

Interactive comment on “ESD Reviews: mechanisms, evidence, and impacts of climate tipping elements” by Seaver Wang and Zeke Hausfather

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Summary

Thank you for this paper, it is a helpful addition to the literature, and it will be well-suited for publication following review. I have made several comments specifically in the introduction and ice-sheet sections, which I hope the authors will consider and find useful for improving the manuscript. My primary suggestion is the inclusion of a more thorough discussion on how uncertainty affects the physics, projections, and

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understanding of timescales of ice-sheet tipping points.

General Comments

Line 7: “Shifting towards” seems a bit odd in the definition of climate tipping elements. It might be better to frame tipping elements in this context as a clearing a threshold, rather than the sign of a derivative? This wording appears more like it is referring to a climate anomaly, rather than moving to/arriving at a new climate paradigm.

Line 35: This paragraph would be strengthened by including a mention of adaptation. Certainly once a tipping point has been passed, significant changes in planning, decision-making, and climate adaptation will need to be adopted.

Line 94: As written, it was unclear to me what “0.54 in the high-end of the RCP8.5 warming scenario” means, or refers to. It probably needs to be either explained more directly and with appropriate context, or should be mentioned generally and more specifically covered in section 4.

Section 2.3.1: For context and contrast with the ice-sheet changes, it should be noted that sea-level rise from other components (thermal expansion, mountain glaciers) is also irreversible... but these do not necessarily have tipping points and exhibit more immediate gains upon mitigation (e.g. Solomon et al. 2009, Lenaerts et al. 2013, Zickfeld et al. 2016, Ehlert et al. 2018).

Line 388: This paragraph would benefit from being very specific about what is meant by “less insulated”. Why are they less insulated than the EAIS? The reasons and time frames are quite different between GIS and WAIS because of the differences in their physical responses to climate change (see the Hamlington et al. 2020 review), so grouping them like this confuses the message. Specificity and expanding this section would probably help clarify the point of this paragraph as I understand it: that there are heterogeneous tipping points across the polar ice sheets.

Lines 400-405: Marine ice-sheet instability is confusingly explained in this section, and

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will likely lose some readers. It might be helpful to refer to other papers which explain this process (especially less technical and more general papers about MISI, such as Robel et al. 2019), and rewrite for clarity.

Section 2.3.2: In the context of ice-sheet instabilities it would be helpful to more carefully introduce the flux rate across the grounding line as being proportional to the height at the grounding line, and then discuss where reverse sloping beds are found, and how that has the potential to affect the (in)stability of WAIS and EAIS (e.g. see topography in Le Brocq et al. 2010).

Line 409: 90m is not an exact threshold for MICI, it was a suggested model parameter value proposed in DeConto and Pollard (2016). Rather than speculate on these parameter values which are most relevant in a modeling context, it would be much more useful to instead rewrite this sentence describing the physical phenomenon being discussed, e.g., "MICI postulates that ice cliffs become unstable and collapse under their own weight if they exceed a critical height threshold (Pollard et al. 2015)"

Line 416: In introducing MICI, it would be useful to mention Clerc et al. (2019), which argues that MICI is unlikely given viscous relaxation dominating the response to ice-shelf removal. In contrast to Edwards et al. (2019) which makes a modeling/statistical argument, Clerc et al. makes a physical one.

Line 425: This paragraph is missing a key discussion on the deep uncertainty surrounding the Antarctic ice-sheet response. Deep uncertainty is characterized by the lack of agreement between experts (e.g. Bakker et al. 2017, Bamber et al. 2019), which has critical implications for decision-maker response to possible AIS tipping points (Rasmussen et al. 2020).

Line 425: Likewise missing is a discussion that our understanding of ice-sheet tipping points comes primarily from the paleoclimate record, at points in time when we know the AIS was at least partially deglaciated (e.g. Dutton et al. 2015, Capron et al. 2019). Current paleoclimate understanding has limited power in constraining our understand-

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ing of ice-sheet instabilities (Edwards et al. 2019), but improved geological estimates have potential to reduce future projection uncertainties (Gilford et al. 2020, in revision). Modern mass loss rates (e.g. from satellite measurements of SMB model estimates) have very little efficacy for reducing uncertainty on decadal time scales (Kopp et al. 2017).

