confusion (e.g. lines 387-410).

s4. 13: This only refers to part of the results, see M2.

We agree. As in M2, the GIS and WAIS have been chosen to be emphasised as most important for tipping risk as they dominate at 1.5° C. While other elements such as ENSO and AMOC have significant impacts, the ice sheets do appear most consistently as decisive factors in the analysis and as such have been highlighted here.

s5. 4-6: It feels awkward not to cite the AMOC there as it is one of the most central tipping elements (also in this study).

We thank the reviewer for highlighting the omission of this critical component, it has now been added in to this list (lines 7-8).

s6. 8: Stability is quite a precise term in the context of dynamical systems. Maybe replace by "safety" or something similar.

We agree with the reviewer that the term stability has very specific connotations within the field and so this sentence has been rephrased to describe potential danger to human society in a less ambiguous and confusing way (lines 8-10).

s7. 10: I am not sure of what "structural uncertainties" means here.

We thank the reviewer for pointing out this potentially confusing phrasing, the word “structural” has been removed here. The intention of this phrase was to focus the reader on the structural or

model uncertainty in the analysis of the ice sheets due to our gaps in understanding of the true behaviour of ice sheets and how to best represent them in models. As there are a range of uncertainties affecting ice sheet modelling, the word “structural” could be removed without a loss of meaning (lines 10-12).

s8. 12: I would write "climate components".

We thank the reviewer for pointing out the potential confusion and lack of clarity in this sentence. This phrase has been altered to “these key components” to make it clearer that it refers to the same components mentioned earlier in the sentence (lines 12-14).

s9. 37: This should be reformulated (tipping poses risk, not Earth system components).

We agree with the reviewer that this sentence lacks clarity and poorly attributes the risk. This has been adjusted to “Although tipping points and large qualitative changes in key Earth system components pose large risks” to emphasise that it is the large qualitative changes and tipping that pose risk, not the components themselves (lines 50-52).

s10. 42: Sufficiently would be more just and less trivial.

We agree with the reviewer that “sufficiently” is a fairer representation of current progress on understanding of the non-linear nature of the climate system and have replaced “fully” with this (line 55).

s11. 46-50: There is a weird punctuation structure ":-;-;-:-." there.

We have adjusted and restructured this punctuation for clarity (lines 58-63).

s12. 54: I believe it would be nice to give more context to cascading tipping events, for which some key references are already given in the bibliography (e.g. Dekker, Klose, Wunderling).

We thank the reviewer for noting this key element needed for the introduction and an additional paragraph has been added to the discussion describing previous work on cascading tipping points (lines 38-49). Sentences have also been added later in the manuscript to describe the evolution and usage of the Pycascades model (lines 108-112).

s13. 66: "First" is not well announced (what is going to be enumerated?) and is not followed by a "Second" or something similar.

We agree with the reviewer that this structure is confusing and has been removed along with some restructuring of this section to more clearly demonstrate the range of limitations in current modelling approaches (lines 79-95).

s14. 67: I would remove "point".

We agree with the reviewer that the word “point” is unnecessary and may cause confusion and so has been removed (lines 79-82).

s15. 70: The coupling from what?

We thank the reviewer for their comment on this section and agree that it is unclear what coupling is being referred to. In this case the intention was to discuss the links being ice sheet melting and ocean circulations which are not included in many CMIP6 models. This sentence has now been rephrased to state this more clearly (lines 82-83).

s16. 72: I am unfamiliar with the "knock-on" term. I believe it would need either a clarification or reference.

We thank the reviewer for pointing out this potentially confusing term, we have replaced it with “cascading impacts” to improve clarity and readability (lines 83-86).

s17. 74: This statement would deserve a reference.

We thank the reviewer for their close reading of this section and their suggestion of adding references. We have added key references by Wunderling et al. (2022) and Zemp et al. (2014,2017) discussing the importance of biosphere tipping points and links for the climate system (lines 86-89).

s18. 75: Does "Earth system component" refer to the biosphere?

We thank the reviewer for noting the lack of clarity here, the intention of this sentence was to highlight the difference between General Circulation Models (GCMs) and Earth System Models (ESMs) with the former lacking key Earth System Components such as the biosphere. This has been rephrased to make this distinction clearer (lines 86-89).

s19. 77: Do you mean lack of meltwater forcing originating from melting ice sheets? Also, isn't "poor dynamical representation" a bit unfair?

We thank the reviewer for noting the lack of clarity on the meltwater forcing and for the somewhat unfair description of the dynamical representation. This section has been rephrased to make clear the meltwater source of the freshwater that is lacking and also that the issues with the dynamical representation are concentrated in the deep water or convective behaviour (lines 90-93) .

s20. The end of the introduction is a bit brutal. It could end on what will be done (like the first paragraph of the "Modelling approach" section).

We agree with the reviewer that this ending was sudden and have added a new paragraph describing the approach of the study and the content of the following sections (lines 97-101).

s21. 83: I would replace "have used" by "use".

We agree with the reviewer that this tense is more appropriate and have made the suggested substitution (line 103).

s22. 99: Here, I would suggest being more transparent on what the model actually does, as it is important for the framing of the study. Especially, it simplifies tipping elements into linearly interacting, generic bistable systems. This is somehow said in the Methods section but is very relevant at this point.

We agree with the reviewer that adding in additional information about how the model approaches these problems is useful for the flow of the main manuscript and for the understanding of the reader and so additional sentences have been added to clearly explain how the model functions and how it represents some elements as bistable systems and some as monotonic linear systems (lines 108-112, 124-136).

s23. 107: I would replace by "key climate components" and reserve "element" for tipping elements, for clarity.

We thank the reviewer for this suggestion and have implemented it to improve clarity (lines 137-138).

s24. 107-108: I don't really get the sentence "... above preindustrial running equilibrium experiment". Is there a "level" missing after "preindustrial"?

We thank the reviewer for pointing out this unclear sentence. The intention of the sentence was to describe the temperatures being chosen as levels of global warming above pre-industrial temperatures. This sentence has been restructured to clearly describe the temperature forcing above pre-industrial temperatures and the model running lengths (lines 138-144).

s25. The main text should say exactly what the Sobol Variance quantifies (what this percentage means), either in the text or captions of fig 1 and 2.

We thank the reviewer for pointing out this weak definition and lack of clarity. The main text and the methods section now more clearly describe the “Total-effect index” which is the specific measure used here (lines 149-163, 647-685). It is a measure of the total variation in the value analysed (in this case the number of tipped elements) caused by an specific input parameter.

s26. As I understand from fig 1-2, no timescales uncertainty significantly contributes in the Sobol variance. Does it mean that this uncertainty does not really impact the outcome? I would appreciate a comment on this.

