Authors response to RC1 of “Highlighting processes underlying the stability and hysteresis of the Antarctic Ice Sheet” (EGUSPHERE-2025-6566)

We happily received the first reviewer’s feedback which, besides some much appreciated positive comments, included a very thorough analysis of the paper. This will surely help us to improve the quality of the paper and we’d therefore like to express our gratitude. 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 details (lack of comprehensive landscape evolution model, lack of ice-shelf fracture, choice of the enhancement factors, basal friction field, evolution of the atmospheric and oceanic forcing as the topography evolves, dataset used for the lithospheric thickness and mantle viscosity).
3. Our formulation of the perimeter feedback would greatly benefit from an analytical approach to support the associated bifurcation behaviour. More generally, the reviewer seems to be interested in the importance and interaction of the various feedback.

While the two first concerns are easy to address by modifying the manuscript along the reviewer’s suggestions, we will address the third one by deepening our analysis. We hope that this will satisfyingly address all the concerns that were rightfully expressed. A detailed list of the reviewers comments (gray) and our answers (black) is provided below.

Dear Editor,

The authors present a compelling set of numerical simulations on the equilibrium conditions for the AIS under a wide sweep of thermal forcing and mechanism denial tests. The main results use a new, adaptive slow forcing technique yielding a much finer set of bifurcation points than previous studies. These bifurcation points are associated with positive feedback loops in deglaciation and glaciation runs and show uneven characteristics across these stages. The authors combine previous saddle merge and collapse terminology under the name of the perimeter feedback, and show that this feedback plays an important role in setting up equilibrium hysteresis alongside other feedbacks. Overall, the paper is well-written and of strong quality. I have detailed comments throughout given the comprehensive scope of this work.

Thanks for this positive feedback!

Major Points

1. I would like to see more effort to connect the geometric effect of energy minimization that the authors term the perimeter feedback to other physical systems. While I have not seen this formally defined, the geometric idea seems similar to ice floes melting in the marginal ice zone (aka duration of melting of one discrete sea ice chunk of area A vs 2 chunks each of area A/2), droplet aggregation and splitting as the authors mention with surface tension, and bacterial colony growth.

   a. The authors show in Figures 6, D2, and D3 what drives the perimeter feedback, and mention in line 296 that it is analogous to surface tension for droplets in an energy minimization framework. If possible, describing energy minimization with something like an energy functional could strongly complement the numerical results.

We agree that this would nicely complement our findings. We stress that the perimeter feedback comprises many aspects (dynamics, rheology and SMB) and formulating all of them within an energy functional is beyond our abilities. However, we managed to derive an analytical formula of the SMB bifurcation associated with the merging of two idealised ice caps and will include this in the revised version of the manuscript.

2. Equilibrium limitations are not as thoroughly described as I believe they should be.

   a. Lines 68-69.

      i. State which feedbacks you are considering.

We will specify this.

   b. Since you're trying to stay in equilibrium, these processes are not going to happen in the exact order or timing as in the future AIS. Rather, you can say that these processes take place in the absence of being overwhelmed by external forcing and take roughly x many years to re-equilibrate. Equilibrium behavior is a useful end member of ice sheet dynamics, and a good way to understand the system.
   However, it largely never happens in the real world because ice sheets are not in equilibrium and environmental noise can create a variety of responses (Sergienko and Haseloff, 2023). I think it is important that you convey these ideas.

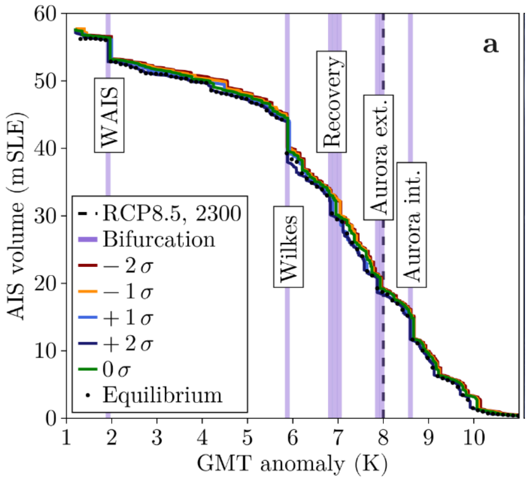
      i. Lines 131-132: You should frame capturing self-sustained ice loss capture in your equilibrium framework as a tradeoff, since you lose disequilibrium information.

