

Authors response to RC2 of “Highlighting processes underlying the stability and hysteresis of the Antarctic Ice Sheet” (EGUSPHERE-2025-6566)

We would like to thank the second reviewer for their positive appreciation of the paper, their scrutiny and the detailed comments they provided. This will undoubtedly make our paper much stronger! The main concerns of the reviewer fall under three categories:

1. Our description of the equilibrium and quasi-equilibrium runs lack clarity.
2. The model setup and its limitations should be described in more detail (calving, calving mask, subgrid parameterisation, spun-up steady state, lateral melting, lack of hydrofracture, optimisation of thermal forcing corrections, dataset used for the geothermal heat flux, treatment of the gravitational feedback, parameter choices for subglacial hydrology).
3. The difficult readvance of the AIS should be investigated in more detail.

While the two first concerns are easy to address by modifying the manuscript along the reviewer’s suggestions, we are currently performing additional runs to support our hypotheses concerning the AIS regrowth. We hope that this will satisfyingly address all of the reviewer’s concerns. A detailed list of the reviewers comments (gray) and our answers (black) is provided below.

This is a novel and comprehensive study of tipping and hysteresis for the Antarctic Ice Sheet (AIS). The authors introduce a new technique, adaptive quasi-equilibrium forcing (AQEF), which improves on step- and ramp-based methods and is efficient and easy to implement. They show that marine ice sheet instability (MISI) is fundamental to AIS retreat in a warming climate for both the West Antarctic Ice Sheet (WAIS) and (at higher temperatures) the East Antarctic Ice Sheet (EAIS). They also quantify the role of glacial isostatic adjustment (GIA). They find that the ocean is the main driver of retreat for global mean warming up to about 5 K, while the atmosphere dominates for high levels of warming. They show that the EAIS does not have a single bifurcation point, but multiple bifurcations in different basins. Also, they highlight the importance of perimeter feedback, not only for land-based saddles but also for marine-based ice.

The paper is original, well organized, and clearly written. I liked the physical explanations describing the interplay of ice dynamics, geometry, and climate forcing. I also appreciated the discussions of relevance to future sea-level rise. I found AQEM to be an elegant method that could enable other groups to do similar experiments.

We really appreciate the positive feedback of the reviewer!

My critiques fall into two main categories. First, the paper could be clearer about the differences between equilibrium and transient experiments (with the latter being a closer analog of AIS retreat in the next few centuries. Second, it is unclear to what extent some of the results – including the limited role of atmospheric forcing for lower levels of warming and the long delay in marine-ice formation during regrowth – might depend on model simplifications. The authors discuss the influence of some model choices, such as the basal sliding law and grounding-zone melt parameterization, but they say little about other choices, such as ice-shelf hydrofracture and calving.

We agree that both aspects should be better described/discussed for clarity.

Specific suggestions follow, with some minor corrections at the end.

3: When I first read “This was simulated”, I was unclear on whether the reference was to this study or earlier studies. Please reword for clarity (e.g., “Earlier studies have simulation AIS hysteresis through ...”)

Agreed, we will rephrase in the revised version of the manuscript.

6: I suggest rewording slightly, e.g. “are only increased when the rate of ice volume loss is below a magnitude significantly smaller than the present-day rate of ice volume loss.”

Agreed, we will rephrase in the revised version of the manuscript.

36: MISI is defined here in terms of retrograde bedrock. Some authors (e.g. Schoof 2007) have described MISI in the context of 1D flow without buttressing from lateral margins or pinning points. There are studies (e.g., Gudmundsson et al. 2012) showing that marine ice sheets on retrograde slopes are not necessarily unstable in two dimensions. Please say a few words about this.

Yes, we will include this reference and discuss that negative feedbacks (buttressing following the reference provided by the reviewer, or upstream precipitation increase following Sergienko (2022)) can stabilise the grounding zone on retrograde beds.

Also, I suggest adding that MISI can also play a role in EAIS retreat. The next paragraph refers to two studies identifying melt–elevation feedback as the main driver of EAIS mass loss, but ice dynamics and ice–ocean interactions are also important for the EAIS.

We agree that it is the case (as shown later in our own paper) but it was not pointed out by earlier hysteresis papers of the AIS.

56: This is a good explanation of the drawbacks of step and ramp experiments. I suggest stating explicitly in the first sentence what quantities are typically stepped or ramped: T_{atm} , T_{ocn} , CO_2 , etc.