We thank the reviewer for raising this interesting aspect of the results. The timescales have less impact on the final results than we expected. This is partly as the runs are essentially run to equilibrium, so each element has time to relax regardless of its intrinsic timescale. However, timescales play a very important role in transient simulations and simulations that are not run into equilibrium. We haven't checked this here, but some results can be found in Wunderling et al. (2023, Nature Climate Change) and Möller/Högner et al., (2024, in review). Paragraphs have been added to the analysis explaining these nuances and results (lines 196-202, 207-213).

s27. On fig 1-2, the fact that timescales and threshold temperature are represented in red is confusing, I would suggest black or grey.

We thank the reviewer for their suggestion for clarifying these figures. The colours were chosen to stand out from the greyscale background and also to emulate the common colour scheme of the "burning embers" diagrams used in the tipping points literature and IPCC reports. However, if the reviewer feels that a greyscale colormap would be preferable then we would of course be very happy to edit these figures.

s28. 152-154: To my understanding, this is not consistent with fig 2, which shows that most important are the GIS critical threshold and the AMOC->GIS link. Then the AMOC threshold, and then the rest on a similar level.

We thank the reviewer for noting this potentially confusing/ misleading focus and we have updated this description to focus on the GIS and AMOC-GIS link, as well as the increased importance of ENSO shown in the updated analysis at 4° C (lines 203-213).

s29. 179-180: While I understand the intention of the authors and do not have a better formulation, I would suggest to be careful with such sentences, as the model does not represent that. This can be confusing for an unwary reader, especially regarding comment s22.

We thank the reviewer for raising this potentially confusing or misleading sentence and we have rephrased it to "can be considered as a representation of the true climate" (lines 236-239). We hope this will make it clear to readers that the model is not a perfect representation of the climate but can be considered a representation for some features and behaviours.

s30. 209: I do not see that on fig 3. Do you mean fig 4?

This is now corrected to Figure 4 (lines 267).

s31. It feels awkward not to include WAIS in this analysis as it was pushed forward in both the title and Abstract, see M2.

We agree with the reviewer that WAIS should be shown in this analysis (as mentioned in M2 and our response) and so WAIS has now been added to Figure 4 and to the following analysis (lines 285-292). This shows that the WAIS now has the greatest impact when removed in the Leave One Out analysis, overtaking the GIS from the previous analysis. This helps to emphasise the focus on the GIS and WAIS which we see through the rest of the manuscript, with the WAIS being consistently dominant in the Leave One Out analysis while the GIS is consistently more dominant in the Sobol analysis.

s32. 221-223: "e.g." are here are confusing as the GIS and AMOC case give opposite results.

We thank the reviewer for raising this potential source of confusion, the “e.g.”s have been removed to avoid confusion as they are not critical for this part of the argument (lines 281-283).

s33. 242: Tipping of what? of GIS or in the whole system?

We thank the reviewer for noting this lack of clarity, the sentence was intended to refer to the increase in tipping of the GIS when the stabilising link from the AMOC was removed, this has been clarified in the text (lines 309-311).

s34. 243: So AMOC is not a critical component at those temperatures? This is what fig 4 insists on.

We thank the reviewer for pointing out this lack of clarity. This section intended to emphasise the importance of the AMOC for controlling the critical components which are more likely to tip, rather than to downplay its importance. The original phrasing is very unclear and so this section has been rewritten, making the importance of the AMOC clear (lines 309-321).

s35. Fig 4 and 6 only provide representative cases of removal. Why those ones? Are other cases negligible?

We thank the reviewer for their keen analysis of these figures. These examples have been chosen as they show the largest overall changes and the largest changes in the distribution of the tipping across the elements (in the case of the AMOC removal). Representative cases at the extremes and with interesting dynamics have been chosen to emphasise and complement the discussion, displaying all possible removals over the two figures would require 17 bars across all nodes and links which would be very cluttered and less helpful to show.

s36. 251: I would replace "altered" by "decreased".

We agree with the reviewer that “decreased” makes the meaning clearer and so we have made this change (lines 325-327).

s37. 278-279: There seems to be a problem with this sentence.

We agree with the reviewer that this sentence was poorly punctuated and have now restructured the section with a clearer and more functional structure (lines 355-359).

s38. 289-291: "Missing key dynamics can have significant impact" sounds very trivial as a conclusive statement.

We agree with the reviewer that the end of this section needs a clearer and more conclusive end. We have replaced this sentence with a focus on the fact that missing key dynamics can change our future projections and assessments of potential tipping risks and cascading effects (lines 365-367). As mentioned in reviewer 2's comments, we are keen to avoid putting too much emphasis on specific numerical values as these will not trivially scale to larger models while results about the non-linear impact of removing components are much more likely to scale generally.

s39. 302-303: Again this is given as the conclusion of the results, which are more nuanced. Sobol analysis indeed shows that for low warming, but leave one out only gives the GIS as an important element in low warming, and AMOC as important element for higher warming. I also see that 247-252 motivate the importance of WAIS but only indirectly, as the reader must refer to the supplementary table 3 to see that this limit percentage is in fact given by the WAIS removal case.

We thank the reviewer for pointing out the lack of focus on the WAIS in this section and displayed in the figures as well as the AMOC for higher warming. The updated results and addition of the WAIS to Fig 4 should highlight the importance of the WAIS more effectively. This is now discussed in more detail in the text as well. The AMOC has retained its importance in the analysis and is still discussed at this level, although it is consistently less important than the ice sheets and so is not the key focus pulled out here.

s40. What about the 4 deg. C warming experiment in the conclusion?

We agree with the reviewer that discussing the 4° C changes and comparison to the 1.5° C results are critical for the conclusions and so a section has been added to the conclusion to better represent the nuance in the results and make the links between the results and the conclusions clearer (lines 380-386).

s41. 307-308: It would be more just to invert this sentence into "Although the network model used in this study is a strongly simplified non process-based model, it has many common features ..." or something similar.

We agree with the reviewer that this sentence could be restructured for clarity and it has now been inverted (lines 387-389).

s42. 326-331: This is a quite strong statement made here. Would one prefer quantity over quality when aiming at prediction of the climate? Is many poor representations better than some

more accurate ones? I do not have an answer myself, but I do not believe that the results give an answer to this question.

We agree with the reviewer that this is quite a strong statement, it has been rephrased slightly and some additional justification has been added (lines 420-424). The aim of this section is not to suggest that many poor representations are better than some more accurate ones, but more that very accurate representations of a single climate element (or subset of climate elements) can be very inaccurate if a key element is not represented, and so climate models will likely benefit by focusing on including as many key elements and dynamics as possible, rather than focusing on improving the elements already included.

s43. 339: I would replace "if we believe" by "assuming" (as no one really believes that).

We thank the reviewer for pointing out this nuance and suggesting a strengthening of this argument. This entire paragraph has been rewritten to better explain the key takeaways and the phrase “if we believe” has been removed to avoid making overly strong claims (lines 434-449).

s44. 492-493: There is an inconsistency between the ref here and the ones of the table.
We thank the reviewer for their correction here. In terms of the references provided for the interaction parameters, the paper referenced in the text is included in the list with the supplementary table, but the supplementary references are given a different numbering system to those in the main text. Some references have been added to the main text for clarity (lines 641-642). If an issue persists we are happy to correct it, please do specify the particular section and we will readily fix the error.

s45. 54(sup): Does the climate tipping point report provide number? what is the "(supplementary information)"? To my understanding those numbers have been chosen by the authors informed by the report, not "taken from".

