Review 1:

We sincerely thank the reviewer for their useful comments. We will accordingly make revisions that will improve the clarity and presentation of the paper.

In the following, we respond to the reviewer (in red) point-by-point.

The authors explore tipping (both bifurcation- and rate-induced) in a three-dimensional model of the Greenland ice sheet coupled to a regional energy-moisture balance model. They find that the ice sheet will tip to a much lower ice state depending on the rate of the forcing and the overall level of the forcing. Considering the rate-induced transitions, they find that the time of tipping appears to relate in a non-monotonic way to the forcing. They conjecture the existence of an intermediate (chaotic edge) state or a boundary crisis to be the reason for this unpredictable time of tipping. Oscillatory behaviour of the system is also explained through the response of the Humboldt and Petermann glaciers and is removed to try to isolate the rate-induced tipping.

I have a couple comments regarding the presentation of the results.

1. Initially I was suspect of the chaotic behaviour claims in the paper. At the end of page 7 it is claimed that there is sensitive dependence on initial conditions, however this is not rigorously justified. Moreover, the model response is then continuously referred to as chaotic. Appendix B, however, has a much stronger argument for the conjecture of chaos in the system, including calculations which aim to verify (reject) the existence of a chaotic attractor (chaotic saddle). While I do not think all of this appendix should be moved into the main text, I strongly feel that some critical conclusions do need to be. In the first mention of chaos. I recommend adding a short discussion detailing your evidence and conjecture for the boundary crisis. Some of the more detailed discussions can still be left in the appendix.

Indeed, the justification behind describing the system as chaotic is perhaps somewhat naively based on the observed sensitive dependence on initial conditions. Of course, with only three initial conditions it is difficult to make a rigorous justification, and the stronger evidence is somewhat hidden in the appendix. We will move the argument laid out in appendix B to the discussion. We will remove the first mention of chaos on page 7 and move it to the discussion to maintain the flow of the paper.

2. The authors claim that there is no critical rate for rate-induced tipping. I find this very questionable. If the system does not tip when forcing is constant and then tips when it is not, there is some critical rate (albeit could be very small). Unless that the authors can prove that the system continues to tip as the rate limits to zero, I recommend softening such statements of no critical rates and acknowledging the critical rate could be smaller than the smallest value tested.

Indeed, there is no rigorous proof of the absence of rate-induced tipping, but the rates of forcing increase that are tested do cover the range of physically meaningful temperature increases. We will soften this statement by clarifying that there is no rate-dependent effect over the tested range, which covers all realistic rates of forcing.

3. The forcing in the model is not well described. I imagine it is some kind of regional temperature anomaly based on its units, but I could not find an explicit description or

expression. Additionally, the authors mention that the forcing increase is somehow adapted to equilibration time. This would be nice to see written out explicitly.

The forcing of the model occurs as an increase in the temperature of the atmosphere at sea level on the boundary of the atmosphere model REMBO. We will also describe the adaptive quasi-equilibrium function used to generate the bifurcation diagram in more detail.

4. In general section 2 need to be rewritten so that it is much clearer. I found it very hard to follow the introduction of parameters and governing equations. There is a lot of repetition where a parameter is defined mathematically and then introduced later for its physical interpretation. This can be easily fixed with a careful rephrasing.

We will revise section 2 so that the description of the model and its parameterizations are more clear.

5. The terms "tipping time" and "tipping point" seem to be used interchangeably in the text. This is a pedantic comment but important because often they mean different things. Choose one term and clearly state how you are defining it.

Indeed, these are different concepts and were not meant to be used interchangeably: the tipping point is a parameter value relevant in the equilibrium setting, and the tipping time is only relevant in the transient setting. We will correct the text to clarify.

6. On page 8 you mention the oscillatory behaviour for the first time. If there is oscillatory behaviour there could be existence of an unstable periodic orbit as a boundary that could potentially explain the irregular transition times which are perhaps more related to crossing this threshold. This also would strengthen your boundary crisis argument as the chaotic attractor can then collide with an unstable periodic orbit at the crisis.

We will include a description of unstable periodic orbits and how they relate to the boundary crisis in the discussion where we include the material from appendix B.

7. Figure 6 appeared to me to have "edge state" or chaotic saddle like dynamics. Again, perhaps a short discussion of the results in the appendix would be useful in the text to clarify why this is not believed to be the case.

We will reference this figure in the discussion where we include the material from appendix B.

Review 2:

We sincerely thank the reviewer for their useful comments. We will accordingly make revisions that will improve the clarity and presentation of the paper.