We agree that equilibrium simulations are idealised and will emphasise that they mostly provide information about the attractor of the AIS given a certain forcing. It should be noted that our results are largely coherent with recent long-term projection studies (Coulon et al. 2024, 2025; Klose et al., 2024), as pointed out in the discussion. However, we will also emphasise that the out-of-equilibrium behaviour of the AIS is studied in more detail in a related study (Swierczek-Jereczek et al., in rev.).

   c. Please touch upon how time-dependent feedback processes work in your equilibrium setting. For example, GIA is often described in terms of the response time based on the mantle viscosity in West Antarctica, with the ability to act as a negative feedback or to not kick in fast enough (e.g., Whitehouese 2018). There is not a clear enough discussion of the relative timescales of feedback processes and forcings in your study that are blended through the adaptive forcing, as well as how long they would take in a non-equilibrium run.

i. Lines 221-222 are an example of where it is important to qualify GIA in this equilibrium manner.

All the runs performed in our study are transient. The only difference between them is the forcing applied, which (1) evolves in time according to the AQEF criterion (dm/dt < epsilon) for quasi-equilibrium runs or (2) stays constant for equilibrium branch-offs. Thus, all model components including GIA are integrated forward in time. Since equilibrium is tracked, the various time scales of the model components are filtered out, and very similar quasi-equilibrium retreats are obtained regardless of the mantle viscosity (see figure attached below). The only exception to this are the bifurcations, which can vary slightly in amplitude because during a basin collapse the simulated system cannot maintain equilibrium and timescales can play a role (e.g. a higher mantle viscosity leads to a larger amplitude of the bifurcation). We will clarify this in the description of the AQEF and its results.



Retreat diagram of the AIS for various fields of mantle viscosity (sigma is the standard deviation of the viscosity estimate). This figure stems from the revised version of [Swierczek-Jereczek et al. (in rev.)](#).

d. It would be very insightful to expand upon the relative magnitude of feedbacks, e.g. melt-elevation, melt-albedo, and MISI. Lines 228-230 start to discuss this, and I think a clearer evaluation of these would be useful to consider.

We will spend more time on quantifying the role of melt-elevation versus MISI in the revised version of the manuscript.

i. What do you mean when you say melt-elevation exerts a strong control on retreat, but never represents the sole driver of bifurcation? Is there an order where one kicks off the other? Please expand.

We mean that surface melt can kickstart marine retreats, which are driven by MISI and reinforced by the melt-elevation feedback but never driven by the melt-elevation feedback. The melt-elevation only becomes the main driver of self-sustained retreats at very high warming, where it plays an important role in the perimeter feedback that drives the saddle collapses. We will clarify this.

e. Lines 324-326.
    i. To what extent are these results interpretable outside of equilibrium?

Once triggered, bifurcations are largely independent of the forcing because the internal/autonomous dynamics dominate. The relative importance of MISI vs. perimeter feedback should therefore largely apply to transient runs too. We will add a sentence to mention this.

f. Lines 570-571.
    i. Please reference that these processes are identifiable on equilibrium timescales.

They are also identifiable on transient timescales (e.g. MISI-driven flow acceleration in projections, saddle collapse of the Laurentide ice sheet in deglaciation runs).

3. More explanation of reasoning when it comes to modeling choices.
a. Lines 76-77.
    i. Please state why you only use one field for mantle viscosity. For example, there is debate about the mantle viscosity in West Antarctica. I recognize these experiments are expensive, but acknowledge the uncertainty associated with this.