We thank the reviewer for pointing out this lack of clarity and justification for these numbers.The climate tipping point interactions and cascades review informed the numbers chosen for ENSO and other variables as similar reasoning had informed previous variables for this. The use of this report to inform the choice of numbers is now better described in the supplementary information (lines s65-s84).

s46. (sup) How was ENZO link chosen, as it did not appear in previous iterations of PyCascades-related publications? The supplementary section 2 should be clearer about what comes exactly from where (each critical thresholds, timescales and link strengths). As of now, it is quite convoluted.

We agree with the reviewer that there is insufficient explanation for the choice of the ENSO link. ENSO has been chosen based on the climate tipping point interactions and cascades review (Wunderling et al. (2024)) as it is a key interacting element in the system, with theorised tipping point behaviour and indications of state change under global warming. Even if it does not

demonstrate true tipping point behaviour, it has strong influences on a number of other elements in the model and representing the potential variation in state as a linear variation allows this forcing and relation to be considered. The reasons for adding ENSO and its description as linear response variable are described in more detail in the supplementary information (lines s54-s62)

s47. The caption of sup table 1 has a problem in the last reference (1 is Kriegler).

We agree with the reviewer that there is a problem with the reference. The Kriegler and Wunderling citations have been split for clarity (sup table 1).

s48. In general, citations in the supplementary material mix text and number citations.

We thank the reviewer for noting this inconsistency. The citations have primarily been added as numbers but occasionally as text citations additionally to this to add clarity to some of the sentences where the detail of the reference is important for the argument. If this is unclear or breaks the clarity of the text we would be happy to fix specific instances where issues occur.

Reviewer 2:

General comments
Many of these comments reference the whole of the tipping point literature, with this article being a case in point, but not unique.

Starting with the title, I thought this point was well known to anyone who has studied past, present and future climate in any detail. From the abstract: Cryosphere tipping elements (Greenland and West Antarctica ice sheets) are most decisive for tipping risks and cascading effects within our model.

We thank the reviewer for their feedback on the title. We have revised the title, also in line with later comments about the model and extrapolation. The new title is “Polar ice sheets decisive for tipping risks and cascading effects in a conceptual model of the Earth system”. Further, while similar results have been shown in different studies, the importance of the ice sheets for overall uncertainty in tipping cascades using a leave one out analysis and a SOBOL analysis has not been performed before. We hope that this study will be a useful addition to the literature, strengthening our understanding of these critical elements.

This is a paper about a specific model, PyCascades and how it represents tipping risks and cascades. So, as a reviewer, I might ask “How well does this model represent the climate system?” An associated question is “How well do the CMIP6 models represent the climate system?” The fact that CMIP6 models have improved but not enough to diagnose the interactions between tipping points is clearly stated in the article.

This raises the question as to when will they be able to do so? If we look at the history of development of Earth System models, through CMIP3, CMIP and CMIP6, they will not be able to represent the complex interactions between tipping points with sufficient confidence to make predictions within the time limit of a 1.5 °C overshoot. In fact, such changes are more likely to occur in the real world before they occur in the fully coupled models, just because of sheer amount of development needed.

Hence the need for models that explicitly represent these interactions. I would prefer the paper to cut straight to this point. The other introductory points have been made many times.

We thank the reviewer for these comments and feedback regarding the introduction. We agree that the possibility that CMIP6 models may struggle to fully represent all these key tipping points in the near future is very important, and have emphasised this in the introduction (lines 68-95). Regarding the other introductory points around CMIP6 and the PyCascades model/tipping points literature, we shortened the introduction where possible but kept the introduction at a length that allows the reader to understand key previous works and the research gap that this paper works out. We will, of course, be very happy to make any additional changes upon reviewer feedback.

Another aspect of the broader literature is that it deals with two aspects of tipping points: the scientific understanding of them in general and of specific examples (e.g., the six in this paper), and the risk they pose.

PyCascades is a model designed to test specific interactions between tipping points. It is a model for testing uncertainties, and, given sufficient confidence in the science, calculating risk. It relies on scientific understanding, and that understanding is crucial for characterising and translating risk.

We thank the reviewer for their summary of PyCascades, which is indeed the goal of the model.

It should not be the role of this paper to advocate for any particular position, but it does so in a couple of ways. The abstract warns against neglecting the large cryosphere tipping elements in a really confusing way. What does “can reduce the mean number of disintegrated Earth system components by as much as 56%” actually mean? The we see the statement ‘Overshooting 1.5 °C is becoming inevitable while ESMs lack dynamic ice sheets. Including them is necessary as is meeting stringent targets.

Here, you are advocating two quite different actions based on a confusing double negative. Tipping point risks are being used to advocate for stringent targets, and this advocacy has been going on for some time. The scientist’s warnings literature is an example of this, and in my view, this market is saturated. Everyone who is going to get the message has got the message.

Policy makers know this. They need research that is policy relevant, not policy prescriptive.

Mitigation targets and model development are two different things and should not be in the same sentence. They run on two different timetables, which need to be taken into account when making recommendations. Cutting through this Gordion knot cannot be a topic of this paper.

We agree with the reviewer that the original manuscript contained too much advocacy and policy prescriptive elements and that the paper should focus on the key science and the next steps motivated by this. We also agree that the focus on specific numbers in the abstract is unnecessary and puts too much emphasis/ confidence in numbers that are inherently highly uncertain. Finally, we agree that some statements are poorly phrased and unclear. With these points in mind, the abstract and parts of the discussion have been altered and rephrased to be clearer and avoid too much policy commentary (lines 16-17, 387-449) .

My really big concern is that climate models are much less sensitive to forcing than the real world when the hydrological cycle and regime shifts are taken into account. The ensemble method of reducing everything to the global trend removes even more of this sensitivity. The ‘truth’ element of some distributions may be appropriate (e.g., temperature) but for others, it is not (e.g., relative humidity). There is a lot of information in so-called statistical noise that is being discarded.

We agree with the reviewer that climate models may exhibit less sensitivity to forcing than the real world and that ensemble methods may simplify this uncertainty more than would be ideal. The temperature is not always the key real forcing variable for tipping elements (which might be precipitation changes for the AMOC and freshwater input for the AMOC). While we cannot take this into account in the current version of PyCascades this would be a great area for future work.

The details of how observations and models could be developed to try and overcome this is beyond the scope of this paper. This comment is partially aimed at the Discussion, which advocates a particular view of the authors. Parts of it read like a marketing pitch for PyCascades and how it fits into the broader modelling ecosystem. It's a paper, not a press release.

