

Response to the Editor

Dear Dr. Colleoni,

We would like to thank you for the careful handling of the review process. We are grateful for the valuable comments and suggestions from the referees as they really helped to improve our manuscript.

Having found an inconsistency in the code of the bed deformation model that partly explains the strong sensitivity of our results to the lithosphere thickness, which was mentioned by all three referees, we took the effort of repeating all model simulations with the corrected model code. As expected, the corrected results generally show lower tipping thresholds of ice-sheet collapse, in particular for the smallest lithosphere value, meaning that our results now are generally less sensitive to the lithosphere thickness than the previous results. Note that besides these quantitative changes, the qualitative picture of our results more or less stayed the same. In particular, the regimes of R-tipping occurrence remain largely the same, as well as the existence of GL overshoots and the occurrence of self-sustained oscillations.

Furthermore, we carried out all the additional simulations requested by the referees, providing deeper insights into the model response. The analysis and visualization of these new simulations help to better illustrate the physical mechanisms underlying the main results of our study (as requested by all three referees). In particular, part of the new simulations serve as a sanity check for the choice of the bed-deformation model configuration and the validity of the results in this study. All new findings are carefully discussed in the main manuscript or in the Supplement (see our point-to-point responses to the referee comments).

The revised manuscript now contains two more figures, i.e.,

- Fig. 5 (shifted from the Supplement here, as requested by two referees) and
- Fig. A3 (comparing grid resolutions, as requested by Referee#2), which is discussed in the new Appendix B

The Supplement now contains several more figures and one new section, i.e.,

- Figs. S2-S3: Equilibrium centerline profiles of the ice-sheet-shelf system and the underlying bed for two different spinup methods (as requested by Referee#2)
- Figs. S9-S10: Maps of bed and grounding-line evolution for the exemplary hysteresis loop shown in Fig. 4, also illustrating the occurrence of grounding-line overshoot.
- Figs. S11-S12: Maps of fully uplifted bed topography at the end of the forcing ramp-up for the two different initial bed-depression depths (as requested by Referee#1)
- Sect. 1: Details on the bed-deformation model domain and discussion of new perturbation experiments applying two different model domain sizes with accompanying new Figs. S13-S15 (addressing comments from Referee#1 and Referee#2)

Once again, we would like to acknowledge the work and the time the Editor and the referees put into the review process from which we think that it really enriched the manuscript. We hope that the Editor and the referees are content with our proposed revisions to the manuscript. Please find below the *Referees' comments in italics* and [our response in blue](#).

Sincerely,

Johannes Feldmann et al.

Referee#1

General comments

The authors investigate the role of vertical bedrock displacement on the marine ice-sheet instability. To this end, they sample various combinations of the lithospheric thickness and upper-mantle viscosity, and use an idealised setup that mimics some of the important features of Thwaites Glacier, which is likely to undergo a marine ice sheet instability in the coming centuries. By forcing the system with a very slowly evolving melt rate from the ocean, the authors assess the bifurcation point of the system. By using a forcing with significant rate, they simulate a collapse of the glacier below the bifurcation point, a situation also known as R-tipping. For some parameter combination, they observe a back and forth migration of the grounding line position across the unstable region at a constant forcing. This results in three main messages of the paper: varying the lithospheric thickness and upper-mantle viscosity can (1) shift the bifurcation point, (2) lead to R-tipping and (3) give rise to self-sustained oscillations.

The main messages conveyed by the authors are undoubtedly relevant for past and future marine ice-sheet evolution. The setup of lower complexity eases the understanding of the mechanisms at play and facilitates a clear description and visualization of the results. The text is easy to follow, the figures are nice and informative. It is obvious that the authors put great effort into the present work and I am sure that it will be a valuable contribution for the community.

However, I believe that a few important points need to be checked/modified before a possible publication.

We would like to thank Referee#1 for their willingness to review our manuscript, the many helpful comments and the constructive suggestions to improve our study. We are glad for the Referee's overall very positive assessment of our study. We prepared a revised version of the manuscript, addressing all the points raised by the Referee.