In the following, we respond to the reviewer (in red) point-by-point.

This paper investigates the conditions of emergence of tipping points of the Greenland ice sheet (GrIS). The work is based on an ensemble of simulations performed with an ice sheet model (YELMO) coupled to an energy balance-moisture model (REMBO). The runs consist in different warming scenarios with respect to the present-day period with various amplitudes of temperature anomalies and rates of change. The authors explore in particular b-tipping and r-tipping bifurcations and highlight the emergence of oscillations in the Humbold (HIS) and Petermann (PIS) ice streams (northwestern Greenland) which they attribute to chaotic variability leading potentially to a delay of the tipping time of the GrIS (large-scale ice loss). This study suggests that modest forcings may induce complex and unpredictable responses of the ice sheets. It represents an interesting and innovative contribution despite it remains difficult to see how it can be applied to the real world. The objective of the paper is clearly presented. The manuscript presents plausible processes but, unfortunately, not formally demonstrated. What is interesting are the numerous hypotheses/ideas put forward to interpret the results. These provide a new perspective on the behaviour of the climate-ice sheet system despite the simplicity of the atmospheric model. However, the results require a more in-depth analysis to be more convincing and some sections need to be further developed. This is why I recommend the publication of this paper after the authors have addressed a number of revisions listed below.

Major Comments

1. The dispersion of ice volume curves is interpreted as a signature of chaos (and this seems very plausible), but this has not been formally proven. I recommend therefore to provide additional figures showing visual evidence of the chaotic behaviour. These could be a plot of the ice volume trajectories (i.e. Vice(t+dt) vs Vice(t)) and/or a plot showing the dispersion around the mean of the simulated ice volumes (taking all simulations into account) as a function of time.

It is difficult to formally demonstrate the existence of chaos, so there is indeed no proof thereof. The strongest evidence of chaos is the scaling of the mean lifetimes of the chaotic transients which are outlined in the appendix. Part of this appendix will be moved to the main text to strengthen the argument that this is indeed chaos. In terms of other visualizations, plotting the mean ice volume of all the ensemble members as suggested is challenging due to the sporadic tipping of individual members. A plot of ice volume trajectories as suggested will be included, and comparing the runs at a forcing of 1.00 K versus those at larger forcing will be a good visual indicator of the different forms of variability.

2. There is evidence for r-tipping demonstrated with the collapse below the bifurcation threshold for certain ramping rates. However, it remains unclear whether a critical ramping rate exists or whether the tipping results from an internal chaotic variability. This issue is discussed later (Section 4.2), but I think it should be emphasized in the comments of Figures 5 and 6. One way to distinguish pure r-tipping from internal chaotic variability is to plot the final ice volume as a function of the ramping rate for a

given DT forcing. This would make it possible to determine whether tipping points occur abruptly beyond a certain threshold or, on the contrary, in a chaotic mode.

Plotting the final ice volume as a function of the rate, as suggested, gives no evidence of a threshold rate. We will include this figure as suggested so as to better

3. As I am not specialist of chaos dynamics, I found the Appendix B1 too technical. It took me a while to fully understand what you meant. The first part (lines 347-358) sounds like a lesson on non-linear dynamic systems and I regret that the explanations do not refer more to the physical systems under study, despite this is partially rectified in the second part. For example, I suggest to define what are the monostable or the bistable parameter regimes. By the way, some of these explanations could be moved in the main text. This would allow to clarify the link between local fluctuations and the large-scale ice loss.

We will improve the clarity of this section and more concretely relate the mathematical concepts to the physical system, as suggested. For example, the positive melt-elevation and ice-albedo feedbacks give rise to a range of atmospheric temperatures than can sustain both an ice-covered Greenland and an ice-free Greenland: this range of temperatures is the “bistable regime”. On the other hand, for large atmospheric temperatures, an ice-covered Greenland is no longer possible: this is the ”monostable regime”. There is another monostable regime corresponding to very low temperatures such that an ice-free Greenland is no longer possible.

4. I have also a couple of questions regarding the origin of the oscillations.

i/ How the initial states A, B and C differ? Are the differences due to the vertical temperature profile? sub-glacial topography, sub-glacial hydrology?