We agree with the reviewer that the issue of model development and observational improvements are beyond the scope of this paper. We have rephrased the discussion to avoid advocating for particular views and next steps, and have tried to present a broad view of the results, how they fit into the literature, and potential next steps within the field without being too specific or policy prescriptive (lines 387-449). We are happy to make any further changes which might be deemed necessary.

My view is that this paper should focus on the analyses carried out, emphasising how the variance and leave one out analyses build on the previous findings of the PyCascades group (which should be summarised in a little more detail – you don't want to be seen as paper milling). PyCascades could well be useful – it is your task to illustrate how using the results in this paper and perhaps those that precede it.

As mentioned in our response to Reviewer 1's comment s12, we agree that the paper should more clearly state the recent progress in the field and usage of the pycascades model. We have added information to the introduction and methods to make clear exactly what the pycascades and variance analysis methods can do, the questions they answer, and how they build on what has been done before (lines 107-167). If there are additional elements which would be worth adding we are happy to do so.

While the large uncertainties inherent in the process are mentioned, more time should be spent on how those uncertainties influence the interpretation of the results.

We thank the reviewer for this feedback and agree that more nuance is needed in our discussion. We have added an additional section to the discussion reviewing the key uncertainties in the method and how this will affect the interpretation of results (lines 387-410).

The word disintegration is used a few times, but that is not really appropriate when all of the different processes are taken into account. At best it might be part of some processes.

The language in the tipping point literature becomes a problem when translated into everyday parlance. Even the term tipping point as popularised by Gladwell has its own connotations in every day language, while the definition in the literature has become far more nuanced. Words may have multiple meanings. For example, a mathematician may refer to the collapse of a function, and a physical collapse that follows can be very drawn out, but that's not how the person on the street understands the process; more like a building or bridge collapsing in a pile of dust and smoke.

For these reasons, the communication of high-stakes, highly uncertain risks and its translation from the formal literature is actually a highly-skilled task in its own right.

The tone in this paper could follow that in the Global Tipping Points Report 2023, which some of this paper's authors worked on.

We agree with the reviewer that the tone is incredibly important in this manuscript, as are the specific word choices. In response to these comments and those of other reviewers (such as Reviewer 1 s3) we have rephrased some sections to avoid misleading the reader or implying disintegration where it is not appropriate (lines 16-17, 27-30, 52-54, 301-304) . We have replaced the word "disintegration" with more appropriate wording such as "tipping" or "collapse" throughout the manuscript.

Given the large uncertainties articulated in the paper, how can a particular percentage result be relied upon (i.e., the 56% after element removal)? Some of the issues that arise from this type of analysis are: when is a tipping point initiated, how does the rate of change proceed after initiation, what (if any) is the window where overshoot and recovery is possible and what is the point of no return? In a previous paper discussing the individual elements this information is provided for the GIS, for example, but not here.

We agree with the reviewer that specific percentage results are highly uncertain in this paper and so have de-emphasised them where possible and used them in an indicative way rather than valuing the numbers for their value specifically (lines 16-17). We agree that there is ongoing uncertainty about how tipping points are initiated, how the rate of change behaves, and responses to overshoot. Unfortunately, these are beyond the scope of the paper and are either not currently implemented in the PyCascades model (as a bifurcation system is the main response function, varied rates of change to tipping are limited to just changing the single timescale rather than changing the tipping in a structural way) or have already been addressed to some extent in other papers (such as the overshoot response in Wunderling et al. (2023)). For example, in this paper it is shown that for the Greenland ice sheet the risk of tipping is shown to be relatively insensitive to overshoot when returning to a stabilisation temperature of 2.0°C. We hope that further work may be possible to address some of these issues and concerns within the tipping point field, but they are unfortunately beyond the scope of this analysis.

It also follows that these factors will interact beyond pairs, but across a network, which is not in the scope of PyCascades, or at least the version used here.

We agree with the reviewer that non-pairwise or non-linear interactions may be critical for the behaviour of the system, but are indeed not the focus of PyCascades yet (but could be if future literature proves interactions to be nonlinear or beyond pairwise; until then we believe that using the simplest assumption of linear interactions is useful) and also remain highly uncertain for inclusion in the model. These limitations are now discussed in more detail in the discussion (lines 401-407).

In my view the results as presented here are interesting and useful, but they are only a step in a process. PyCascades is a toy model when compared with the complexity of the whole system (nothing wrong with that), and starting off with paired analyses in a small network is sensible. However, the functions, such as those for ENSO are very crude (nothing wrong with that, either).

If the paper concentrated on the results of the analysis, how they can inform, what are the key caveats, what findings are robust and those aspect that may be modified with new information, it could be considered as publishable. The discussion should be a very clear guide as to how the results can be interpreted as informed by this and related research.

We thank the reviewer for their positive feedback on the value of the results and for their clear analysis of the benefits and limitations of the model. We are now better reflecting on the capacity and limitations of the model and the relevant functions within the introduction and discussion (lines 107-136, 384-403). We also better discuss the limitations and uncertainties relevant to interpretation in the discussion (lines 387-410). Alongside this we note the potential improvements to this work and further steps that would be useful. In doing the updated analysis (an additional 32312 runs of the model) and using linear functions for the ENSO and ASSI we have also been able to do some analysis on the robustness of some results, the previous results are included in the supplement for comparison (Supp Figs 1-4), but the primary results of the significance of the ice sheets and the sensitivity to removing elements or links remain robust to these changes.

Reviewer 3:

Summary:
This paper by Rosser et al investigates the interaction of tipping elements in the climate system. The basic idea is to represent interacting tipping elements as Cusp or Hopf bifurcations where an interaction term is added to the right hand side of the normal form. Depending on the strength of the interaction term, bifurcation behaviour is either strongly or weakly coupled. The modelling itself is relatively simplistic, but allows for sampling of a large subset of parameters, and statistical analysis of the results. The model itself has been described in the published literature together with aspects of the components, but the detailed analysis of the model behaviour applied to the climate dynamics is novel. I thought the paper was generally interesting to read, but the figures and analysis are not as clear as they could be.

We thank the reviewer for their positive summary of the results in the paper and greatly appreciate the feedback regarding figures and analysis. As described in responses to more specific comments in this review and from other reviewers, the figures have been adjusted and improved for better clarity and to show their key messages (Figs 1-6). The analysis has also been adjusted to show a clearer flow through the manuscript and with a more nuanced discussion with a clear understanding of the limitations of this work.

The main take-away message from this paper is that coupled simulations of the cryosphere with other climate systems are necessary for capturing the interactions between different components correctly. I think this point is nicely illustrated by this study, and why this point is not revolutionary in itself, I believe highlighting it clearly might provide extra justification for renewed efforts to develop fully-coupled models. (Of course, these efforts are already on-going, and mostly hampered by technical difficulties, rather than lack of motivation).

We thank the reviewer for their clear summary of the main results of the manuscript and for assessing that it has been shown nicely in this study. We agree that the study aims at providing some extra justification and support to ongoing efforts in these areas.