The authors find that the lithospheric thickness, T_e , plays a significant role in shifting the B- and R-tipping point. The explanation for this is found in Fig. S2, where it is shown that the uplifted bathymetry is very different for each choice of T_e . On many occasions, the authors mention that smaller values of T_e lead to a more localized response, but it really looks like the decisive factor is that the response is emphatically larger. For instance, the maximal bedrock uplift yields nearly 1,000 m for $T_e = 20$ km but only about 300 m for $T_e = 200$ km. Besides the fact that the authors should emphasize this more and move Fig. S2 to the main text, I am very surprised by this result: other studies have shown that, although T_e affect the gradient and rate of the displacement, it only marginally affects its equilibrium magnitude (Whitehouse et al., 2012; Milne et al., 2018; Coulon et al., 2021; Swierczek-Jereczek et al., 2024). I propose a simple way to verify this in the specific comments.

We thank the Referee for pointing to the significant role of the lithosphere thickness, T_e , in our simulations. To check for the validity of our results we reviewed the choice of our model

configuration and also the model code. Regarding the model configuration, we carried out test experiments which are similar to those suggested by the Referee (see Referee's point 6 below) and found that the chosen model setup is appropriate for the needs of this study (please see point 5 further below for more details). However, we found an inconsistency in the code of the bed deformation model in considering the ocean water load that indeed led to an overestimated response in bed uplift to the ungrounding of ice which partly explains the very high sensitivity of our results to the lithosphere thickness. Consequently, we took the effort of repeating all simulations with the corrected code. As expected, the corrected results generally show lower tipping thresholds of ice-sheet collapse, in particular for the smallest T_e value, meaning that our results now are generally less sensitive to the lithosphere thickness than the previous results. Note that besides these quantitative changes, the qualitative picture of our results more or less stayed the same (compare heatmaps [old Fig. S4 versus new Fig. S5] and hysteresis curves [old Figs. 5, S3 versus new Figs. 6, S4]) . In particular, the regimes of R-tipping occurrence remain largely the same, as well as the existence of GL overshoots and the occurrence of self-sustained oscillations.

Nevertheless, our results still show a clear dependency of the tipping threshold on the lithosphere thickness, which, based on the literature mentioned by the Referee, might be unexpected. However, we don't think that our results contradict the outcomes of the mentioned earlier studies due to three essential differences between our experiments and those from the mentioned studies which strongly limit comparability:

1. Three of four of the mentioned studies apply T_e values that are substantially larger than the 20 km lithosphere thickness for which we see the strong change in the response compared to the rest of our experiments. Investigated ranges in the mentioned studies are:
 - a. Whitehouse et al., 2012: T_e values ranging between 71 km and 120 km.
 - b. Milne et al., 2018: between 50 and 175 km
 - c. Swierczek-Jereczek et al., 2024: between 50 and 250 km

Only Coulon et al. 2021 cover the very low end of possible T_e values (applying a range from 10 to 40 km in West Antarctica). So three out of four of the above mentioned studies use a parameter range for which we only see a comparatively weak dependency of our results to the lithosphere thickness. Compare e.g., the weak variation in the inferred critical B-tipping thresholds ($m_{B\text{-tip}}$ between 1.3 and 3.0 m/yr) within the range $T_e = 80$ to 200 km which strongly deviate from the thresholds inferred for $T_e = 20$ km ($m_{B\text{-tip}}$ between 3.5 and 18 m/yr; see Figs. 7 and S5).

2. Except for the study by Coulon et al., 2021, the model simulations either prescribe an ice loading history (Whitehouse et al., 2012 and Milne et al., 2018) or introduce an ice body that remains fixed in time (Swierczek-Jereczek et al., 2024). Both approaches make the ice-bed interaction one-directional, i.e., the change in the ice load induces a solid-Earth response but this response does not feed back to the ice-sheet evolution and thus does not account for the interaction of feedback loops shown in Fig. 1 of our study. In our simulations it is this interaction that leads to the strong stabilization effect (or resilience) near a tipping threshold (multistable regime), particularly for small lithosphere thicknesses

due to the very localized bed uplift which can hamper ice-sheet retreat effectively (i.e. the retreat of the freely evolving GL). This is not possible in a model setup which prescribes the position/migration of the ice-sheet margin.