As mentioned above, we show in a related study that the mantle viscosity field chosen only marginally affects the equilibrium of the AIS (Swierczek-Jereczek et al., in rev.). We will include a sentence clarifying this. We didn't explore additional datasets yet and will mention this source of uncertainty in the discussion.

b. Lines 79-80.
    i. How is the vertical resolution advantageous for changing the temperature? If not what's the thermomechanical bit doing?

The finer resolution at the base allows the model to better capture the temperature changes that are relevant for subglacial hydrology, sliding and rheology (the thermomechanical bits). We will include a sentence to clarify this.

c. Line 81.
    i. Please explain the sigma-transform and provide a citation.

The sigma-transform is standard in ice-sheet modelling and we believe that an explanation here would not be appropriate. However, we will provide several citations for this (Greve and Blatter, 2009; Robinson et al., 2020).

d. Lines 82.
    i. Please provide some reasoning for making the ice shelf viscosity different.

The viscosity evolves heterogeneously throughout the simulations depending on the pressure and the temperature fields. We understand that the reviewer refers to the enhancement factor which captures the fact that, as ice is deformed, its crystal structure is altered and its mechanical behaviour becomes increasingly anisotropic. Therefore, it is quite common to use lower enhancement factors in fast flowing regions (e.g. shelves) compared to slow flowing ones (e.g. shear). We will add a sentence and a reference to clarify this.

e. Line 83.
    i. Please provide the suitability and limitations of DIVA for your application in one sentence.

We will add this.

f. Line 84.
    i. Please provide a statement of the selection of Von Mises criteria in terms of its advantages and simplifications. Further, acknowledge if you do not all for any fracture-driven collapse, as observed in Larsen A, B, Wilkins, Wordie, Prince Gustav, Conger-Glenzer, and Hektoria-Green ice shelves, for example.

We agree that this is an important clarification since the calving method used in our simulation setup excludes the possibility of hydrofracture, damage evolution and marine ice-cliff instability. This will be discussed in the revised version of the manuscript.

    ii. More description for the lack of additional processes – are processes like glacial carving and sediment transport, or volcanic activity, okay to neglect on these timescales? I understand that you cannot model everything, nor are there models readily incorporated for these processes, but it is useful to state what is not modeled and the associated magnitude of response, as well as any estimates of the validity of these assumptions on the timescales of your models.

In fact, none of these processes are represented in a typical state-of-the art ice-sheet model, including ours. To avoid listing processes that are never represented (e.g. volcanism), we will mention that our model setup includes all processes within the state of the art for continental ice-sheet modelling, as described by Robinson et al. (2020).

g. Line 109-110.
    i. Please provide the readers with the reason that you did not include an energy balance. It is quite relevant, in the main three feedbacks that you have listed, and included in other recent studies (Leloup+ 2025).

A comprehensive treatment of the energy balance requires a global land surface model, which in turns requires an atmosphere-ocean circulation model. This makes the computation particularly expensive. Instead, we prioritized the possibility of performing many permutations in our study (e.g. OCN, ATM, DPR, UPL, EQL, regrowth from intermediate retreat) in order to increase our understanding of the many processes that govern the AIS response before the melt-albedo feedback becomes dominant. The latter only occurs at very high warming levels (Leloup et al., 2025) and is therefore less relevant when focusing on the policy timescales. We will add this to the model description.

4. Restructure your Forcing Setup subsection for readability.

a. Something like this - “We force our AIS model with the decomposition of a global-mean temperature (GMT) anomaly f into atmospheric f_a(f) and oceanic f_o(f) forcing”. (You should define or redefine GMT and f in this paragraph.)

We will redefine GMT and formulate f_a and f_o as f_a(f) and f_o(f).

Please then state the spatial dependence (in x,y,z) of these atmospheric and oceanic forcings in adjacent sentences. “The timescale of applied forcing f is treated as a dial that adaptively tunes online such that the ice sheet effectively reaches equilibrium between each step Δf, discussed in the next paragraph.”