Main points:
1. The most important question is how valid the representation of interactive climate components as relatively simply connected bifurcation systems is. While I think this model is very useful to provide insights about possible interactions of the climate system, I would like to encourage the authors to make sure they better discuss and acknowledge limitations. For

example, cryospheric systems can react on several different timescales to changes in climate, not only one, with each of these timescales representative for different regions and physical processes (e.g., MISI, MICI, melt-elevation feedback etc.). In the model, there is only one timescale. Moreover, the interaction terms are linear, but nonlinearity might turn out to be essential in determining whether tipping of one element can cause the tipping of another.

We thank the reviewer for noting this potential improvement to the manuscript. While answering the question of exactly how representative this system is of the real climate system is beyond the scope of this paper (and difficult to answer with current observational data of systems which have not exceeded their tipping points in recorded memory), we have made extensive changes to the manuscript to better describe the limitations (lines 387-410). The ability of this system to represent the real climate system is also discussed in Wunderling et al. (2021) where key climate systems such as the GIS, WAIS, AMOC, and AMAZ are analysed in comparison to the fold-like tipping points used in this model. Significant new sections have been added to the discussion to emphasise the limitations of the model used here and the factors to consider when extrapolating these results to global climate models and the true climate system (lines 387-410). Particular attention is paid to the limited representation of timescales as only a single value and of interactions only represented as pairwise linear interactions (lines 401-409). Alongside this we note the potential improvements to this work and further steps that would be useful. In doing the updated analysis (an additional ~32000 runs of the model) and using linear functions for the ENSO and ASSI we have also been able to do some analysis on the robustness of some results, the previous results are included in the supplement for comparison (Supp Figs 1-4), but the primary results of the significance of the ice sheets and the sensitivity to removing elements or links remain robust to these changes.

2. Representation of Arctic Summer Sea Ice (ASSI) and ENSO as tipping elements: As the authors acknowledge, neither can be considered a tipping element. While they clearly have links to the other elements, they should fundamentally be represented by a different kind of equation. Unless my math is flawed, using the parameters provided, Arctic Summer Sea Ice exhibits a hysteresis behaviour, which it shouldn't. Given the relatively simple form of the ODE's underlying the model, I suggest implementing a monotonically increasing component that can represent ASSI and ENSO more adequately. The ASSI tipping point is also frequently referenced throughout the manuscript (e.g., line 138, line 48 of the supplement), after it is established that it is not in fact a tipping element. Please correct.

We thank the reviewer for their keen analysis of the model dynamics and representation of climate system components. Based on this and other reviewer's comments, we agree that ENSO and ASSI would be more adequately represented by a monotonically increasing component. We have added this to the PyCascades method and resimulated all results including a linear ENSO and ASSI. We now describe this in the methods and supplement in more detail (lines 126-132, 615-621, s44-62, s88-100). The updated results (with an additional 32312 runs of the model) are now included in the main body of the text with the original figures and data with the variables as bistable systems is included for reference in the supplementary

information. The updated runs maintain the importance of the polar ice sheets, suggesting some resilience to the specific representation of these elements, as well as continuing to demonstrate the impact of discounting or removing a link is large across different dynamical representations. The main alteration was in the significance of the ENSO for uncertainty at 4° C with the linear representation allowing the element to respond to forcing without tipping, unlike the bifurcation case. This allowed for greater variation in the ENSO and so greater influence on other elements and the overall uncertainty.

Other points:
Figures 1 and 2 are not every informative, in particular the difference between the two is only small (in contrast to what is claimed in lines 150-151). I suggest using a logarithmic colorscale for the Sobol variance importance or some other way to emphasize the difference between them. Also scaling the arrow width with the mean strength of the link between two components would be helpful (or potentially providing this information in a separate figure, rather than only the table in the supplement). The strength of the interaction is, after all, key for understanding the results.

We thank the reviewer for their analysis of the Figures and advice on their potential improvement. Altering Figures 1 and 2 to show a logarithmic colour scale was implemented, but this meant that much more focus was drawn to order of magnitude changes in very small links (such as between 0.01% and 0.1% of significance) but reducing the clarity of the dominance of 2-3 elements on the majority of variance (which was the focus of the study). Scaling the arrow width with the mean strength of the link was informative and helpful for clarity but unfortunately made the figure much more difficult to read due to the large variation in link strengths and so this has been kept in a Supplementary Table 2.

Analysis of leave one out removal studies: I think the authors should have gone more in depth here. In particular, I suggest in figure 4 to not stack the number of different tipping points, but to plot the different components (AMOC, GIS, etc) clustered side-by-side (i.e., individual bars for each component), with the y-axis showing the percentage of simulations which show tipping for each component and scenario. This would help illustrate and identify the strength of the links for the different components better. Same for Figure 6.

We thank the reviewer for their suggestions for this analysis and the figures. We have changed Figures 4 & 6 to no longer stack the tipping points which allows them to more clearly show the changes in individual components, while keeping the overall changes in the plots below. We agree that this has improved the clarity of the figures and their use in demonstrating the key results of this section, particularly the changes in distribution of tipping due to changes in the system.

Dear Editors, dear Reviewers,

We have now revised the manuscript in line with your requests and the comments of the three reviewers. In line with these we have added an initial section clarifying the role of the ENSO and ASSI as not being tipping elements (or at best uncertain) but displaying known qualitative transitions with strong links to other tipping elements considered in the study. We have also revised areas of the text to avoid misleading the reader around tipping points and to clarify key terms and messages.

We are confident that we are resubmitting a notably strengthened manuscript by comprehensively addressing the reviewer comments. We greatly appreciate the consideration of our revised manuscript, thank the reviewers for their reports, and have provided a point-by-point response below. We have added a track changes version to the submission where all changes are highlighted in blue.

With kind regards,

Jonathan Rosser, Ricarda Winkelmann and Nico Wunderling

Reviewer 1:

I appreciate the effort of the authors for answering my previous comments, and especially for adapting the experiments for a better representation of ENSO and ASSI.

I have still one important, general comment labeled M1, and some more specific comments labelled m1-28.

M1: Even though the model has been adapted in a way that ENSO and ASSI are not represented as tipping elements, the terms “tipping”, “tipping threshold”, “tipping point”, and “tipping timescale” or “critical temperature” are still used for them or in sentences that include them. This is incorrect and brings confusion in many places of the manuscript, including text, figure title/captions and table content/captions.

We thank the reviewer for this recommendation and for pointing out this lack of clarity in the manuscript. To clarify the behaviour of these elements, they have been described in more detail as elements with strong qualitative transitions under climate forcing with strong links to the other elements. Although the ENSO and ASSI are not classified as clear tipping elements, they both undergo large qualitative changes in behaviour under climate forcing, transitioning to new states and producing impacts on other elements in this study. Due to this they have been included in the study and , to maintain clarity and coherence, will be described similarly to the other climate components with ``tipping thresholds'', ``tipping timescales'', and calculating the ``fraction tipped''. The meaning and usage of these terms is clarified at the start of the text. In these cases, if the components such as ASSI and ENSO are not considered as tipping elements then these values can be taken to refer to the transition of the component from its current state to a qualitatively altered one under climate forcing. This additional text is added lines 125-139.

