

Review report for “A risk assessment framework for interacting tipping elements”

September 10, 2025

This manuscript introduces a fully probabilistic model to represent interactions among climate tipping elements and ENSO. Each subsystem is described as a Markov chain, and pairwise interactions are represented via a network, with strengths primarily parameterized from expert assessments (notably Kriegler, 2009). The authors’ aim is to provide a coherent and computationally efficient framework for risk assessment, integrating parameterized interactions into long-term and scenario-based analyses.

The manuscript includes analytical results for equilibrium risks and extends these with simulations under SSP scenarios to draw conclusions for the next 300 years. The probabilistic approach to parameterized interactions constitutes a novel contribution, and the framework is an original and relevant contribution, with the potential to advance the literature on interacting tipping elements. However, in its current form, the manuscript has several significant weaknesses: the unclear treatment of ENSO, an insufficiently explained role of timescales, a barely present reflection on limitations, and issues with clarity and presentation. I am inclined to support publication, provided these substantial issues are addressed. See below for more details.

Major Comments

M1: ENSO as a tipping element. The study considers nine climate subsystems, including ENSO. While the inclusion of ENSO can be justified by its central role in the climate system, it is not generally recognized as a tipping element, and its classification remains disputed, as acknowledged by the authors. The manuscript, however, repeatedly refers to ENSO explicitly and implicitly as a tipping element (e.g., “the nine tipping elements”), in the text, tables, and figures. I understand that the difference is hard to make throughout the study but, as it is, this is not correct.

M2: Timescales of tipping. The role and interpretation of tipping timescales are not sufficiently clear, at least for me. As written, it is difficult for readers unfamiliar with this methodology to grasp how these timescales should be understood, and what they influence. Do they represent the time for a subsystem to transition between states? If so, this should be made explicit.

M3: Conclusions and limitations. The conclusion is lengthy and could be made more concise and focused. In my opinion, several parts belong in the results section. More importantly, the discussion lacks sufficient reflection on limitations. Key questions remain:

- Is a fully probabilistic representation appropriate for physical systems? Is it more suitable than, say, Wunderling 2021?
- I understand that the interaction is still linear between the subsystems. This is a limitation, similar to previous work, and has to be acknowledged as such.
- What are the implications of limited data quality?
- What new insights are provided beyond previous studies such as Wunderling 2021?

- At present, many of the conclusions appear to be direct consequences of the assumptions rather than emergent results (e.g., interactions destabilise the system, when it is hypothesised that most are destabilising). It is crucial to articulate clearly what the results obtained via this framework add to current knowledge.
- How far could one go in studying coupled tipping elements via this framework?

M4: Readability. The manuscript contains many convoluted sentences and vague formulations, which make the first read unnecessarily difficult for a paper that is otherwise not highly technical. I strongly recommend careful editing for clarity and precision. I have listed specific examples below.

Specific Comments

Abstract

- 1: Capitalize “Atlantic Meridional Overturning Circulation”.
- 3: “collapse” and “into a degraded state” sound redundant; collapse alone should be enough.
- 5: Not sure, but isn’t “literature-based expert assessment” more correct?
- 6: “equilibrium risks of tipping elements” is not understandable at this point.
- 6: “their stationary distribution” alone, referring to tipping elements, is vague.
- 8: Capitalize “Shared Socio-economic Pathway”.
- 8: “Hypothetical” is not correct; I guess you mean “idealised”.
- 11: More correct would be “SSP scenarios with the strongest greenhouse gas emissions”.
- 12: What do you mean here by comprehensive? It does not seem fair to me. At least, in climate science, it is a confusing denomination.

Introduction

- 15: “Earth systems” is not correct. I think you mean “climate subsystems”.
- 15–18: The second sentence is not really an example of the first. The initiation of the marine ice sheet instability beyond some tipping point is indeed a nonlinear effect, but does not have to do directly with either path dependency or chaotic behaviour.
- 21: Please explain what you mean by “levels”.
- 25: More correct would be “most interactions between tipping elements are destabilizing”.
- 26: Say at least “tipping cascades”. I would say that here can be a moment to explain what a tipping cascade is.
- 27: Replace polar ice caps by “the WAIS”. I understand that the sentence does not work with this change, but the paper does not say that ice caps stabilize the AMOC.
- 28: Here and in other places, “saltwater” should be “freshwater”, or maybe “meltwater” if originating from an ice sheet.
- 33: Replace point by ,
- 35: I don’t understand “system model”.
- 31–44: Personally, I find that these two paragraphs are a bit of an overkill in this introduction. Or at least, they give me the feeling of taking too much space.
- 55–58: There is excessive use here of the —, this part is not readable. Please separate it into sentences, as it is I do not really understand what the authors are trying to say.
- 55: “noise-induces tipping in some climate models occur when” is not grammatically correct.
- 56: The description of N-tipping quoted from Ashwin 2021 introduces unnecessary technicality.
- 57: Maybe it is because of the weird phrasing, but I don’t understand “or through confidence intervals on parameter values”.
- 64: You do not remedy the shortcoming of linear interaction though, which is a big one when it comes to interacting climate tipping elements. As far as I understand, the interaction impacts the probability linearly.
- 77: “which accounts for the physical sensitivity of the element, or at least uncertainty in the critical temperature” is a very vague formulation, especially the “at least”.
- 82: “including under external forcing” is awkward in this sentence.