The initial states are generated by the same spinup simulation at different time steps. This is done to generate internally consistent ice-sheet states, as opposed to perturbing some variable(s) which may result in an unstable ice sheet. For this reason, all the fields are different. However, the magnitude of their differences are, by design, not so large, so as to represent similar ice sheets. We will include a figure which shows the difference between the surface altitude and surface velocities for all three combinations of two initial states. The greatest differences between these three states are in the surface velocities of the marine-terminating ice streams/glaciers along the coast, especially on the western coast. However, the differences in the region of the HIS and PIS, where the largest variability occurs, are not so large, even though this region is responsible for the largest amount of variability before a tipping event. This strengthens the suggestion that deviations in the trajectories of simulations for these initial states are sensitively dependent on the very small differences in these initial states in the region of chaotic variability.

ii/ You show that oscillations can be removed by lowering the basal velocities resulting in an increase of the basal friction. This raises questions regarding the sensitivity to the choice of the sliding law and this could be commented in Section 4.1 (see for example Brondex et al. 2017).

Indeed, the evolution of the ice sheet depends greatly on the basal friction parameterization, which is actually a major source of uncertainty in ice-sheet modelling. A discussion of this will be added to Section 4.1 as suggested, along with a discussion of other model configurations and parameterizations the behaviour may be sensitive to.

iii/ As oscillations are claimed to be due to thermomechanical coupling, a special attention may be given to the initial ice sheet topography. In this study, model initialization consists in a 400 ka spin-up experiment to arrive to an initial state corresponding to the present-day GrIS. Although such approaches are widely used, particularly for paleo experiments, they generally provide a too large GrIS compared to its actual present-day geometry (with the ice volume above flotation estimated at around 2.9 106 km3). In general, the longer the spin-up, the further the final state is moved away from observations. I acknowledge that the difference between the GrIS initial state and the present-day GrIS geometry is of secondary importance within the scope of the paper and does not question the main findings. However, I think it would be interesting to perform additional simulations to investigate the impact of the GrIS geometry on the chaotic fluctuations and add a dedicated comment in Section 4.1.

Indeed, the initial state used in this manuscript is not representative of the present-day GrIS, which is not in equilibrium. Rather, we are starting our simulations from an equilibrium state with the same external forcing as the present-day GrIS. This is necessary for constructing the bifurcation diagram of the GrIS (i.e. Fig. 2). The subsequent rate-induced tipping experiments must then be done in the context of this bifurcation diagram, so these simulations must also start from an equilibrium state. We will be more explicit in the description of the initial states of the GrIS used in the experiments. This also leads us to re-frame the results as being idealized and not a meaningful simulation of the future state of the GrIS. For this reason, we believe it is not necessary to include further simulations comparing the impact of different initial geometries.

1. How do you explain why HIS and PIS behave differently? Are these differences related to the geothermal heat flux, bed topography, basal velocity?

The geothermal heat flux in the PIS is slightly larger than in the HIS with a spatial average of 48.75 and 48.25 mW/m^2, respectively. During the periods where oscillations occur, the basal mass balance in the PIS varies much more than in the HIS, primarily due to the larger frictional heating (Q_b in equation 6) which is a direct result of larger basal velocities in the PIS while it is streaming (Fig. 8, panel g). The bed elevation of the HIS is lower than the PIS, leading to lower average basal friction and thereby making it more prone to the steady-streaming state as opposed to occasional refreezing. Finally, the HIS receives more precipitation than th ePIS on average, about 0.28 m.w.e/a compared to 0.26 m.w.e/a, respectively. This might allow for the mass lost by the HIS during streaming to be better balanced by the accumulation, as opposed to the PIS, where lower accumulation means the ice thickness can only regrow once the streaming has quiesced. An explanation of these differences, along with a figure, will be included in the article.

L180-181: The fact that the ice thickness variations of PIS are influenced by the oscillations of HIS in the unretreated configurations sounds a bit speculative. Can you provide other arguments in addition to the proximity of the ice streams? In the same way, they report an influence of PIS on HIS in the retreated case (L194-195). What is the coupling mechanism that would cause one of the oscillators to influence the other?

The main evidence that the PIS is influenced by the HIS in the unretreated configuration is the timing of the peaks in its mean ice thickness. These occur precisely when there is a peak in the mean ice thickness of the HIS, which in turn occurs when the basal velocity changes suddenly from slow to fast, ie. when the build-up phase is over and a surge occurs. Since the basal velocity in the PIS is relatively stable, it is not in the build-up/surge mode, and would not be expected to show periodic variability. The proposed coupling mechanism between the ice streams is that they both are fed by a common upstream source, which is supported by the fact that the oscillations of the HIS and the PIS are in-phase. If the ice streams are fed by a common source, a sudden surge in the HIS (corresponding to decreasing ice thickness) will "pull" mass from upstream and divert it from the PIS, which will cause it to lose mass as well.