We have also clarified the usage of “element” and “component” throughout the text and their relationship to “tipping”, with the term element referring to the four tipping elements and the term component referring to the six climate components of the system.

m1 38-49: “tipping point interaction” or “tipping point cascades” is vague as tipping points do not really interact or cascade. I see that this terminology is used in the title of Wunderling & al 2024, but I would find “tipping element interaction” and “tipping cascades” more suitable.

We thank the reviewer for raising this issue and potential tightening of the terminology. In this section the phrases “tipping point interaction” and “tipping point cascades” have been replaced with “tipping element interaction” and “tipping cascades”, lines 41-55.

m2 40: I would find "…may occur…" more suitable. In this same line, I guess that "This" refers to the interaction? If yes, "Such interactions" or something similar would be clearer as, for the moment, it seems to refer to "cascading tipping".

We thank the reviewer for these recommendations and have edited line 42 as suggested to improve the clarity.

Finally, there are quite some references motivating that interaction between tipping elements can decrease the likelihood of cascading tipping, which can be a counterintuitive idea for some readers but is crucial for this study. For example, Ciemer & al 2021, Sinet & al. 2023 or Nian & al. 2023 may be relevant here.

We thank the reviewer for providing these references and suggesting the addition of further references to support this claim. These and other key references have been added to line 42.

m3 45: a point is missing before "These". This sentence is again a bit unclear as "These interactions" refers to both stabilizing and destabilizing interactions. Maybe "In a worst-case scenario, these interactions …"?

We thank the reviewer for noticing the punctuation error and clarity. The missing full stop has been added and the start of the sentence has been amended to "in a worst case scenario, the interactions" (lines 50-51).

m4 51-52: was "Large uncertainties about their existence, interactions, and key uncertainties" intended? Uncertainty about uncertainty is possible but, if so, the formulation could be less confusing.

We thank the reviewer for noting this confusing formulation; it has now been changed to "existence, interactions, and behaviour" to more clearly illustrate the key uncertainties (lines 56-58).

m5 59-61: commas concluded by "and" would be more suitable in my opinion. Also, I do not understand how polar ice sheets have "structural uncertainties" in sea level rise impacts.

We thank the reviewer for their clarification to the grammar and we have changed the structure to commas concluded by "and". To clarify the last section of this list we have edited the phrasing to "very large structural uncertainty in their contribution to sea level rise" (Lines 63-68)

m6 62-63: the example of marine ice cliff instability is a good example but is given as an impact in this sentence, not as an uncertainty - this number should be compared to the one without MICI, clearly stating the modelling of the MICI is a still ongoing challenge.

We agree with the reviewer that the role of the marine ice cliff instability in the uncertainty is unclear in this sentence. It has been emphasised, with comparison to the values without MICI, with an additional line to state that MICI is currently the subject of ongoing research. (lines 68-71)

m7 89: if it applies, I would write “important biases” to make this statement less trivial (as all models have biases).

We agree with the reviewer that the biases should be emphasised here and have altered the phrasing to “Important biases” (line 94-97).

m8 128: note that every linear function is monotonic. In any case, this formulation seems incorrect to me, as there is no response function used here – the internal dynamics of these elements is represented linearly, while forcing is additive.

We thank the reviewer for noting the tautology in the use of “monotonic” here and have therefore removed it. The linear response to forcing has been rephrased in this sentence for clarity (line 148-149)

m9 138: do you mean the uncertainty on the tipping behaviour?

We thank the reviewer for noting this lack of clarity. This sentence refers to analysis of the final tipping state of the components, as a proxy for the tipping behaviour and this has been clarified in the text. (line 157-158)

m10 141-144: I believe that this part should be in the discussion, and could be less defensive – these temperatures have been used for the given reasons, but further temperature scenarios would be interesting to investigate.

We agree with the reviewer that this comment on the modelling choice and future investigations would be better placed and have moved this to the discussion along with additional discussion of further analysis of the temperature range (lines 411-415).

m11 149: do you mean the uncertainty on the number of tipping elements?

We thank the reviewer for noting this lack of clarity, in this case we do mean the number of elements which tip, or the tipping behaviour, rather than the number of tipping elements. This sentence has been rephrased to “the tipping behaviour, measured by the number of tipped elements” to improve clarity (line 166-168)

m12 149-163: can we then trust the relative values? If yes, this part would really gain from stating it clearly.

We agree with the reviewer that the utility of the relative values needs stating and confirming here and so a line has been added to this effect. (lines 183-185)

m13 Fig. 1: I had a previous comment on the color used in Fig 1 and 2, which might have been misunderstood. The color scale used is very much adequate, but the representation of the timescale and temperature threshold icon in the legend is represented in red. By consistency with the other items in the legend, these should be black.

We agree with the reviewer and apologise for our previous misunderstanding. We have changed the figure to give all of the legend features an intermediate orange colour as changing all of them to black would have obscured the timescale icon.

m14 191: do you mean “below or at 1.5”?

We thank the reviewer for noting this unclear wording. In this sentence, we are referring to the fact that temperature thresholds dominate the uncertainty for global warming levels of 1.5°C and so have altered the phrasing to “importance of temperature thresholds for uncertainty at global warming levels of 1.5°C”. (lines 208-211)

m15 193-195: this sentence involves to much information which do not bind together. In particular, the structure “It is unclear whether … as well as the probability of tipping …” does not make sense.

We agree with the reviewer that this structure is very unclear and so have split the sentence into two to improve clarity. (lines 211-215)

m16 258: “impacts of knock-on impacts” simplifies to “knock-on impacts”.

We thank the reviewer for noting this clarification, we have replaced “impacts of knock-on impacts” with “knock-on impacts”. (lines 263-266)

m17 297: on -> of.

We thank the reviewer for noting this clarification and have replaced “on” with “of”. (line 319)

m18 306: impact on the locations of tipping does not really work, do you mean impact on which element tips or not?

We agree with the reviewer that this wording is unclear and have replaced “locations of tipping” with “the behaviour of different elements and which elements tip”. (lines 326-328)

m19 316 and 321: “GIS tipping is higher“ or “decrease in the tipping of the GIS” is a wrong formulation. Should be something like “GIS tips more often” and “decrease in the number of GIS tipping events”.

We agree with the reviewer that the phrasing could be improved and have changed it to “GIS tips more frequently” and “decrease in the number of GIS tipping events”. (lines 339 & 344)

m20 401: if I understand, not all timescales were taken from Armstrong McKay & al, only those for GIS, WAIS, AMOC and AMAZ. Also, which “behaviours” did you take from this reference?