We will make clear that the reference fields have a spatial dependence, although the anomaly applied to them is spatially uniform.

b. For atmospheric and oceanic conditions, please explain what your forcing is when the ice sheet is smaller than present-day, and you must interpolate/extrapolate into currently ice-covered volume.

For the ocean, we apply the forcing of the nearest neighbour. In contrast, the atmosphere does not need any extrapolation but the temperature and SMB are adapted to the topography evolution.

c. When you shift your atmospheric forcing by a constant, you claim that this is uniformly added to the surface temperature. Presumably this is uniformly added to the entire atmosphere as you have a fixed lapse rate, but the only thing you do numerically is add a constant to the surface layer. This was confusing to read and could be clearer.

The anomaly is uniformly added to the reference temperature field. This is then corrected according to the (non-uniform) topography evolution via the (spatiotemporally) constant lapse rate. We will make this more explicit.

d. End with the limitations that come from multiplying by constants, and if there are studies (e.g., Moorman 2020) that show strong regional variations in the forcing that a constant multiplier will not capture.

Thanks for providing this additional reference. We will include it in the discussion.

e. Lines 127-128.

   i. You can write f dot a and f dot o in one equation to show that your preserve the forcing structure.

We will formulate the forcing rates as scaled GMT rates.

> Further, please state how you arrived upon epsilon as 40 Gt/yr.

Epsilon should be sufficiently small to ensure quasi-equilibrium while being sufficiently large to spare computation time. We found this value to be a good compromise by trial and error. We will include a sentence specifying this.

5. Can you develop a more robust selection process for bifurcation points?
    a. Figure 1 shows that there are many steps in the staircase that is the retreat branch of the reference experiment. Could you define the bifurcation points based on standard deviations away from the mean change in m SLE, such as 3 standard deviations? This would provide a more generalizable framework for future studies.
        i. E.g. lines 159-160: 50 m SLE makes sense here, but perhaps in other ice sheets like GrIS, and paleo ice sheets, a statistical outlier detection will be more practical. For your purposes you don't have to change your results, but it would be nice to know how large of outliers these are from the mean or median.

The AQEF itself provides a reproducible framework to identify bifurcations based on a physical criterion (dV/dt > epsilon), since an abrupt transition is taking place if the condition is true while the forcing is held constant for a certain amount of time (we will insist on this last aspect). This is particularly apparent in the videos provided in the supplements, which serves as the primary basis to identify bifurcations in this spatiotemporal system. We understand that the reviewer would like to see an a-posteriori statistical way of determining bifurcations but we believe that this would crowd the paper without adding value to it.

    b. Similarly, a more quantitative description of how/why the authors select the three phases to characterize the retreat curve in Line 152 would be beneficial for readers.

The phases are identified by relying on the large differences in the average slope of dV/df, which we believe is sufficiently quantitative for a paper that focuses on physical mechanisms rather than on statistical methods.

        i. Add to line 167 – it is worthwhile to say that is it the largest bifurcation, and then that it kicks off further retreat.

Thanks, we will add this important detail.

6. On Policy Implications and Uncertainty Communication
    a. Lines 152-153.
        i. i. Either say relative to the entire ice sheet loss, which I believe you imply, or don't say a relatively small sensitivity to warming through 5.9 K given that WAIS loss is a major concern for the next century of research.

As guessed by the reviewer, we mean relative to the entire ice sheet loss and will include this for clarity.

b. Lines 230-232.
    i. You need to be clearer about assumptions. You effectively have an equilibrium response, and it is not clear to me that you test for critical slowing-down or any other early warning signals. You have a very simplistic representation of calving and no ice shelf collapse. It is unclear how these large perturbations may change your results. Be cautious as you don't know the background of future readers that wish to cite your work.

The statement does not refer to CSD nor any other early warning technique but to the fact that ice loss is intrinsically abrupt and fragmented, which is hard to mitigate. We will make this clearer.

c. Line 505.
    i. "Probably have other undesired impacts" – please back this claim with an example and citation, or please remove it.