We will refer to the quantities from the beginning.

72: This and the following paragraphs briefly describe the model setup and parameterizations. I realize that you don’t want to get bogged down in technical details.

However, some of the details are pertinent to later results and should be stated explicitly, perhaps in appendices so as not to break the flow of the text.

The reviewer is right, and we are happy to include additional information in the supplement.

Here are several examples:

- The calving parameterization is described in one short sentence with a reference to Lipscomb et al. (2019). That paper simulates calving for Greenland, which is very different from Antarctica. Please describe the calving algorithm and calving law used in your simulations. (The CalvingMIP wiki has a good discussion of the distinction between laws and algorithms.) I am surprised that a simple stress-based parameterization in a coarse-resolution model can reproduce observed AIS calving fronts, as suggested by Fig. A2. Were any masks used to fit the observed present-day calving margin? How do ice shelves evolve under warming? Does the calving rate increase as the ice thins, or does the calving front retreat only when the surface or basal mass balance is sufficient to melt the column from the top or bottom?

As pointed out by the reviewer, Lipscomb et al. (2019) showcase the calving law applied to the Greenland ice sheet but follow-up work focuses on the past (Berdahl et al., 2024) and future evolution of the AIS (Lipscomb et al., 2021; Seroussi et al., 2024; Van den Akker et al., 2025). Nevertheless, the reviewer is correct: we prevent ice from growing beyond the calving front. This mainly serves the optimization procedure. To satisfyingly address the reviewer's concern, we will perform a control without imposing this mask and will show the result in the supplements. Furthermore, we will include more details about the calving front in the revised version of the manuscript: it only retreats when the combination of calving (governed by the stress), surface and basal melt is sufficient.

- Is there any treatment of lateral melting at grounded marine termini?

Yes, lateral melt of grounded marine termini (the vertical face of a ice-margin point grounded below sea level) is applied with melt rate given by basal melt rate, scaled to area exposed to ocean. We will include this information in the model description.

- The FastIsostasy model is said to account for sea-surface changes resulting from gravitational anomalies. Were gravitational effects included in any of the experiments? The later sections refer to isostatic rebound but not gravitational changes.

All experiments include gravitational perturbations, which are however of second-order magnitude compared to vertical land motion and therefore not highlighted in the rest of the paper. We will specify this in the model description section.

- The effective pressure N is based on a local basal till model. What is the value of the parameter δ , which determines the minimum N as a fraction of overburden? Similarly, what is the range of c_b values? It might be worth mentioning that the inferred τ_b is probably too large for the Siple coast, leading to a thick bias as shown

in Fig. A2. Also, I would be curious to know what fraction of the bed is thawed at the end of the spin-up; this could be illustrated with a plot.

We will illustrate the thawed bed fraction and the basal friction coefficient at the end of the spinup in a supplementary figure. We will mention the thickness bias at the Siple Coast, which is a region that commonly yields thicker ice in simulation compared to reality in many ice-sheet models (e.g. Garbe et al., 2020), especially when the model resolution is relatively coarse ($> 5\text{km}$, Williams et al., 2025). Furthermore, we use $\delta = 0.8$ and will motivate this choice in the supplements.

- How is the basal geothermal heat flux determined?

We use the GHF from Hazzard and Richards (2024), prescribed 2 km below the bedrock surface and the upper 2 km are simulated with active thermodynamics via diffusion.

Which datasets provide the thickness and velocity targets for the inversion? Is the spun-up state (mostly) free of drift?

The spun-up state is largely free of drift, as we will show in the supplements. The datasets used as targets for the inversion are Schaffer et al. (2016) and Rignot et al. (2011), as shown in Fig. A2. We will include this information in the main part of the manuscript.

- Why was hydrofracture not considered as a possible source of shelf breakup under atmospheric warming?

The reviewer is right to point out that hydrofracture should be included in the best case. However, hydrofracture is (1) computationally expensive to represent for such large timescales on continental extent and (2) difficult to constrain for configurations that are very different from the present-day one. Including it would certainly increase the equilibrium sensitivity of the AIS and we will mention this in the discussion.

120: Please briefly describe the PDD method. How well does the resulting SMB agree with the RACMO-simulated SMB?