In the retreated configuration, the same upstream coupling exists. Kypke et al. (https://arxiv.org/abs/2510.12525) outlines a reduced-order model of ice streams that are coupled in this way and show that if the two downstream ice-streams are oscillating, the variability of the entire system can be chaotic.

You mention oscillations in the HIS/PIS sector, but you do not specify whether similar oscillations have been reproduced by the model in other marine-terminating glaciers and ice streams. Should these oscillations be specific to the HIS/PIS ice complex only, can you explain why or provide suggestions? Have such oscillations been detected in the real world with satellite observations in other GrIS basins or in Antarctica?

While a lot of other, smaller marine-terminating ice streams found in the model output also oscillate, they have a much smaller amplitude and shorter period than the oscillations of the retreated HIS and the PIS. The HIS and PIS are also unique in that they impact the tipping behaviour of the ice sheet. The other oscillating ice streams are similar to the unretreated HIS and PIS, which is why these two configurations were used as a point of comparison. In a supplement, we will include some figures of other ice streams that are oscillating, specifically on the west coast, and compare their behaviour to the retreated HIS and PIS.

There are no direct observations of these types of ice-stream oscillations due to their timescale. However, there is evidence of ice streams which are both accelerating (ex. Catania et al 2012, Rignot et al 2022) and decelerating (ex. Catania et al 2006, Beem et al 2014) due to basal hydrology. These references will be added.

1. In the introduction, you point out the importance of the melt-elevation and temperature-albedo feedbacks. However, the results are never discussed as a function of these feedbacks. Another effect which has a significant influence through the melt-elevation feedback is the global isostatic adjustment (GIA). Owing to the relaxation time (3000 years) in the ELRA model and the time scales considered in this study, GIA should not be ignored in the analysis/discussion of the results.

The maximal uplift of the bedrock in both the HIS and the PIS is 40 metres, which occurs during a long surge event (between 230 and 260 ka in Panel c of Fig. 8). During this time, the change in ice thickness is about 600 metres for the HIS and 500 metres for the PIS. So, while the GIA is in play, the magnitude of the uplift of the bedrock is never so large as to affect the build-up/surge variability of the ice sheet. If the regrowth of the ice sheet after a surge were triggered by the GIA bringing the bedrock to an altitude that allows for a positive SMB, we may expect to see a period of minimal ice thickness during which the bedrock is uplifting, and at a certain altitude the ice thickness starts increasing again. Instead, we see the GIA simply lagging the ice thickness. The ice regrowth instead corresponds to a minimal basal velocity, i.e. an end of the surging. It may be the case that the GIA contributes to the maintaining of the oscillations by adding some altitude to the ice surface, but it does not set the pace of the oscillations.

2. The presentation of the method requires some additional details concerning the REMBO model, its performance and its resolution. Furthermore, I am not sure that all potential readers of the manuscript are familiar with the ITM method, and I believe it would be necessary to provide the equation linking the melt rate to insolation,

temperature and albedo. Finally, what downscaling method is used between REMBO and Yelmo?

A summary of the ITM method, which is described in the cited Robinson et al. 2010, will be added for clarity. We will also add additional descriptions of REMBO as well as the downscaling method in section 2.1.

3. In the abstract, you mention that ice stream oscillations may complicate the anticipation of ice-sheet tipping points. I guess you refer to near future projections. On the other hand, you explain in the conclusion that the consequences of chaotic transients on anthropogenic climate change are phenomenological rather sociologically relevant because of the longer time scales addressed in this study. Both statements seem to be a bit contradictory. My feeling is that this paper demonstrates that chaotic fluctuations may theoretically occur in ice sheets. However, it is difficult to conclude about potential implications on decadal to centennial (or multi-centennial time scales) without explaining how to reconcile both time scales. However, I would be very interested in having your comments on this issue.

Yes, the overall message is contradictory. This is a weakness in the framing of the manuscript, which will be alleviated by removing any references to ‘present-day’ and ‘future evolution of the’ GrIS. Instead, we will make clear that this is a modelling study of a hypothetical GrIS under idealized forcing, with the result that chaotic variability can affect the timing of the tipping of an ice sheet.