Lines 445 and 482: It is unnecessary to continue mentioning the uncertainty highlighted by Edwards et al. (2019). Instead it would be better to flush out the original discussion in line 425, noting how it brings in deep uncertainty reflected in Bamber et al. (2019), and highlighting that anytime someone refers to a MICI scenario is a high-end or worst-case scenario being considered (such as the excellent line 477).

Section 2.3.3 and e.g. Line 490: It would be very helpful to reference the Bamber et al. (2019) expert judgement paper for projections; it is probably our best prior for projected ice-sheet mass losses.

Line 505: This should read "Consequently, under our current best understanding, ice-sheet collapse..." It's important to note that given the deep uncertainty (e.g. if MICI is possible), then changes could actually be abrupt. We just don't have a good enough grasp on the physics yet, and our observations (paleo and modern) are inadequate to constrain this property. Furthermore, because of these deep uncertainties, without progression of the science it may be impossible to know whether we are on a trajectory towards a tipping point (for instance, initiated MICI) until we have already crossed it (e.g. Kopp et al. 2017, their Figure 5).

Suggestions for Minor grammatical/structural/citation changes:

-Line 7: add comma after "some"

-Line 28 and elsewhere: "sea level rise" should be "sea-level rise". Please adjust accordingly throughout (e.g. lines 356, 358, 377, 386), and likewise for places where "ice sheet" should be "ice-sheet" (e.g. line 368, 377, 386)

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- Line 40: Should this be Turetsky et al. (2020)?
- Line 44: “larger uncertainty” is a strange word choice here. I think this is referring to ambiguity (Ellsberg 1961)
- Line 74: It will probably be addressed in type-setting, but this line is not formatted correctly
- Line 87-88: This sentence reads oddly as written, with subject confusion, etc. “the climate impact of the” should be “the climate impact of a”, and “uncertainties surrounding factors required” is very wordy. It should be rewritten for clarity
- Throughout section 2.3: the acronyms for AIS, GIS, EAIS, and WAIS need to be introduced only once (in lines 360 and 371-372)
- Line 360: add “By contrast” before “The Greenland Ice Sheet”
- Line 361: can this be more specific than “recently”? Maybe “accelerated over the past four decades (IMBIE 2018)”
- Line 365: A full review of the current understanding of modern ice-sheet contributions to sea-level rise is available in the in press Hamilington et al. (2020) review
- Lines 370-374: This sentence is written very oddly and is difficult to follow. I suggest rewriting for clarity
- Line 377: Suggest rewriting this sentence as “Major ice-sheet processes have had a dominating influence on sea-level patterns...”
- Line 380: Add “global mean” before “increase of 3.4m”
- Line 392: Add “(no net sea-level rise)” after “mass balance”, for clarity
- Line 392-393: Remove “and precipitation”, it is redundant, and add that it is related to increased atmospheric moisture from increasing temperatures.
- Line 393: Add “this balance is” before “subject to considerable...” for clarity

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- Line 395: Rewrite the beginning of this sentence as “Ice on the AIS margins is typically...”, and remove “at their edges” from the end of the sentence
- Line 401: Suggest citing Weertman (1974) at the beginning of the MISI discussion
- Line 406: Add “can” after “heating”
- Line 409: Replace “is” with “would be”
- Line 412: Please add a citation for the 800m threshold
- Line 453: Should “proving” be “providing”?
- Line 456: “net” before “feedbacks” would help clarify this sentence
- Line 457: Is this referring to geological field observations?
- Line 457: One key warming threshold paper for GIS that should be mentioned is Robinson et al. (2012)
- Line 478: Add “(but very uncertain)” before “MICI feedback,”
- Line 489: Would this discussion be helped by adding a sentence or two about the regional variability of ice-sheet loss projections (e.g. Kopp et al. 2017)?
- Line 495: Do albedo feedbacks also make it more difficult to stabilize the GIS?
- Line 496: Add “ice-sheet” between “individual basins,”
- Line 517: “instability” might read better as “instabilities”

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