We will describe PDD in more detail and the supplements will include a figure that compares the PDD results in Yelmo to the RACMO-simulated SMB.

124: When using the Jourdain et al. (2020) dataset and the Favier et al. (2019) non-local quadratic law, did you do any tuning, either basin-scale or local? For instance, did you adjust values of K or δT in Eq. (1) of Jourdain et al. (2019)? Tuning is often necessary to get a good match to the observed grounding line.

We are pleased to see that the reviewer is familiar with those problems and points out the limitations of our description. In fact, we optimize a per-basin thermal correction between -1 and 1K . We will show the corrections in the supplements.

Table 1: If the GIA model includes ice sheet self-gravity, please indicate how this is handled for UPL and DPR.

In UPL and DPR, GIA is completely turned off (including gravitation). We will specify this in Table 1.

126: When running the REF experiment using AQEF, how many model years elapse during the bifurcation periods with fixed forcing, as compared to the periods with slow, steady warming? It would be informative to add a table showing the number of model years elapsed during each of the major bifurcation periods. This would show whether MISI events tend to be of similar duration, or if some are much longer than others.

This is a great idea, and we will be very happy to include it.

141: I would suggest dividing the current Section 3 into Sections 3, 4 and 5 corresponding to the current 3.1, 3.2 and 3.3.

We prefer to have them as subsections of “Section 3 - Results” for clarity.

Figure 1: I suggest rewording the second sentence of the caption to something like “The coloured curves ATM, OCN and DPR show the equilibrium retreat when oceanic warming, atmospheric warming and GIA are deactivated, respectively.” On the first reading, I wasn’t sure whether OCN referred to ocean on or ocean off.

Agreed, we will include this correction.

152: The text refers to a phase of “relatively small sensitivity” to warming. I suggest writing this as “relatively small equilibrium sensitivity” and adding a caution that equilibrium sensitivity doesn’t necessarily correlate with transient sensitivity.

Agreed, this is an important nuance.

161: This is an important point about fragmented tipping, which I wish were more widely recognized.

Thanks, we will emphasise this more in the text.

179: “this ungrounding results in the formation of large ice shelves”. Please comment on how the calving scheme determines CF retreat. For example, does the calving front tend to remain in place as the grounding line retreats, allowing the formation of large ice shelves that can provide buttressing?

The CF retreats substantially during the collapses, as can be seen in all the videos provided as supplementary material. Thus, buttressing by ice shelves can be observed in some cases (WAIS collapse) but are largely absent in general (EASBs). We will include a sentence to discuss this.

220: This sentence confused me at first because it uses two separate measures of hysteresis: Δf for a given V and ΔV for a given f . Later (l. 417), these measures are defined as the hysteresis width and height. It might be helpful to introduce these definitions earlier in the paper.

We understand the reviewer's point but stress that the hysteresis width and height only make sense when comparing a retreat vs. a regrowth curve (l. 220 compares two retreat curves). We will make this more legible though.

Figure 4: I don't see the ATM curve mentioned in the caption. Also, where the caption says "retreat", should this be "regrowth"?

Correct, we will include ATM and will replace "retreat" by "regrowth".

As mentioned above with respect to the retreat experiment, it would be informative to have a table showing the duration of the bifurcation events in simulated years.

231: This is indeed a major policy challenge. Again, it would be helpful to know the simulated collapse timescales for WAIS and WSB.

Thanks for the suggestion, this information will be included.

243: The failure of the EAIS to advance beyond the coastline between 8 and 3.8K is a striking result. At some point (maybe in the Discussion), please comment on whether you think this is realistic.

Specifically, one might expect that when a reasonably thick grounded ice column (say, 200+ m) reaches the coast, it keeps going. It doesn't necessarily form a shelf, but it becomes grounded marine-based ice. Since it's grounded, it doesn't melt from below, so its advance is limited mainly by calving and lateral melting. Why doesn't grounded marine ice form earlier in the model?

The answer depends on the model details, but I could suggest one possibility. Suppose that during a given timestep, 1 m of ice is advected from the last cell above sea level to the first cell below sea level. If the ocean is warm (say, 5 K warmer than PI) and the ice is considered to fill the cell uniformly, then the simulated melt rate could be tens of m/yr. This would immediately melt the ice, such that the ice margin cannot advance. But if the model has a subgrid calving scheme, a grounded column with $H_{af} \gg H_t$ could advance to fill a small fraction of the grid cell and experience little or no basal melting. Have you considered such possibilities and determined that the failure to form grounded marine-based ice is not a model artifact?

