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

Seaver Wang and Zeke Hausfather

seaver@thebreakthrough.org

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Asterisks denote reviewer comments

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 understanding of timescales of ice-sheet tipping points.**

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.

Following the reviewer's suggestions, we have revised this wording to better emphasize that the new system state represents a very different paradigm from the original and better reflect the threshold-based definition of tipping elements.

New text:

"Increasing attention is focusing upon "climate tipping elements" – large-scale earth systems anticipated to respond through positive feedbacks to anthropogenic climate change by transitioning towards dramatically different long-term states upon passing key thresholds."

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.

We have incorporated a mention of climate adaptation in this sentence based on this suggestion: "Many tipping mechanisms may also be difficult to halt, reverse, mitigate, or adapt to once they have begun shifting between states in response to climate perturbations."

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.

We have provided additional context before this sentence and have clarified this language: "and find the additional global mean surface temperature increase over the 21st century with the inclusion of tipping elements to be around 0.54C in the high-emissions

RCP8.5 scenario."

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).

We now note, with explanation, that these components contributing to sea-level rise are irreversible. After some consideration, we have decided it would be best to avoid elaborating further on threshold-like behaviour of glaciers and thermal expansion or exploring their response to different levels of mitigation, as these go beyond the scope of a section on ice sheet feedbacks that is already lengthly.

Added text at the end of the first paragraph of section 2.3.1: "While thermal expansion and loss of glaciers outside the GIS and AIS cause irreversible sea-level rise on human timescales due to the ocean's gradual response to warming (Ehlert and Zickfeld, 2018; Solomon et al., 2009; Zickfeld et al., 2017) and to shifts away from climate conditions that permit mountain glaciers to persist (Lenaerts et al., 2013), their relative contribution to future sea-level change will diminish (Clark et al., 2016). Mass losses from the Greenland and Antarctic ice sheets are similarly irreversible but will contribute the majority of expected future sea-level changes."

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.

We agree with the need indicated here to expand upon this passage. We have replaced

"less insulated to current and projected climate change" with "are more sensitive to current and projected climate change" and elaborated significantly on this section based on the reviewer's suggestion. In particular, we have moved the discussion of Greenland surface mass balance and related feedbacks up so that it directly follows this paragraph.

Edited passage and new transition into Greenland SMB paragraph are as follows: "Generally, ice basins of the GIS and WAIS are more sensitive to current and projected climate change and will likely reach key thresholds first, while the EAIS region responds more to higher intensities of warming (Golledge et al., 2015; Robinson et al., 2012). Significant ice loss is already occurring for both the GIS and WAIS in the present day, with an ongoing sea-level rise contribution of 1.20 mm/yr (Bamber et al., 2018; Oppenheimer et al., 2019; The IMBIE team, 2018). The EAIS is potentially at a mass balance (no net sea-level contribution) currently thanks to increased snowfall caused by a warming-induced increase in atmospheric moisture, although this balance is subject to considerable temporal variability and uncertainty that do not rule out the possibility of net loss since observations began (Bamber et al., 2018; Boening et al., 2012; Martin-Español et al., 2017; The IMBIE team, 2018; Velicogna et al., 2014).

The feedbacks affecting the major ice-sheets and the patterns and timeframes of their physical responses to climate change differ markedly between regions. In contrast to the Antarctic sheets, Greenland glaciers typically terminate on land prior to reaching the sea."

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

This passage was indeed unclear as originally written, and we have reorganized and revised it for clarity accordingly. We have also incorporated the suggested references and included additional discussion of the general topography of Antarctic ice sheet beds and the implications for WAIS and EAIS stability:

“However, marine warming represents the primary driver of Antarctic ice loss, as ice on the AIS margins is typically in direct contact with the ocean. Consequently, such marine-terminating glaciers are at risk of mass loss from processes that result from both oceanic warming (Shepherd et al., 2004) as well as atmospheric warming (Figure 3) (DeConto and Pollard, 2016). The observational record has established the key role of ocean-driven melt in the thinning and retreat of Antarctic ice shelves (Khazendar et al., 2016; Liu et al., 2015; Wouters et al., 2015), although ocean temperatures are themselves subject to atmospheric variability and forcing (Jenkins et al., 2016).