3. None of the four studies apply quasi-equilibrium conditions, i.e., prescribe an enormously slow forcing rate. Using different RCP forcings, as done in Coulon et al., 2021, naturally implies not only a dependence of the results on the response timescale associated with the viscosity of the solid-Earth structure but also on the applied forcing rate. In fact, for weaker forcing scenarios (lower forcing rates) Coulon et al., 2021 find that the influence of the lithosphere thickness increases and the influence of the viscosity timescale decreases, which is in accordance with our results.

We now discuss these aspects in the Discussion section (L. 492-523) to stress that our results do not contradict findings from previous studies. Moreover, we are convinced that the design of our experiments, which is substantially different from the above mentioned studies, actually helps reveal the potentially strong influence of the lithosphere thickness on the bifurcation point of a MISI-prone ice-sheet-shelf system. As suggested by the Referee, we moved Fig. S2 to the main manuscript (now Fig. 5) and emphasize the physical reasoning underlying our results (L. 250-260). For this purpose we now also show the strong difference in the horizontal extent and the magnitude of the response for a very thin and a very thick lithosphere in plan-view plots (new Sect. 1 added to the Supplement and Figs. S13 and S14). Please see our response to the Referee's point 5 for more details.

However, I believe that the strong dependence on T_e cannot be solely explained by the differences in uplift and the authors do not dwell into the details of this, which is a shortcoming that needs to be addressed. I suggest the following explanation. The bathymetry of the side walls is close to 0 around the coastal sill. Thus, any significant bed uplift suppresses basal melt there and provides lateral pinning points that greatly hinder the grounding-line retreat. Therefore, the role of T_e in shifting the bifurcation point so strongly might be largely due to the choice of the bathymetry. Given the large amount of work already provided by the authors, I don't think they should perform additional runs to investigate this. However, they should show heatmaps of the fully uplifted bathymetry (with a colour bar that changes rapidly around $z=0$, for various T_e , optionally in supplement, not only transects of the centreline) and critically discuss the fact that their setup is particularly sensitive to bedrock uplift because of the large pinning-point potential, right before the retrograde portion of the bedrock.

We find this an interesting speculation of the Referee. However, carrying out the Referee's suggested analysis does not confirm this guess as can be seen by the heatmaps we added to the Supplement (see new Figs. S11 and S12): the lateral grounded parts of the ice body that are situated on the side walls either remain grounded during the entire ramp-up (shallow initial bed depression, Fig. S11) or they unground partially in the course of the collapse (deep initial bed depression, Fig. S12). However, additional pinning points do not emerge: the very steep side walls and the limited bed uplift inside the bed trough (bed elevation stays well below zero there) prevent

the grounding line from migrating from the lateral ridges into the central trough.

In fact, the bed elevation inside the trough would have to increase to values close to zero (sea level height) in order to induce GL migration from the lateral ridges into the trough. For some ensemble members this indeed happens in the vicinity of the ice divide ($x=0$), causing GL overshoot: when the GL has retreated all the way to $x=0$ and the induced local bed uplift is strong enough such that the bed depth becomes very shallow there, the GL re-advances into the bed trough, producing GL overshoot (see our answer to the Referee's comment #9 further below).

Furthermore, the update frequency of the LC model is set to 10 years, although the authors infer a characteristic timescale of the solid-Earth of 7.5 years for $\eta = 10^{18}$ Pa s. I think the simulations involving low values of η need to be performed again with a higher update frequency, unless the authors can prove that this is not critical. Of course, the LC model is unconditionally stable since it uses an implicit time-stepping scheme, but numerical stability does not imply numerical accuracy.