We appreciate the suggestions and agree that these details are important. We should have described that we use a subgrid scheme for the mass balance of partially ice-covered cells, which discards the possible caveat mentioned by the reviewer. This will be included in the revised version of the model description. In addition, we have performed new runs that explain the difficulty in readvancing the grounding line between 8 and 3.8K, which can be largely attributed to the choice of melting at the grounding line and under shelves. We will include these results in the revised version of the manuscript along with a discussion.

252: This is the first mention of the WAIS failure to regrow when temperatures return to PI values. This is an interesting result, but I wonder if it might depend on model simplifications (such as the lack of a subgrid calving scheme) or the equilibrium assumption. For instance, WAIS fails to regrow after a retreat period during which the bedrock equilibrates to the lighter load. What about a transient case in which the climate cools before the bedrock fully rebounds? Please comment on how the transient outcome might differ.

Also, can you extend the AQEM simulation and determine at what temperature the WAIS does, in fact, fully regrow?

We believe that this is related to a question of the first reviewer to which our answer is: *We regrow most of the WAIS close to $f = 0K$, as can be seen in Video 5b of the supplement. However, as noted by the reviewer, the volume obtained is smaller than the present-day one. Going to more negative f does not allow regrowing back to 58 mSLE. This can be expected since the present-day AIS is the result of a transient trajectory coming from the LGM. In particular, the low barystatic sea level (BSL) from the LGM is necessary to regrow the AIS back to its present-day state, as we noticed by fixing $BSL = \{-120m, -90m, \dots, 30m, 60m\}$. We will include and discuss some of these runs in the supplements.*

260: Here and elsewhere, I suggest changing “bathymetry” to a more general term such as “basal topography” in cases where you’re considering both oceans and land.

We are happy to modify this.

271: What is the meaning of the initial time, $t = 144$ kyr? I’m not clear on how time is measured during AQEM experiments.

We believe the confusion results from an inaccurate formulation of the sentence beginning at l. 271. We will clarify this and add details of how time is treated in the quasi-equilibrium runs, since it seems to be a source of confusion for both reviewers.

305: See the question above about the details of grounded ice advancing into a warm ocean. I can imagine that with a warm enough ocean, calving and/or lateral melting at grounded ice fronts could keep the grounding line shallow, but these processes aren’t explained in the text.

404: “when the ice sheet has already reached most coastlines but is incapable of advancing beyond” – see comments above.

See our answer above about the subgrid parameterisation and difficulty of re-advancing.

466: The text says “melt–elevation feedback”. Should this be “melt–albedo feedback”?

Thanks for spotting this, we will correct it.

475: With regard to the AIS being incapable of advancing over marine regions, please see the comments above. PMPT and the regularized Coulomb law might be part of the explanation, but other model choices might also be important.

486: The text says that including melt at the grounding zone implies larger hysteresis. If it turns out that the failure of grounded ice to advance into a warm ocean is in part a model artifact, then this statement would need to be qualified.

We are currently investigating this in more detail via additional runs and will include these results to provide a compelling base for this argument, which should discard model artifacts as a potential explanation for the difficult readvance.

490: What is meant by a “realistic projection”? I’d suggest stating the temperature level and pointing out that this level is surpassed by most (or all?) of the SSPs, without commenting on the realism of the various SSPs, which is more of a socioeconomic judgment.

Agreed, we will rephrase as “any projection that does not include an immediate reduction of GHG emissions”.

498: “the regrowth curve from an ice-free Antarctica is a quite good proxy for the AIS regrowth from intermediate stages.” That depends on how close the intermediate stages are to equilibrium. On timescales of a few centuries or less, the system likely won’t be in equilibrium.

We will specify that we refer to the equilibrium regrowth.

500: The text states that following the regrowth scenario, sea level continues to rise even as temperature fall, because less snow is accumulating. I don’t know that this would be true for a transient simulation. It would depend on the relative timescales for ice dynamics, snow accumulation, and GIA.

The response timescale of the atmosphere is much shorter than that of the AIS and the reduction in precipitation would therefore take place faster than the adjustment of ice dynamics. We will include this physical argument in the revised version of the discussion to support that our statement would also apply to transient simulations such as climate scenarios.