For marine-terminating ice-sheets, the rate at which ice flows out to sea is proportional to the height of the glacier above the grounding line beneath it where the submerged ice-sheet first contacts the bedrock below. On reverse slopes, where the bedrock's height decreases with further distance inland, this relationship results in a positive feedback where the height of the glacier above the grounding line increases as the ice-sheet retreats, which then further accelerates the rate at which the glacier flows out to sea (Schoof, 2007). This irreversible positive feedback mechanism for ice loss is called Marine Ice Shelf Instability (MISI) (Thomas and Bentley, 1978; Weertman, 1974). While not all vulnerable Antarctic glaciers terminate on reverse slopes and are currently thought to be undergoing MISI today, several major basins are currently retreating thanks to processes that may indicate MISI dynamics (Favier et al., 2014; Joughin et al., 2014; Rignot et al., 2014). In this process, warm subsurface waters cause melt beneath ice shelves, resulting in inland retreat of the grounding line (Shepherd et al., 2004). Should grounding lines retreat beyond forward slopes and onto reverse-sloping topography, as is the potential case for many major Antarctic basins

(Ross et al., 2012), the process of MISI would begin rapidly accelerating ice loss from many major glaciers. Much of the WAIS lies on reverse-sloping bedrock well below sea-level (Le Brocq et al., 2010), leading to generally higher susceptibility to MISI and greater instability of the WAIS under modest warming scenarios relative to the EAIS (Pattyn et al., 2018).”

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 theirown weight if they exceed a critical height threshold (Pollard et al. 2015)"

We agree that specific parameter values should not be included here and have revised the text along the suggested lines:

“a feedback mechanism known as Marine Ice Cliff Instability (MICI) may trigger at locations where the height of cliffs at an exposed ice-sheet's edge exceeds critical thresholds. Beyond such heights, the shear strength of ice would be insufficient to withstand longitudinal stress at the cliff face”

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 iceshelf removal. In contrast to Edwards et al. (2019) which makes a modeling/statistical argument, Clerc et al. makes a physical one.

This is indeed an important reference and we have now included it in our discussion of MICI in this section.

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).

We have revised the manuscript to elaborate upon the large uncertainties regarding ice sheet dynamics and the resulting wide range in sea-level projections within the literature and as shown by structured expert judgement. However, we have chosen to discuss this at greater length in Section 2.3.3 rather than in this paragraph focused on MICI. We have added a sentence to the MICI paragraph to guide readers to this discussion in the following subsection.

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 understanding 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).

Following this suggestion, we have added a passage discussing the role of paleoclimate work in assessing the response of future sea-level rise and ice sheet loss to climate change and the associated limitations (lines). We have included these suggested references aside from the paper in review, also referencing a couple of other relevant studies:

"Paleoclimate evidence points strongly towards past instances of significant ice-sheet loss and associated global mean sea-level rise during past warming episodes, Researchers have leveraged such paleorecords to estimate the response of global sea-levels to global mean temperatures (Levermann et al., 2013) and atmospheric CO2 concentrations (Foster and Rohling, 2013). However, uncertainties remain large due

to the limitations of proxy data, including the resolution of time dating and the need to extrapolate regionally or globally from available datasets. Paleoclimate records also possess limited ability to resolve key temperature thresholds for major ice-sheet loss, the ice-sheet mechanisms responsible (Edwards et al., 2019), and historical rates and magnitudes of sea-level rise, as reviewed by (Capron et al., 2019; Dutton et al., 2015). Nevertheless, further paleoclimate research carries the potential to better constrain the sensitivity of ice-sheets and global sea-level rise during periods that could serve as historical parallels to current warming."

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).

We now omit these redundant statements and have revised the indicated passage to highlight the nature of MICI scenarios as upper-end: "studies including MICI dynamics arguably represent high-end or worst-case scenarios based on ice cliff collapse mechanisms not yet fully validated by field observations"

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.

We have followed this suggestion and highlighted this reference within this section to emphasize the wide uncertainties associated with end-of-century projections and beyond: "Nevertheless, projections of future sea-level rise from ice sheet losses remain highly uncertain, primarily due to limited observational records, incomplete understanding of ice sheet dynamics, and model limitations. Estimates of ice sheets' contribution to sea-level by 2090 evident across a selection of published literature span a fourfold range (Bakker et al., 2017), with structured expert judgement yielding a similarly wide range of estimates (median estimate: 0.51 m, 95th percentile estimate: 1.78 m) (Bamber et al., 2019)."

