Response to Referee#2 (John Erich Christian)

We would like to thank John Erich Christian for the careful reading of our manuscript, for the helpful comments and suggestions as well as for the constructive criticism. We are delighted by the Referee's overall positive assessment of our study and will address all the points made by the Referee in a revised version of the manuscript. Please find below the *Referee's comments in italics* and our response in blue.

Sincerely,
Johannes Feldmann et al.

Review of "Hysteresis of idealized, instability-prone outlet glaciers under variation of pinning-point buttressing", by Feldmann et al., 2024.

Summary

This manuscript addresses the stability of ice streams or outlet glaciers in the presence of buttressing via topographic pinning points. The authors investigate this by conducting numerical experiments on a variety of idealized ice sheet configurations, thereby highlighting the hysteresis of such systems and the dependencies on topography. While hysteresis and the effects of buttressing on grounding line-stability have been examined in different configurations, this study adds an important consideration which is the buttressing from isolated pinning points. As they show, this has some effects that appear qualitatively different from buttressing via confinement. As such, I think this study can make a useful contribution to our general understanding of ice sheet stability, in particular by considering an element of geometric complexity (pinning points) that is difficult to incorporate into simpler theoretical or 1-D frameworks. They do this using a comprehensive 2D model (PISM) in an idealized geometry and straightforward experimental protocol, so that results can be illustrated within conceptual frameworks (i.e., hysteresis loops). Overall, this approach is sound, the model used is well-suited, and the figures are clear.

However, I think there are some significant clarifications needed before publication. My major comments mainly deal with experimental choices that I think are incompletely explained, as well as the applicability to Thwaites glacier, which I feel is stretched (and somewhat inconsistent with the caveats the authors do mention). These are mainly issues of presentation and discussion, which I think are important to address, but they are not major issues with the experiments themselves. I also have a number of minor comments, which are mainly issues of clarity, and a few minor technical changes.

Major comments to the authors

1) Connection to Thwaites and overall framing: *You are careful to mention caveats in the discussion section, and highlight the idealized nature of the setup, but I still find the connections drawn to Thwaites and Pine Island glaciers strained. This is not to say that no insights can be drawn – for example raising Thwaites and Pine Island as examples where lateral confinement and thus the likely role of pinning points is different. However, given the significant difference of the simulated bed slopes, overall bed depth, size of pinning points, etc… I am not sure why Thwaites is highlighted as the main real-world example. For example, I think it is a stretch to have Thwaites as a highlighted implication at the end of the abstract, given the actual experimental geometries.*

I think the insights in your study might be better appreciated if it were framed as a more general, theoretical contribution to understanding ice sheet stability and hysteresis. For one example, I think there are interesting implications for how ice sheets expand from a collapsed state and the role pinning points may play in that. You touch on this a bit when describing the "growth" branch of the hysteresis curve, but you could potentially expand on it as a discussion point. Or, perhaps the geometries are more suitable for commenting on glacial/interglacial transitions across gentler continental shelf slopes? I am not saying you need to add these particular points, but I think diversifying the implications beyond current-day Thwaites might aid the overall robustness.

We see the Referee's point here and are willing to give a broader picture of the implications of our results that is less focused on Thwaites Glacier. We appreciate the related suggestions made by the Referee, which we will try to pick up in the revised version of the manuscript, including a more detailed discussion of the effect of GIA (and thus pinning-point uplift) on ice-sheet advance and to which geometries our simulations apply in the real world.

Hysteresis analyzed via pinning point size vs. environmental forcings. *I think more explanation and justification should be provided for this choice. I'm not suggesting it isn't a valid choice to look at hysteresis across parameter space rather than an environmental forcing, and I realize you address the difference in the discussion. But given the potential effects (which you acknowledge, especially regarding basal melt), I think more explanation is needed. The reader is left wondering why this choice was made.*

We recognize that our purpose of applying a synthetic and simplified perturbation compared to a more realistic perturbation needs more explanation. As suggested by the Referee, we will provide a more detailed discussion of the synthetic perturbation versus environmeltal forcing for a better justification of our approach.

Minor comments (line by line)

48: would be helpful to provide some info from the Gudmundsson reference – it is a potentially significant qualifier in the context of this study.

We will provide more detail on this study as suggested by the Referee.

49: "pinning points… vanishing" – ambiguous.. sounds like the topography is changing when I think you mean the pinning effect is vanishing as ice thins? Consider clarifying.

We will improve the wording according to the Referee's suggestion.

61: instead of "altering the buttressing strength", why not be more direct and say "by altering the amplitude of the pinning point"? There would be other ways to alter the buttressing strength of a given point (e.g., shelf thickness, rheology) so I think this would be clearer.

We will modify this phrase for a clearer wording.

64: "simulated similar ice-sheet-shelf" is a long string of descriptors for "systems"… consider rephrasing to clarify

We will simplify the wording following the Referee's suggestion.

65: "local" presumably refers to the GL? Perhaps clarify.

We will clarify.

Fig. 2 and Generally: The upper limit for pinning point depth is extremely shallow. Might want to flag that for readers.

We will highlight this point.

100: How is this elevation chosen a priori? Or do you mean this is chosen as the starting point for hysteresis experiments?

The initial elevation of the topographic high is chosen as the starting point for the hysteresis experiment. We will add more a detailed explanation to clarify.

119: what is meant by "fast" ice dynamics, and why is it needed here? Fast in a rate sense, or as in "landfast ice"?