Thanks, we will exemplify this and support it by citations.

d. Paragraph starting in line 599.
    i. Doesn't your first sentence imply that we cannot make takeaways about WAIS collapse, as well as EASBs? Please make a statement about uncertainty with respect to this fact and policy implications. Can you provide an estimate of how much uncertainty is there around values of f and bifurcation points relative to controllable model parameters, such as grounding zone resolution? (I understand computational cost, but this seems relatively easy to test in a simple setup.)

This is in fact what we mean. The present paper is not an assessment of the exact quantitative values of the bifurcation points (or the underlying uncertainties), which is impossible given the state of the art. Rather, the paper shows how the stability and hysteresis diagram is more complex than previously assessed in other studies and we analyse why. We will state this more clearly.

1. Seems to entirely be at odds with lines 636-638. How do you feel confident in this prediction based on timescale, MISI resolution requirements, and unknown future state of our world?

This prediction is not based on a timescale, as we simply mention that the bifurcation point will be overshot while specifying, in the discussion section, that (1) overshooting does not imply a collapse unless the forcing is maintained for sufficiently long and (2) our bifurcation points for the WAIS and WSB are coherent with other results from the literature. Nonetheless, we will slightly modify our wording to prevent any confusion.

7. Figure 12.
    a. I personally really appreciate that you've shown the nearly horizontal nature of ramping down GMT. I would do more to highlight this result.

Thanks for this comment, we will happily highlight this more thanks to additional runs that we are currently performing.

b. i. Lines 496-498: can you go into more explanation into why these lines are horizontal? Is there an intuitive explanation how a lack of intermediate regrowth curves?

Yes, we are currently running additional experiments to highlight the importance of the basal melt parameterisation at the grounding line and in shelf regions, which are partly responsible for the “horizontality” of these curves.

c. ii. You may want to add in words that you cut off the GMT anomaly to not be negative. Your results suggest that negative GMT anomaly is necessary to actually regrow WAIS after collapse, which is the first tipping point in your work. If you have already run the results, can you show the negative values of this plot (negative f) that would be required to regrow WAIS?

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, …, 30m, 60m}. We will include and discuss some of these runs in the supplements.

Minor Points

1. Line 6.
   a. Please specify your rate of volume loss-dependent temperature anomaly scheme so that it is immediately clear to readers.

Specifying this numerical value is quite technical and feels odd for an abstract to us.

   b. Missing the word loss in “ice volume loss”.

Thanks, will be corrected.

2. Line 9.
   a. Either use “The merging of two ice caps”, or “The merger event of two ice caps”.

Agreed, we will use “merging of two ice caps”.

3. Lines 10-12.
   a. This is too vague. Perhaps something like the tendency to correct for geometric deviations from axisymmetric spreading and retreating ice caps.

We will try to make this clearer.

4. Line 16.
    a. Be more specific in which positive feedbacks.

Mostly MISI, we will include this.

5. Line 17.
    a. It is unclear whether you are arguing that the magnitude of hysteresis is larger in your experiments, or whether you think that it should be larger, or for which experiments you are discussing. Please clarify.

We agree that this is formulated in a confusing way and will correct this.

6. Lines 18-19.
    a. It wasn't clear on first read that these are changes that you include that weren't included in other models (partially due to their publication year relative to the publication time of process studies). I would mention that you include these updates relative to previous studies.

We agree and will clarify this.

7. Line 27.
    a. On many occasions, you state irreversibility in practical terms (e.g. also line 506). Please provide an order of magnitude estimate of the associated timescale, or do not make a vague claim. This is especially relevant if you hope people from outside of glaciology read your work from the policy sphere.
        i. Where is the millennium timescale coming from in line 595? If it is from other studies, I would cite them directly after the timescale is stated to be clear.

We tried to convey our definition in this very sentence: if readvancing a region of the AIS requires pre-industrial GMT (or even less), we deem this as "irreversible in practical terms". We will make this clearer and will preferably use "irreversible over policy timescales" wherever applicable.