The limitations of the modelling approach are briefly mentioned in Section 5 and should be developed further developed either in the Conclusion section or in a dedicated section addressing the main sources of uncertainties. I have noted several points (see below) but there may be others of interest.

i/ The use of a simplified atmospheric model raises questions about its ability to represent properly the accumulation (and its variability) of small spatial scale regions (i.e. the HIS/PIS sector) and, therefore, the surface mass balance.

ii/ The absence of coupling with the ocean results undoubtedly in missing processes that are of key relevance for marine terminating glaciers (see for example Catania et al., 2020, Crowton et al., 2018). This is particularly necessary to mention this point as you refer to Alvarez-Solas and Bassis in Appendix A.

iii/ The resolution of the ice sheet model (16 km) prevents from an explicit representation of small-scale features such ice streams. This could be mentioned

I would like to point out that I am not denigrating your modelling approach, as I am aware that studies are conducted using the available numerical tools. However, I believe it is essential for the climate modelling community to clearly explain the framework within which the results are obtained.

We value the suggestion and will include a more thorough discussion of the limitations of the model.

Minor Comments

Several figures are quite difficult to examine (insufficiently detailed captions, lines too thin, colours too similar and font too small).

Figures 1, 2, 3, 5, 6, 7, 8: Increase label font size

Figure 1b caption: surface velocity instead of ice sheet extent

We will improve the readability of the figures.

Figure 2a does not resemble an “Equilibrium ice volume”. The term “Equilibrium” should be removed

Indeed, we have used equilibrium as a shorthand for ‘snapshot of the ice sheet on the stable attractor’. We will rectify the verbiage.

Figures 3, 7, 8: Increase the size of the colour scale

Fig. 6 Choose another set of colours to make more visible the different curves.

Fig. 7: Panels b and c show other marine-terminating glaciers that are not discussed in the manuscript. Could these maps be restricted to the same region as those displayed In Fig.8?

We will improve the readability of the figures.

Fig. 8 caption: Which is the time period considered for the temporal average

We will improve the clarity of the figure caption.

L18: Provide also more recent references (e.g. Wunderling et al., 2018)

We will include more recent references.

L43: forcings (instead of forcing)

L48: the ability of what?

We will change this to “the possibility of”.

L121: saddle-node bifurcation could here be related to melt-elevation and temperature-albedo feedbacks.

L131: Why 120 ramping experiments?

This is a mistake, it should be 35 ramping experiments per initial condition.

L135-136: Do the authors mean that tipping can occur for ramping rates below 10-5 K/yr. Did they conduct simulations with lower ramping rates?

Indeed, rates below 1e-5 were not tested, as the computational cost of simulations of this length are too great. Rather, there was no critical rate within those tested, which will be made more clear.

L139-142: The sentence seems to contradict the previous sentence (unless I missed something?).

Indeed, the sentence “However, there is no clear relationship between the tipping time and the rate of forcing.“ is contradictory and will be removed.

L153: I would say “during the first 25-50 ka of simulation time

Yes, for larger forcing values the initial mass loss ends much sooner than 80 ka.

L155: Refer here to Figure 7.

This reference will be added.

L184: “In contrast to the PIS” à “In contrast to the unretreated PIS”

This clarification will be added.

L185: “as the HIS” à “as the unretreated HIS”.

This clarification will be added.

L186: The oscillations are not displayed for the ice extent in panels (b) to (i) of Fig. 8. Replace “extent” with “thickness” or “volume”.

This mistake will be fixed.

L188-190: If the basal velocity increases, the mass loss should be accelerated. This contradicts the statement “This slows the mass loss”. Replace “slows” with “accelerates”.

This mistake will be fixed.

L189-190: This results from the thermomechanical coupling and could be reminded here.

This clarification will be added.

L199-202: The sentence is a bit long. Please split in two.

This mistake will be fixed.

L201: I did not find in Fig. 8 some features showing periods when both retreated HIS and retreated PIS are in a steady flow. Can you be more specific?

This was not seen in the simulations displayed in Figure 8. They will be included in an additional figure.

L208: lost à loss

This mistake will be fixed.

L216-219: Would the results/conclusion be the same if the forcings were between 1.05 and 1.30 K? There is no firm conclusion in section 3.4 concerning the role of the parameter delta.

Increasing the value of delta makes the ice sheet less dynamically active due to slower ice streams, so larger forcing is required to make it lose mass. This will be made more clear in the text.

L243: Please provide references and add comments related to the complexity of models used in other studies. I wonder whether the use of more complex climate and ice-sheet models would enable to reproduce a tipping of the ice sheet on shorter time scales.

Our model setup is of similar complexity to the other studies referenced in the discussion, and is about as complex as possibly beyond fully coupling to an Earth System model. The treason for the tipping occurring over such a long time scale compared to other studies is not due to lower model complexity, but rather because the system is only forced very close to the tipping point.