Line 505: This should read "Consequently, under our current best understanding, icesheet 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, initated MICI) until we have already crossed it (e.g. Kopp et al. 2017, their Figure 5).

We have rephrased this sentence accordingly based on the reviewer's suggestion: "Consequently, under our current best understanding, ice sheet collapse cannot generally be considered an abrupt phenomenon"

Suggestions for Minor grammatical/structural/citation changes: **-Line 7: add comma after "some"**

To maintain the flow of the introductory element to this sentence, we elect to omit this change.

-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)

These adjustments have been made.

-Line 40: Should this be Turetsky et al. (2020)?

This is the case – change made.

-Line 44: "larger uncertainty" is a strange word choice here. I think this is referring to ambiguity (Ellsberg 1961)

Rephrased as suggested.

-Line 74: It will probably be addressed in type-setting, but this line is not formatted correctly

ESD submission format requests a justified rather than right-aligned layout, and this should indeed be addressed during type-setting.

-Line 87-88: This sentences 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

We have rewritten this sentence for greater clarity: "Evaluation of the risks posed by climate tipping elements requires considering their timescales of action, climate impacts, and important uncertainties surrounding triggering thresholds and associated factors."

-Throughout section 2.3: the acronyms for AIS, GIS, EAIS, and WAIS need to be introduced only once (in lines 360 and 371-372)

We have made this change.

-Line 360: add "By contrast" before "The Greenland Ice Sheet"

Addition made as suggested.

-Line 361: can this be more specific than "recently"? Maybe "accelerated over the past four decades (IMBIE 2018)"

This is a good suggestion and we now employ this wording.

-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

We now reference this review at the start of section 2.3.3.

-Lines 370-374: This sentence is written very oddly and is difficult to follow. I suggest rewriting for clarity

We have rewritten this sentence for clarify and brevity: “Climate change is expected to cause collapse of the GIS and large-scale losses from the West Antarctic Ice-sheet (WAIS) at lower levels of climate forcing, followed by further Antarctic ice loss as vulnerable basins of the East Antarctic Ice-sheet (EAIS) retreat under higher levels of warming (Meredith et al., 2019).”

-Line 377: Suggest rewriting this sentence as “Major ice-sheet processes have had a dominating influence on sea-level patterns: : :”

Rewritten to “Major ice-sheet processes have exerted a dominating influence on sea-levels over the geologic past”

-Line 380: Add “global mean” before “increase of 3.4m”

Addition made.

-Line 392: Add “(no net sea-level rise)” after “mass balance”, for clarity

Addition made.

-Line 392-393: Remove “and precipitation”, it is redundant, and add that it is related to increased atmospheric moisture from increasing temperatures.

Edits made.

-Line 393: Add “this balance is” before “subject to considerable. . .” for clarity

This has been clarified as suggested.

-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

Changes made.

-Line 401: Suggest citing Weertman (1974) at the beginning of the MISI discussion

After consideration, the current placement of this reference seems appropriate to us.

-Line 406: Add “can” after “heating”

Addition made.

-Line 409: Replace “is” with “would be”

Replacement made.

-Line 412: Please add a citation for the 800m threshold

This sentence was removed and is no longer present in the revised manuscript.

-Line 453: Should “proving” be “providing”?

This is indeed a typo and has been corrected.

-Line 456: “net” before “feedbacks” would help clarify this sentence

Edit made.

-Line 457: Is this referring to geological field observations?

Yes. We have clarified this.

-Line 457: One key warming threshold paper for GIS that should be mentioned is Robinson et al. (2012)

We have now cited this paper further above towards the start of Section 2.3.2.

-Line 478: Add “(but very uncertain)” before “MICI feedback,”

Addition made.

-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)?

This is a good suggestion and we have added a sentence to this effect: “Model projections also produce variable results for the magnitude and distribution of future regional sea-level rise if different assumptions are used to model Greenland and Antarctic ice loss (Kopp et al., 2017)."

-Line 495: Do albedo feedbacks also make it more difficult to stabilize the GIS?

We have clarified this.

-Line 496: Add "ice-sheet" between "individual basins,"

Addition made

-Line 517: "instability" might read better as "instabilities"

Change made.

New references featured in this comment:

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