8. Lines 34-35.
    a. Please claim that these are the main three positive feedbacks if you intend to do so to properly prime your audience. It is odd as a reader to see these three, and then a couple of smaller or speculative ones afterwards.

We will emphasise this in the sentence introducing them: "To date, the presence of an AIS hysteresis is explained in literature by highlighting three **main** positive feedbacks."

9. Line 48.
    a. Please state at the end of this paragraph that your model does not include the melt-albedo feedback. Same in lines 526-527.

We haven't described our model yet at l. 48 and it therefore feels odd to mention this here. In addition, we mention this limitation in the model description and in the discussion.

10. Lines 73-75.
    a. Please mention the time step, discretized grid and forward model choice, and other basic information related to how you solve your numerical equations.

The time step is adaptive and we will mention this. The grid is already specified at l. 79. The numerical equations are solved via finite differences (and an associated linear solver) but this information, along with many others, is already in the model description paper that we refer to (Robinson et al., 2020) and would unnecessarily crowd the paper.

11. Line 90.
    a. Only introduce the necessary number of new variables. You only need w and (1- w) if 0 <= w <= 1.

Thanks. We agree and will modify this accordingly.

12. Lines 102-103.
    a. Please write out mathematically what you are describing with words and with your citation. I find it to be too dense to read the sentence once with full comprehension in its current form.

The equations are described in the original paper and would crowd the present one, which does not focus on subglacial hydrology.

13. Line 105.
    a. Is the basal friction coefficient static for all time? Regardless of whether you keep it static, please comment on whether this should evolve, and the physics that would go into modeling this.

The field of friction coefficient is constant in time, but the friction itself depends on the time-evolving conditions (overburden pressure, ice velocity). We will state this more clearly. Furthermore, we emphasise that this field is deliberately not evolved in time, since subglacial erosion is not a converging physical process (erosion rates are present as long as an ice sheet exist) and therefore prevents the obtention of equilibrium states.

14. Line 134.
    a. It seems that the purpose of REF is to build an equilibrium response. Therefore, I find it confusing that it is discussed as a transient experiment and would remove the word transient in 134.

Agreed, we will remove “transient” to prevent any confusion.

        i. Same with lines 144-145; I did not find this surprising. Can you explain why you would expect this to be striking?

The match between quasi-equilibrium and equilibrium is (strikingly) good compared to previous studies. We will rephrase for clarity.

ii. Line 148 – are there other studies that have used a large ensemble of equilibrium runs that you are considering? Is there any advantage to doing this?

Yes, Leloup et al. (2025) use this approach. This results in many experiments, which can be run in parallel and therefore reduce the wait time. However, the computational cost is higher, the resolution of the forcing space is discontinuous (and often sparse), and rate-induced effects can arise (Swierczek-Jereczek et al., in rev.). We are happy to mention this in the discussion.

15. Figure 3
   a. What are the associated f's corresponding to this color sequence in the middle and right columns?

We will include the values of f, which is constant (but different) for each row.

   b. Does time matter here? Why is that the x axis on the left plots?

As mentioned above, quasi-equilibrium experiments run forward in time, which allows studying the transient behaviour of the mass balance during a transition and helps understand what are the (autonomous) timescales of each transition. Hopefully this will be clarified by our improved description of the transient nature of quasi-equilibrium runs.

16. Lines 214-216.
   a. Please compare the numbers in this sentence to the values REF

We agree that this would ease the reading and will include these.

17. On word choice – bathymetry to my knowledge means topography below sea level, and you use this word to mean all subglacial topography to my knowledge. E.g. lines 260, 261, and various other places.

As suggested by reviewer #2, we will simply refer to this as basal topography.

18. Line 272.
   a. Do you specifically mean higher vertical shear stresses? Please be specific as to what component you are referring to.

Yes, we will add “vertical” for clarity.