L241-242: I suggest that the fact that the occurrence of chaos in this study is an open debate should be mentioned earlier

Indeed, this will be amended.

L246: What is the amplitude of the oscillations in the present work? It is not mentioned in the text? See Major Comments

Indeed, this will be amended.

L247-248: Can you explain why the isostatic adjustment has a much larger role in the Zeitz et al's (2022) work? Maybe you could also discuss the differences with their results (possibly in terms of the differences in the experimental setups)

The ice sheet and GIA models used in these experiments are very similar to those of Zeitz et al., so it is possible to understand our work in the context of their experiments. The mantle viscosity used in Yelmo has a value of 1e21. The temperature lapse rate used in REMBO is 6.5 K/km. Finally, the temperature change in the ramping experiments is between 1.0 And 1.3. This puts our experiments into the parameter regime of Zeitz et al. where the ice sheet volume 'partially recovers' (their Figure 4), which is different to the results seen in the present study, where the ice sheet loses most of its mass at larger temperature changes. This is because our initial condition is an equilibrated GrIS at a lower ice volume than present-day, meaning lower temperature increases are required to tip the ice sheet.

L250: Could you add more recent references (e.g., Wood et al., 2021)

L251: "suggesting that…in these areas" should be justified

References to the sensitivity of marine-terminating glaciers will be included.

L253: Note that Joughin et al. (2010) reported more significant changes in southeastern glaciers than in northwestern glaciers due to differences in bed geometries

This discussion will be refined.

L282: "the value of the tipping point" à Do you mean here "the value of the forcing"?

Yes, we mean the value of the forcing that causes the GrIS to tip.

L296-298: I am not sure that all potential readers of the manuscript are familiar with the notions of edge-tracking algorithm (like me). Can you define please? Same for ghost attractor and chaotic saddle. Moreover, this is the first time you employ the terms "ghost attractor" and "chaotic saddle". In a previous comment (see the "Main comments" section), I recommended that part of Appendix B be moved to the main text and that the explanations of these terms refer to the physical system (and not be discussed in excessively theoretical terms).

We will move part of Appendix B, as noted, to the discussion, and will make more explicit references to the physical system. A description of an edge-tracking algorithm, as well as references to it, will be added to this section of the discussion.

L365-368: The sentence is too long. This is detrimental for the understanding. Split it in two parts. It would also benefit from a clearer explanation based on terms describing the GrIS and not only on chaos terminology.

We will enhance the clarity of the discussion as suggested.

L376: I am not sure to understand what you mean with "from the left"?

We mean "increasing the value of the forcing", the verbiage comes from two-dimensional bifurcation diagrams in mathematics where the parameter is on the horizontal axis. We apologize for the confusing terminology and write it in a more understandable way.

L378: What do the different parameters actually represent in Eq. B1?

Not including a description of the parameters was a mistake and will be amended.

L385: Remove the first occurrence of "either" and the term "otherwise"

L402: What do you mean with "diminished size"?

This sentence is incorrectly written and will be amended.

Review 3:

We sincerely thank the reviewer for their thorough review and helpful comments. The reviewer correctly points out flaws in the study which we believe can be rectified by a proper re-framing of the results.

In the following, we respond to the reviewer (in red).

The submission by Kypke examine the potentially significant role of ice stream cycling in the tipping point behaviour of a GRIS model. To avoid repetition, my review will largely focus on issues not yet raised by the other two reviewers.

major comments

Foremost, I can't adequately assess the significance of this submission given some missing critical information about the model configuration, and the lack of any analysis of numerical and input sensitivities in the model's simulation of ice stream cycling. For instance, in Hank et al (2023, https://doi.org/10.5194/gmd-16-5627-2023) we carried out a detailed analysis of simulated ice stream cycling response to grid resolution along with an assessment of approaches to minimize that sensitivity. Given the significant resolution sensitivity identified in that study (even between 6.25 and 3.125 km grid resolution), along with the challenge of accounting for the impact of the large km scale variation in basal topography around much of the GRIS ice margin, I'm skeptical of coarse resolution analysis of ice stream cycling dynamics without associated numerical assessment. At the very least, I would need to see a couple of ice stream cycling sensitivity experiments at 8 km grid resolution (twice as fine as current). Otherwise, I'm unclear of the extent to which the current results are just numerical artefacts.

We believe these comments indicate that we have made an error in the framing of the results of this paper. Rather than attempting to make claims about the realism of the physics of the oscillations or their actual likelihood to occur in the future, our goal was to describe their influence on the stability and collapse of the Greenland ice sheet (hereafter GrIS). As mentioned above, we will make clear that this is a modelling study of a hypothetical GrIS under idealized forcing, with the result that chaotic variability can affect the timing of the tipping of an ice sheet.

