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. In addition, descriptions and figures of the precipitation fields will be included.

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.