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.

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.
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.
   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.

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.

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.

    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.

e. Lines 324-326.

    i. To what extent are these results interpretable outside of equilibrium?

f. Lines 570-571.

    i. Please reference that these processes are identifiable on equilibrium timescales.

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.

b. Lines 79-80.

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

c. Line 81.

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

d. Lines 82.

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

e. Line 83.

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

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.

   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.

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).

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

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.

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.

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

6. On Policy Implications and Uncertainty Communication

a. Lines 152-153.

   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.

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.

c. Line 505.

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

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.)

      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?

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.
    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?
    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?

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.
    b. Missing the word loss in "ice volume loss".
2. Line 9.
    a. Either use "The merging of two ice caps", or "The merger event 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.
4. Line 16.
    a. Be more specific in which positive feedbacks.
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.
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.
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.

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.
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.
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.
11. Line 90.
   a. Only introduce the necessary number of new variables. You only need w and (1-w) if $0 <= w <= 1$.
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.
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.
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.
      i. Same with lines 144-145; I did not find this surprising. Can you explain why you would expect this to be striking?
      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?
15. Figure 3
   a. What are the associated f's corresponding to this color sequence in the middle and right columns?
   b. Does time matter here? Why is that the x axis on the left plots?
16. Lines 214-216.
   a. Please compare the numbers in this sentence to the values REF
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.
18. Line 272.

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

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.

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.

21. Lines 435-436.

   a. Why are H94 and ATM not both plotted on the same figure 11?

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?

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.

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.

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

25. 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?

26. Line 558.

   a. "This is likely" is too strong given that it is speculative. I recommend "We hypothesize" or this may have.

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?

28. Lines 589-591.

> a. Please separate this into two sentences.
29. Lines 611-613.
> a. Please split this into two sentences.
30. Lines 634-635.
> a. It seems relevant to refer to Figure 12 here.

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
- Minchew B.M., Meyer C.R., Robel A.A., Gudmundsson G.H., Simons M. Processes controlling the downstream evolution of ice rheology in glacier shear margins: case study on Rutford Ice Stream, West Antarctica. Journal of Glaciology. 2018;64(246):583-594. doi:10.1017/jog.2018.47
- 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.
- Moorman, R., Morrison, A. K., and McC Hogg, A. "Thermal Responses to Antarctic Ice Shelf Melt in an Eddy-Rich Global Ocean–Sea Ice Model". Journal of Climate. In: (Aug. 2020). doi: 10.1175/JCLI-D-19-0846.1.