That being said, we believe it is out of the scope of this paper to perform simulations of the model at finer resolutions, or perform other sensitivity analyses. Rather, what is missing in the manuscript are major caveats explaining the limitations of our modelling approach, which will be added to the discussion to address the points raised by the reviewer: that the timing, scale, etc. of these surges are dependent on and sensitive to the model configuration (ex. resolution, geothermal heat flux BC, precipitation BC, etc.). That is, any mis-representation of the dynamics due to the numerical modelling considerations might change how often and at what thresholds these surges occur. However, their appearance alone is worthy of investigation and is the purpose of the present study.

The submission is also missing critical model configuration information, in particular that of the 2 km deep geothermal heat flux boundary condition and the details on the climate forcing (as raised by others). In that regard, given the strong impact of uncertainties in deep geothermal flux identified in Hank and Tarasov (2024, https://doi.org/10.5194/cp-20-2499-2024) for Hudson Strait cycling (for which one of the co-authors of the current submission, Alvarez-Solas, was a reviewer), this submission also needs a sensitivity assessment in response to those uncertainties. This sensitivity is not surprising, since geothermal heatflux will strongly impact the time required to reach the basal pressure melting point required for ice stream activation along with subsequent basal meltwater production. The lack of any reference to the above two papers are also examples of insufficient review of relevant literature. Other relevant papers not cited include Soucek and Martinec (2011, https://doi.org/10.3189/002214311798843278) and Sayag and Tziperman (2011, https://doi.org/10.1029/2010JF001839).

We will include a major caveat in the discussion that the geothermal heat flux is uncertain and the appearance of the oscillations are dependent on it. The focus of the paper should be not whether the oscillations are realistic, but how they affect the tipping of the GrIS if they do appear the way they do in our model. In addition, descriptions and figures of the boundary conditions for the geothermal heat flux will be included.

The precipitation aspect of the climate forcing will likely have the largest uncertainties. It also plays a major role not just in surface mass-balance, but also in vertical cold advection to the base with associated impact on basal temperatures. Given that the uncertainties in precipitation are not considered, there at least needs more details in the appendix or supplement about exactly what REMBO accounts for along with plots of a few precipitation map timeslices.

As with the geothermal heat flux, we will include a major caveat in the discussion that the precipitation fields are uncertain and highly relevant to the manifestation of the surging ice stream. A figure of precipitation fields in the region of interest has been included, but REMBO does not exhibit any major change in precipitation related to the oscillations other than those due to changes in surface elevation.

Given its pivotal role, the choice of basal drag representation and associated parameter choices will have a major impact on results. To justify the base value choice, there needs to be a present-day comparison of simulated surface velocities against those observed. The analysis needs more depth as to controls on the stream activation/quiesence. As an example a few basal temperature maps would convey the spatial range of basal temperatures proximal to the ice streams. For context, I'm a bit surprised with the amount of lateral velocity diffusion in the simulated ice streams. Is this due to most or all of the GRIS bed being close to the pressure melting point? It would also help to have the map plots made in non-fill (ie no graphical smoothing) mode to show the actual grid cells. The core take-away (subject to numerical verification) for me is further affirmation that ice dynamics (as opposed to just surface/marine mass-balance processes) can really matter in GRIS future evolution (though here on a relatively long time-scale herein). A possible elephant in the room for any detailed analysis of GRIS ice stream dynamics, especially in the context of chaotic or near chaotic response, is that even if these simulations were done at 4 km resolution, they would be at least 4 times too large to even partially resolve the dominant and extremely rough fjord valley/mountain scale of the GRIS margin. This is another consideration that is never discussed.

While the resolution is important for being able to resolve, for example, small fjords and thereby connectivity to the ocean which would apply a basal melt that might impact the ice streaming, it would not affect the chaotic behaviour that is observed, which is due to the ice dynamics and basal hydrology. In the retreated configuration, the HIS is never touching the ocean, and the PIS only occasionally extends to the beginning of the Petermann fjord, but never beyond, when it is at its maximum extent. Since this occurs after a streaming, the connectivity to the ocean is not contributing any additional basal melt to cause the streaming.