19. Can you mention your perimeter to surface area metric in Figure C1 much earlier on? Such as around line 285? One of my comments would have been to show this as a quick metric of your perimeter feedback.
   a. Same for the paragraph starting in line 533. You make multiple references to information in the appendices in the second paragraph of the discussion section, while your audience may not have viewed this information. I would rewrite this section from the perspective of an audience that hasn’t read your appendices yet.

We mention the metrics for the first time at l. 326, where they become important to quantify the importance of MISI and the perimeter feedback. We believe that this is an appropriate place to introduce them. We agree that lines 533 and following might be confusing and will rephrase them.

20. 3.2.3 Marine perimeter feedback – I don't think the use of marine is necessary here. I would continue to call it the perimeter feedback and just say it's applied in the marine-terminating glacier setting.

In marine regions the ice sheet is additionally exposed to a much higher basal melt and calving along a very irregular shape (the coastline). All of these are important for the perimeter feedback but do not apply, for instance, to the merger of continental ice caps. We therefore see value in emphasising "marine".

21. Lines 435-436.
    a. Why are H94 and ATM not both plotted on the same figure 11?

We agree that they should be plotted together and will modify the figure accordingly.

22. Lines 445-446.
    a. Do ice stream shear margins (e.g. Minchew+ 2018) not play a substantial role in the near-equilibrium REF runs? There has been little mention of ice streams and ice stream shear margin evolution, and it would be interesting to read more about this. Is it a function of the forcing, the grid resolution, the prescribed rheology?

We will include a brief discussion of this along with our discussion of the resolution, since both issues are related.

23. Lines 447-449.
    a. I would provide a caveat for G20 in that the processes that you have updated, namely grounding zone intrusion and the regularized Coulomb were both not in as clear of a a consensus at the time of this work.

We will include this in the discussion.

24. Lines 457-459.
    a. Can you list out the ramping schemes of the previous studies for comparison? You have mentioned several, and either a table or a paragraph dedicated to this in the Comparison to Previous Studies subsubsection.

All previous ramping schemes simply consist in ramping up the forcing with a fixed rate, which we are happy to briefly mention in the revised manuscript.

    b. Likewise for line 467, please list the forcing that led to this bifurcation point, and feedback processes setting it

We believe that the forcing and the responsible feedback are clearly stated ("f > 7K", "dominated by the melt-elevation feedback").

5. Lines 519-521.
    a. You mention an albedo change due to WAIS collapse. Can you caveat or consider studies that would allow for sea ice to fill in this surface area?

Good point. This can only be represented in Earth System models (optionally of intermediate complexity) and we will add a brief discussion of this.

26. Line 558.
    a. “This is likely” is too strong given that it is speculative. I recommend “We hypothesize” or this may have.

This is an insight from the paper we are referring to (Albrecht et al., 2025) and not a hypothesis of ours. We will make this clearer.

27. Line 572.
    a. More specific than “these processes” and “by them”.
    b. Likewise – why not start this paragraph with 581 on these feedbacks triggering each other?

Thanks, we will explicitly refer to the processes and possibly rearrange this paragraph for clarity.

28. Lines 589-591.
    a. Please separate this into two sentences.

The sentences appear already separated to us.

29. Lines 611-613.
    a. Please split this into two sentences.

We agree and will do this in the revised version of the manuscript.

30. Lines 634-635.
    a. It seems relevant to refer to Figure 12 here.

Yes, we will refer to the figure in the revised version of the manuscript.

References
- Sergienko, O., & Haseloff, M. (2023). ‘Stable’ and ‘unstable’ are not useful descriptions of marine ice sheets in the Earth’s climate system. Journal of Glaciology, 69(277), 1483–1499. doi:10.1017/jog.2023.40
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- Whitehouse, P. L.: Glacial isostatic adjustment modelling: historical perspectives, recent advances, and future directions, Earth Surf. Dynam., 6, 401–429,

https://doi.org/10.5194/esurf-6-401-2018, 2018.
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Thanks for the additional references. We will include them in the revised version of the manuscript.