We thank the reviewer for noting this misleading statement and have updated this to correctly include all relevant references. We have also replaced the word “behaviours” with “thresholds” to be more accurate. (lines 429-431)

m21 405-407: partly because of what was stated before, this sentence is not correct. Yes, the model emulates key behaviours, but is not sufficient to explore the space of potential transitions and interactions (which are taken linear).

We agree with the reviewer that this statement is inaccurate and does not address the importance of the non-linearity. With this in mind, the sentence has been adjusted to “it is able to explore some of the space of potential transitions” (line 434-436)

m22 410: this sentence is unnecessary in my oppinion.

We thank the reviewer for noting this unnecessary sentence and have removed it. (line 439)

m23 413: I would argue that “possibly” is not necessary here.

We agree with the reviewer that this is unnecessary hedging and have removed the word “possibly from this sentence. (line 442-444)

m24 420-449: I would be much more careful with these conclusions and frame them as key messages rather than key suggestions, especially for the first one.
The first message would be that, depending on the problem, the quality of models can become secondary when missing components have such a great impact. It is shown by this study and is a good message, but I do not agree that inclusion of more components is clearly a priority. For example, when considering the AMOC, better ocean and atmosphere models may result in a much closer tipping point of the AMOC. Is such a change more or less important than, for example, an interactive West Antarctica Ice Sheet?

We agree with the reviewer that these messages can be phrased more clearly. We have altered the wording to “messages” and for the first message, we have stated the main takeaway that “the impact of missing components in a climate model can have as great an impact on the uncertainty as uncertainty in the tipping point of a given climate element.”. We agree that the individual dynamics and their representation in climate models is also very influential for the outcomes of a model, but in our analysis we have shown that leaving out elements (such as WAIS or GIS) has such large knock-on impacts on the overall results that including these elements is critical. We have maintained some lines around the importance of inclusion of additional components as the Leave One Out analysis does specifically address this and show the variation from this to be as great as that from uncertainty in the tipping thresholds/timescales. (lines 449-451)

m25 658: should the “x” in “Nxd” not be a multiplication symbol?

We thank the reviewer for noting this issue and we have corrected this to a multiplication symbol. (line 699)

m26 table T1: in relation with M1, the caption clearly says “… for the tipping elements in this study”.

We agree with the reviewer and have altered this to “climate elements in this study” (Table T1). The use of key words such as “tipping elements” and “climate elements” is discussed in greater detail in M1.

m27 table T2: as for T1, I believe values are taken from or informed by these references. Kriegler & al gives some number for ENSO interactions but to the best of my knowledge, none of those references quantify ASSI interactions.

We thank the reviewer for noting this omission. In our analysis we judged the ASSI to have the same sign interactions as the GIS based on the literature, but certainly weaker in strength. We therefore took the decision to use a range of up to half the strength of the GIS. This is a wide uncertainty range which could likely be reduced in future research, but covers a wide range of possibilities which can be examined in the large Monte Carlo ensemble. This is described in Section 2 of the supplement (lines s76-84) and a new line has been added in the caption for Supplementary Table 2 making this link and decision making clearer.

m28 table T3: again related to M1– tipping risk should not relate to a climate component which is not a tipping element.

We thank the reviewer for noting this lack of clarity and potential to mislead the reader. As mentioned in the response to M1, we have added a section to the introduction of the manuscript to explain the choice of terminology and avoid confusion for the reader. We have also added a clarification to Supplementary Table 3.

Reviewer 2:

This is a much improved paper compared to the one originally submitted, reflecting comments made by all three reviewers. I didn’t comment too much about specifics in the earlier reviews because I thought the larger issues needed attention, so apologies for what might seem like trivial comments but these small things matter in the long run.

Kudos for the work done in revision. If the bulk of these comments were adressed, I would be happy to recommend its publication.

We thank the reviewer for their detailed comments and encouragement.

In the manuscript could you be clear about what you consider a component and what is an element. In the abstract, the distinction is quite clear, but in the paper, this is less clear, partly because the study assesses six components, four of which are elements. It would be best not to use these terms interchangeably. Their usage throughout the text seems to have no underlying rationale (the supplementary material is quite precise).

Could you make the usage consistent, establishing that usage up front?

We thank the reviewer for pointing out this lack of clarity, the terms have been clarified throughout the text, with the term element referring to the four tipping elements and the term component referring to the six climate components of the system. The terms have been refined to be consistent with the clearer definitions given in lines 4-7 of the abstract and 22-26 of the main text.

Introduction

Some general comments about the introduction, which may help the reader if you choose to tighten up the structure a little. As you have written it out, there are two main areas of uncertainty with respect to modelling. One is how the individual components are represented in models, stand alone and ESM. The other is within the ESMs themselves, which represents the interactions between components.

We agree with the reviewer's comment that the main sources of uncertainty are due to how the individual components are represented in standalone models (and Earth system models) and their interactions in Earth system models.

The main reason as to why the estimates of tipping point temperature have become lower is because of better dedicated models. There is a delay in representing these improved understandings in ESMs. The other is that the interactions within the broader climate network can only be represented thermodynamically within ESMs. How these interactions may play out within the network is the largest uncertainty at play here – not the capacity of the models to simulate individual variables into the future. It is likely that specific aspects of each tipping point will come into play here.

We agree that much progress has been made in improving the simulation of individual tipping points, but that currently the behaviour of interactions between elements is also a key factor.

When discussing uncertainty, the temptation is to use uncertain language, but the opposite is needed (this advice goes back to some of the foundational work in this area – Morgan and Henrion, Schneider and Moss). There is also a temptation to use emotive language about uncertainty to raise concern about future risks. Try and be as precise and as clinical as possible. This is why I have made what seems like a series of trivial edits in the introduction.

We thank the reviewer for their detailed and precise comments and suggestions, and have addressed them in detail below. We also looked through the manuscript and avoided uncertain language where possible.

Specific comments

Lines 23–30: Could you be even more explicit about the component/element issue here to help the reader?

We thank the reviewer for their comment, as per the reviewers first comment, the usage of climate tipping element and component has been revised throughout this section to avoid confusion and misuse. (lines 22-29)

Line 23: key components become critical organs on Line 25, then key Earth system components on line 27. Suggest removing the critical organs and staying with components (ENSO and ASSI also not organs).

We thank the reviewer for this suggestion, we have removed the word “organs” and replaced it with “components” (line 24)

Lines 30–32 Suggest While combinations of different drivers (rainfall, wind patterns, local temperatures) are decisive for the state of the key Earth system components, research has been able to trace many drivers back to the global mean surface temperature.
This is not a perfect suggestion either, because to my knowledge, wind patterns do not scale in this way – there are often multiple alternatives that are not linearly scalable.

We thank the reviewer for this suggestion and have implemented it in the manuscript. (lines 29-33)

Lines 32–33: of them – of components (otherwise ambiguous)

We agree with the reviewer that this is ambiguous and have altered “of them” to “components” (line 32)

Lines 34–36: suggest new sentence and reword to 2.2–3.4°C by 2100 under current policies and actions.