The chaotic mode is also at a larger temporal scale (longer period) and spatial scale (larger change in ice volume) than that of ice streams in the other parts of Greenland that exhibit similar oscillations, as it is seemingly dependent on the coupling of two such surging ice streams. This difference in behaviour can even be observed by comparing the HIS and PIS in the unretreated versus the retreated configuration, which was the main point of comparison in the manuscript. To provide additional and more clear evidence of the difference, we will compare the retreated HIS and PIS to other ice streams which display oscillatory behavior in the simulations, such as the Ilulissat Glacier on the west coast.

To more properly alleviate the issues in numerical modelling of ice sheets, and perhaps most critically to aid in the proper understanding of the contributions of this manuscript, we will remove the framing that this can occur in the "future GrIS". The initial states of our runs are a "branch" of the present-day GrIS that has been allowed to equilibrate, so that its inertia does not interfere with the subsequent ramping experiments. This allows a better analysis of the stability properties, but it makes the ice sheet configuration much less realistic and the results not subsequently not relevant to the near future of the GrIS. The references to 'present-day' and 'future evolution of the' GrIS will be removed, as this line of investigation is better suited to a different study which delves deeper into the sensitivity analysis to resolution, basal parameterization, etc.

specific comments

need surface velocity map comparison between a present-day simulation and that observed, eg as in Joughin et al (2018, https://tc.copernicus.org/articles/12/2211/2018/). Ice streams in particular are relevant, as they are characterized by sliding of ice at the base due to a till that is saturated with water.

We will include a comparison to present-day surface velocity maps as suggested.

The dominance of subglacial till deformation is not the case (unless past inferences have significantly changed) for Ilulissat Glacier, or does your definition of ice stream exclude this significant component of GRIS ice drainage?

This is an error in the definition and will be rectified.

The model is first run to equilibrium for 400 ka to arrive at an initial state (Fig. 1) corresponding to the present-day GrIS

This choice is never justified, I can see pros/cons, but at least it should be made clear this is not meant to represent the current (non-equilibrium) state of the GRIS, even though the converse is claimed.

Indeed, the initial state is not representative of the present-day GrIS, which is not in equilibrium, and claiming it was a mistake. Rather, we are starting our simulations from an equilibrium state with the same external forcing as the present-day GrIS. This is necessary for constructing the bifurcation diagram of the GrIS using the adaptive quasi-equilibrium function (i.e Figure 2). The subsequent rate-induced tipping experiments must then be done in the context of this bifurcation diagram, so these simulations must also start from an equilibrium state. We will be more explicit in the description of the initial states of the GrIS used in the experiments.

there is a geothermal heat flux boundary condition imposed 2 km below the surface

What is the specification for this boundary condition and how is this specific choice justified given the large uncertainties for this field. Aside from a sensitivity test, listing of simulated present-day basal temperatures at key ice core sites would provide some sense of how reasonable the chosen boundary condition is.

As described previously, by removing the framing of the results of this study being relevant for the future evolution of the GrIS, we will not require comparison of the initial states to the present-day.

Studies of oscillatory behaviour in ice sheets include parameterized models (Oerlemans, 1983; Fowler and Johnson, 1996; Payne, 1995; Robel et al., 2013) and comprehensive ice-sheet models with both idealized geometries (Calov et al., 2010; Van Pelt and Oerlemans, 2012; Feldmann and Levermann, 2017) and realistic topographies (Papa et al., 2006; Roberts et al., 230 2016; Schannwell et al., 2023).

insufficient review of relevant litterature

Additional references, including those provided, will be added.

The basal topography has insufficient colour contrast in all the map plots

figure 7 b,e and 8 a,j need geographic or km grid axes.

The figures will be improved for readability.

The conclusions are limited by the amounts and types of simulations conducted. An obvious next step is to repeat experiments using a different grid size to observe the dependence, if any, of these oscillations on the model domain.

This needs to be addressed in this study. I could see repeating the full set of experiments would be expensive, but at least a few tests would provide some hint as to what extent the current results may just be a numerical artefact.

We believe this line of investigation would be better suited to a different study which delves deeper into the sensitivity analysis to resolution, basal parameterization, etc. The oscillations may indeed be model dependent, but the fact that they influence the tipping is the focus of this work.

While the variability of the oscillations seen in this study seems similar to the build-up/surge variability 330 seen in most simulations of HEs in the Laurentide ice sheet (LIS) of the LGM (Calov et al., 2002; Papa et al., 2006; Roberts et al., 2016; Ziemen et al., 2019; Schannwell et al., 2023), they do not match exactly. Most notably, the Hudson ice stream in those studies displays a more gradual increase in ice volume followed by a sudden surge.

Hank and Tarasov (2024) should also be cited.

This reference will be added and the difference in the oscillations between these two studies will be included.