We will rephrased the sentence for clarification.

154-155: "… ice shelf remains pinned on the topographic high. // Reversal of the perturbation leads to the re-grounding of the ice shelf …" … These two sentences seem contradictory. Does the ice shelf unground from the pinning point or not? Or does it depend on the experiment? This seems important as it implies some dependence on ice-shelf thickness after retreat, and therefore the boundary conditions and melt assumed (or lack of melt, in this case). In general, some more explanation of the mechanism of readvance might help the reader.

We will rephrase this section to avoid irritation. We will go into more detail regarding the mechanism of GL readvance, as suggested by the Referee.

159: I would suggest a word other than "forcing", since the hysteresis experiments are done over a more abstract parameter space of bed topography. Maybe simply "perturbation"

Will be changed.

188 (Flux balance analysis in general): A suggestion: it might be possible to consider these analyses for the confined case as well, using theoretical arguments for the buttressing factor as a function of shelf geometry.

Haseloff and Sergienko (2018, J Glac. doi:10.1017/jog.2018.30) derive such expressions that might allow you to expand this theoretical analysis to encompass more of your results.

Thanks for the suggestion! In fact, when designing our study, we considered applying the theory by Haseloff and Sergienko, 2018 as it indeed involves an ice shelf and two horizontal dimensions. However, their approach is restricted to a single flow direction (x direction) and parameterizes buttressing through prescribing a lateral drag at the side margins of the ice shelf, not taking into account potential buttressing emerging from a pinning point. In constrast, our simulations involve both horizontal flow directions (x and y) and buttressing emerges inherently without prescribing any stresses. This limits the applicability of their theory to our simulations. Hence we took back a larger step, introducing the laterally unconfined simulations and apply Schoofs theory to these simulations. Please also see our response to the next but one comment.

225-227: does this imply the whole domain ungrounds into a uniform floating shelf pinned on the topographic high?
Yes, exactly. We will add this information explicitely to the text.

256: I'm not sure I understand this argument. It is stated in the beginning of the paragraph that readvance can't occur under these circumstances. Is the point that the flux analysis suggests that a shallower bed would facilitate flux balance in the confined case and therefore explains why readvance can occur in some of those simulations? That is reasonable but just follows from the dependence of GL flux on bed depth, so I'm not sure what is gained by trying to connect the flux analysis on the unconfined case back to the confined cases. I do in general think the flux-balance arguments are helpful for building physical intuition and connecting to theory.. but I think this paragraph needs to be clarified.

The point we want to make with the flux-balance analysis is to show the influence of both the bed depth AND the pinning point buttressing on the re-advance of the GL. Since we cannot apply the theory to the case of two-dimensional horizontal flow (our laterally unconfined simulations), we conducted the quasi-flowline simulations which still involve the buttressing effect of the pinning point. This allows us to explicitely demonstrate how the pinning-point buttressing increasingly suppresses the GL flux in response to the lifting of the topographic high. The presented outcome might not be surprising for an expert on this topic but we are convinced that our illustration will be quite valuable to the broader, less specialized readership. In any case, we think the flux-balance analysis is worth to be shown in the paper, as to our knowledge, it has not been presented this way before (involving pinning-point buttressing). We will revise the manuscript for a clearer discussion of the capabilities/limitations of our application of the flux-balance analysis.

260: I think this summary sentence needs to be adjusted to better fit the analyses presented here. First, I don't think "rapid" should be used as it could be conflated with transient response which is not emphasized in these equilibrium hysteresis experiments. I'm assuming you mean a steep change with respect to the parameter space of D_th. Either way, I suggest clarifiying. Secondly, what is meant by "relatively small buttressing reduction" ? Relative to what?

We thank the Referee for the careful reading. We will revise the sentence, as suggested by the Referee, replacing "rapid" by "large-scale" and "relatively small" by "step-wise".

278: I'm not sure if the Schoof 2012 reference is most applicable here, (a) because as you note, it is the unbutressed case that Schoof 2012 analyzes. (b) in Schoof 2012 I think the case that allows stability on a reverse slope is when there is strong ablation near the grounding line, such that retreat can increase the integrated accumulation flux. If you are referring to a different result, maybe clarify – but it seems Gudmundsson et al (2012) or others investigating stability on reverse slopes with buttressing would be more germane to your analyses anyway.

We thank the Referee for pointing this out. Indeed, the reference to Schoof is not appropriated here. We

will follow the Referee's suggestion and compare our results to studies using setups that are more similar to ours.

349: I would say "suggest" rather than "highlight".. highlight to me implies the analyses focused on Thwaites in particular. But again, see major comment on relationship to Thwaites.
Will be changed.

Technical comments
- 38: comma after Shelf (".. ice shelf, to the two largest")
- 61: suggest "By altering…"
- 113: reduction/increase à reduction or increase
- 179 and on: suggest just writing out "first… second.. third.." etc. rather than 1), 2), 3).
- 227: it's à its
- 260: on a retrograde bed
- 290: it's à its
- 294: suggest "laterally weakly confined outlet glaciers" à "outlet glaciers with weak lateral confinement"
- 295: hysteretic/lock-in … recommend choosing one or rewording to avoid "/"
- 301: minimal-invasive à minimally invasive
- 302: in the real world
- 342: in the case

We are glad for the Referee's technical corrections and will implement them in the revised version of the manuscript.