83: see M1.

85: “equilibrium degraded probability” sounds like the probability is degraded. I imagine there must be a better formulation.

Figure 1: In the caption, see M1.

Methods

92: I am not very familiar with these methods, so here it could be useful to have one or two short sentences that describe what will be done. Like “We describe each tipping element by ...”.

95: Here, I understand “sufficiently” should be “fully”.

In General: it is more common for me to write equations as soon as they are mentioned.

102: I don’t understand the notion of stability of an equation in this context. Is it the stability of the stationary distribution?

102: Absolute value of the Jacobian can mean a lot of different things, so it should be clear here.

109: “the adjacency matrix”.

133: see M1.

135: It is not explained what the two timescales are. In regard to M2, it would be a good moment to explain why the timescales are involved at this moment, in this way, and why in the value of the internal probabilities. As I read it, I understand that probabilities evolve with respect to these timescales, which is not what a tipping timescale is (i.e. the time for transitioning from a state to another)

144: Here, it is not indicated how you set these parameters beta. I understand it is in an Appendix; refer to it.

148: Eq 10 appears many pages after; for me this is awkward.

Table 1: For the table and caption, see M1. Also, regarding the *, I did not manage to find this information in Armstrong. Finally, the index i should be explained.

161: see M1.

167: I imagine you refer here not to Armstrong but Kriegler?

170: Please reformulate the first sentence.

171: I would not use “rife”, as it is not very non-English friendly. Same for dyadic, could use bilateral, or bidirectional.

180: What do you mean all 2 interactions?

180–181: Do you reflect on that later? It is a good reason to justify that you cannot include it, not that it should not be.

Results

188: I understand that the stability is rather the object of the next subsection.

238: I did not catch why eq 10 is “far from a theoretical result”; it seems to me that it is precisely what it is. Also, “into interacting tipping elements in the Earth system” could already be more specific.

247–248: Seeing this figure for the first time, I would judge that this variation is fairly weak on Fig 2, given the fact that most interactions are destabilising. I would like the authors to reflect on that.

252: Why the use of the French word “sans”, also in the figure? I think “scenario without interaction” or “interaction-free scenario” would work.

Fig 2: Again, degraded risk sounds like it reads as if the risk is degraded. For the figure and caption, also see M1.

259: typo for “cab” → “can”.

Fig 3: For the caption, see M1.

Fig 4: For the caption, see M1.

In section 3.3: these are “extended SSP scenarios”, not “SSP scenarios”.

Conclusion

315: This sounds like a direct consequence of the assumptions. Maybe you mean that, considering a robust representation of interactions, global warming remains the most important factor?

316: see M1.

332: What do you mean here by more complex models? In this paragraph, only Wunderling 2021 and

2023 are cited. Do you compare to these references? If yes, “more complex” is misleading.
332: In regard to M3, it sounds trivial, as most interactions are assumed to be destabilising.
342: saltwater should be freshwater or meltwater.
347: Here again, not sure how Wunderling is more complex than the presented model.
358: What does “nor the entire earth system” mean here? There is no representation of the Earth system in this model.
363: I guess the author means ‘safe overshoot’ here.
367: see M1.
395: I would begin with “to conclude”, or similar.

Appendices

Around 435: It seems to me that the method for choosing interaction strength is very subjective, and is not really announced as such.
In general: I have seen only references to appendices A1 and C. Please refer to appendices when relevant.

Suggestions

3: I would write “In doing so, ...”.
4: I would write “therefore, in this work ...”.
9: Here I would write “... . For instance, ”.
23: Maybe “each other”.
24: I would change ripple for propagate.
150: I would write “therefore, for mixed terms, ...”.
162: It would be clearer as “sensitivity to ...”.
163: I would write “for the sensitivity”.
166: I would rather write “although this review dealt with describing response strengths in qualitative terms ...”.
180: I would write “Moreover, note that, in this work, ...”.