507: “cooling the climate below pre-industrial level is unlikely and would probably have other undesired impacts.” There are several issues with this statement: (1) I don’t think that equilibrium experiments, by themselves, give us confidence that a collapsed WAIS would not regrow under a return to a PI climate. (2) It’s hard to say whether cooling below PI levels will be unlikely or unfeasible given the technology and political systems of the distant future. (3) “Undesired impacts” is vague and subjective.

We agree that “undesired impacts” is vague and subjective, and will modify this. We think that cooling below PI levels (independently of its feasibility) poses the same problem as global warming currently poses: human societies struggle in adapting to new climatic conditions. The revised version of the manuscript will be more cautious about this discussion. Nonetheless, we believe that our experiments point out that cooling to PI warming levels does not allow the WAIS to regrow. This can be addressed by lowering the barostatic sea level (see our answer to the first reviewer cited above) and will show this in the supplements.

594: “ungrounding the major EASBs via MISI is relatively unlikely over the coming millennium.” The text hasn’t given a time scale for how long it would take to unground these ASBs with sustained high temperatures, so I don’t see how this statement is justified by what’s gone before.

This refers to the results obtained by Coulon et al. (2024, 2025) and Klose et al. (2024), which are projection exercises. We will make sure there is no ambiguity there.

599: It's appropriate to list model limitations at this point in the paper, but as indicated above, I would like to see more discussion in Section 3 of how particular limitations might influence particular results.

Hopefully, the additional model description will clarify the model setup and will allow us to keep the discussion of the limitations in this penultimate section.

Two other limitations that could be mentioned are (1) a simplified treatment of calving and lateral melting and (2) the lack of ice-shelf hydrofracture.

We will include this in the discussion.

608: I think this is the first mention that WAIS retreat is initiated in Thwaites. Earlier in the paper, could you add a few sentences about how WAIS retreat comes about? You could simply summarize what the videos show.

Yes, we will happily do this.

Figure A2: The letters in several panels don't match the caption. Also, it's hard to see the biases for key regions such as Thwaites. It would be useful to make the panels larger or add some some plots zooming in on WAIS.

Thanks, we will correct this and optionally include a zoom.

Figure D2: The reference to Figure 1 k-l is not correct.

Right, it should be 1.g-h, thanks for pointing this out!

Minor corrections and suggestions

- 30: "display" -> "have"
- 52: "melt-elevation" -> "melt-elevation feedback"
- 164: "amount" -> "number"
- 184 and elsewhere: "Gamburtsev" is misspelled.
- 352: "remoteness to" -> "distance from"
- 369: "We propose to further quantify" -> "We further quantify"
- 431: "We propose to compare" -> "We compare"
- 468: "the similar values" -> "similar values"
- 559: "eased" -> "aided" or "supported". Also l. 562.
- 634: "might result much more complicated". Please reword.

Thanks for spotting these, they will be corrected.

References:

Favier, L., Jourdain, N. C., Jenkins, A., Merino, N., Durand, G., Gagliardini, O., Gillet-Chaulet, F., and Mathiot, P.: Assessment of sub-shelf melting parameterisations using the ocean–ice–sheet coupled model NEMO(v3.6)–Elmer/Ice(v8.3), *Geoscientific Model Development*, 12, 2255–2283, <https://doi.org/10.5194/gmd-12-2255-2019>, 2019.

Gudmundsson, G. H., Krug, J., Durand, G., Favier, L., and Gagliardini, O.: The stability of grounding lines on retrograde slopes, *The Cryosphere*, 6, 1497–1505, <https://doi.org/10.5194/tc-6-1497-2012>, 2012.

Jourdain, N. C., Asay-Davis, X., Hattermann, T., Straneo, F., Seroussi, H., Little, C. M., and Nowicki, S.: A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections, *The Cryosphere*, 14, 3111–3134, <https://doi.org/10.5194/tc-14-3111-2020>, 2020.

Schoof, C.: Ice sheet grounding line dynamics: Steady states, stability, and hysteresis, *Journal of Geophysical Research*, 112, F03S28, <https://doi.org/10.1029/2006JF000664>, 2007.

CalvingMIP wiki: <https://github.com/JRowanJordan/CalvingMIP/wiki>

Thanks for the references, we will include them!