We agree with the reviewer and have adjusted the phrasing as suggested. (lines 33-36)

Lines 38–50: Are ASSI and ENSO included in this sequence? The term climate element seems uncertain on

ASSI and ENSO are included in this discussion as they are able to interact with the other all the other climate components (GIS, WAIS, AMOC, AMAZ). The phrasing has been changed to “climate components” where relevant to make this distinction clear. ASSI and ENSO are also able to undergo qualitative changes to their behaviour which could participate in a cascade. (lines 41-55)

line 40. Is it a tipping element or climate component?

We agree with the reviewer and have changed this to “climate component. (line 43)

Line 51: existence? Is this really an existential point or do you mean fundamental nature or intrinsic nature? It seems to be odd to be assessing the risk of something that may not be real. Especially when you know all these things have changed in the past.

We agree with the reviewer that in most cases the existence of the tipping points is not in question, and the existence of the climate components is certainly not in doubt. However, for some components (such as the ENSO or the ASSI) the existence of tipping point behaviour is uncertain (or even unlikely) and so we have used existence here to describe this. (Line 57)

Line 54: this means that – not needed

We agree with the reviewer and have removed “this means that” from the text. (line 60)

Line 56: dangerously – word choice – Concerningly?

We thank the reviewer for this suggestion to avoid misleading or alarming readers and have adjusted the word choice to “concerningly”. (line 62)

Line 57: greatly – repeatedly readjusted and reassessed?

We thank the reviewer for this comment, the “greatly” is unnecessary and so has been removed (line 63)

Line 64: many of these – most?

We thank the reviewer for this suggestion and have altered the word choice to “most” (line 73)

Line 72: can – redundant

We agree with the reviewer and have removed the word “can”. (line 80)

Line 74: recent progress means that – redundant

We agree with the reviewer and have removed this phrase (line 82)

Lines 76–78: known to still be missing key elements of the dynamics – what does this mean (an element of the dynamic?) Are elements missing or aspects, or are they insufficiently expressed?

This sentence is intended to link to the next paragraph which discusses the missing elements of the dynamics, which range from fully missing components to insufficient expression of other dynamics. We hope that the following paragraphs are sufficient to satisfy this comment, but are happy to make additional changes if required. (lines 84-86)

Also, the Abramowitz et al. review is pre-CMIP6, more about CMIP5 and they are on about model dependency and the current suite not representing the full range of uncertainty. You might need to tighten this up.

We thank the reviewer for noting this inconsistency, we have tightened the phrasing in the paragraph to refer to CMIP models more broadly and discussed their uncertainty (lines 87-90)

Lines 149–163: Is this really overcounting? Isn’t it a network multiplier effect?

We agree with the reviewer that this is a network multiplier effect, but this does lead to the same fractions of the variance being attributed to multiple parameters. This means that the variance effect by all of the parameters sums to greater than one, in effect overcounting.

Lines 164–166: where one element or interaction is removed from the system in order to analyse the system's response to this and understand the importance of the removed element for the overall behaviour of the system. Suggest: where one element or interaction is removed and the system response analysed to understand the importance of the removed component for overall system behaviour.

We thank the reviewer for this suggestion and have implemented it in the text. (lines 185-187)

Lines 171–173: this first sentence should directly refer to the parameter ranges in Table S1 and not be a general statement. This could apply to whole para – it should jump straight into the Sobol analysis and explain how the parameters influence the results. Perhaps the 1.3 °C comment should be in the intro (around Line 35).

We thank the reviewer for these suggestions, we have added a section into this first sentence (lines 192-195) clearly relating the uncertainty to the parameter ranges in Table S1, and have moved the 1.3 °C comment into the intro around line 35.

Lines 180–181: word choice, variance or variation but not variability.

We thank the reviewer for clarifying this word choice, we have changed the choice to "variation". (lines 198-200)

Line 183: No secondly, so no firstly.

We have removed the word "firstly". (line 201)

Line 201: critical – most important? (because critical is used in a different sense in the following para)

We agree with this phrasing and have altered it to "most important (line 220)

Lines 236–247: A little uneasy about the label real climate models on line 236, true climate on line 238 and the claim of full representation following this. This framing belies the caveats given earlier. Philosophically, I don't think truth claims can be attached to models, so a little judicious editing might be needed. Just refer to their respective strengths in representation. No problem with the underlying logic.

We agree that the phrasing here is unclear, we have removed the reference to real climate models and have altered the phrase “true climate” to “real world climate” and have rephrased the sentence for clarity. (lines 255-257)

Line 251: catastrophic – word choice

We agree that this word choice may be misleading for the reader and so have changed it to “damaging”. (line 272)

Lines 413–414: possibly always

We agree with the reviewer, and as mentioned in the response to Reviewer 1’s comment, we have removed the word “possibly” (line 442)

Line 531: ESD upper case

We thank the reviewer for pointing out this error and have corrected it. (Line 573)

Supplementary

Line 11: which – each.

We thank the reviewer for noticing this error and we have corrected this to “each”. (line 11)

Reviewer 3:

I think the authors have generally addressed my reviewer comments (reviewer 3) well, and the manuscript is much improved. I have a few minor comments, which I think should be addressed to improve clarity.

We thank the reviewer for their comments and encouraging remarks on the manuscript improvements.

Line 237 and beyond (Leave one out analysis)I find the difference between “Node Discounted” and “Node Removed” confusing. Can you give an explicit example what that means in terms of interactions etc?

We agree with the reviewer that this is insufficiently described in the text and have added in small sections describing what these scenarios mean in terms of nodes and interactions (lines 258-266). To clarify, the key scenarios are as follows:

> “Node Removed” scenario (where the model is run with the given climate node and all direct linked interactions removed)
>
> “Node Discounted” scenario (where the dynamics of all links and nodes are run as usual, but the climate component is removed from the final count of tipping events)

When interpreting fig 4 in particular, it would be useful to know what the likelihood of each individual component is to tip in the absence of any links. For example, WAIS always collapses for 4 degrees C warming. This is a very important assumption and hence piece of information for interpretation of these results. Could that information be added as a panel in a & b?

We thank the reviewer for this suggested improvement and have added this information as red bars to the first panels of Figure 4 & 6 to give the reader additional context. We have also added in a description of these bars into the figure captions as well as a note on their derivation from the uniform distributions of thresholds used to generate the temperature thresholds for the runs.

Since we have established that ASSI and ENSO are not tipping elements, what does fraction tipped mean for these in fig 4 and 5?

We thank the reviewer for this comment. As addressed in Reviewer 1 comment M1, although ASSI and ENSO are likely not tipping points (although there is uncertainty around ENSO in the current literature) they do show significant qualitative transitions under forcing, and are important for interactions with the (other) tipping elements. So the *fraction tipped* in these figures describes the fraction of runs in which these climate elements transition to the qualitatively different state.