This is indeed a shortcoming of the mentioned simulations. As suggested by the Referee, we repeated all experiments that use $\eta = 10^{18}$ Pa s, applying a sufficiently low update frequency of 1 yr (instead of 10 yr, now mentioned in the model description in L. 128-132). Note that this change only slightly affects the resulting hysteresis curves and tipping thresholds. The largest differences in the results are found for $T_e=20$ km but this is due to the fix in the code.

The authors use the case of a rigid Earth as a baseline to quantify R-tipping (L. 15-16, L. 250-258, Eq. 2, Fig. 6) and therefore compare setups with different physics (GIA on vs. GIA off). This is not the idea of R-tipping. However, it is easy to address by comparing simulations with the same parameters (T_e, η) but different rates, i.e. modifying Eq. 2 to:

$$\Delta m_{R\text{-tip}}(\eta, T_e) = (m_{R\text{-tip}}(\eta, T_e) - m_{B\text{-tip}}(\eta, T_e)) / m_{B\text{-tip}}(\eta, T_e) * 100\%$$

We see the advantage of the Referee's suggested quantification and thus changed Eq. 2, re-calculated the threshold shifts, and updated the heatmaps (Figs. 7 and S5, now with two separate colorbars for B-tipping and R-tipping, respectively) and the respective passages in the text. Note that we removed the ratio of B-tipping and R-tipping threshold shifts from Figs. 7 and S5 since with the new formulation of Eq. 2 we do not find this ratio to be meaningful anymore.

I am confident that the authors will be able to address these points, and I wish them the best of luck for the rest of the publication process.

Thank you so much!

Specific comments

1. The title focuses on R-tipping, but the paper also highlights how certain solid-Earth parameter choices can shift the bifurcation point or give rise to self-sustained oscillations.

Therefore, I feel like the title could be more general, something like "Solid-Earth structure conditions the stability of marine-terminating glaciers". But this is up to you.

We thank the Referee for this suggestion. Although our study indeed addresses more topics than R-tipping, we think that R-tipping is the most relevant one and thus would like to refrain from replacing the current title with a more general one.

- 2. Mentioning the large rate of the anthropogenic global warming compared to the past temperature record would be a nice complement for the motivation of studying R-tipping.*

We added a statement on that to the Introduction (L. 82-84).

- 3. L. 60-61: As mentioned above, T_e should not affect the vertical displacement significantly in the LC model.*

Please see our response to the associated points in the "General comments" section further above.

- 4. L. 69. You say that R-tipping remains largely unstudied in the MISI context, without any citation. However, in the discussion, you cite Swierczek-Jereczek et al. (2025), which has already suggested the timescale separation between ice and GIA to be responsible for R-tipping. This feels odd, especially because you therefore miss a nice motivation of your work in the introduction: to date, R-tipping of the AIS was studied, but never in a setup of lower complexity, where it is easier to identify and isolate the mechanisms. This is a simple but necessary fix to embed your work well in the state of the art.*

Thanks for pointing out this issue! We added the Referee's suggested statement to the Introduction (L. 86-102).

- 5. Your model description is really nice and easy to follow, but I feel like the GIA part misses some important information. What is the upper-mantle viscosity? Furthermore, to impose sensible boundary conditions, the LC model requires the ice-sheet domain to be smaller than the GIA domain. What is your choice here? Especially when it comes to the y-dimension, it is not an obvious choice since you assume periodic boundary conditions in the ice-sheet model which are difficult to consistently treat in the GIA model. From what I see in your plots, the vertical displacement is always very close to zero at $x = 800$ km, which makes me wonder if you chose the GIA domain to correspond exactly to the ice-sheet domain. Since I am not sure of this, the lines below are speculative.*

If you chose your GIA domain to be the same as your ice domain, it would be critical for the experiments with large T_e : since the displacement spreads significantly, it might be that $u_{\text{viscous}} \ll 0$ at $x = 800$ km. However, following Bueler et al. (2007), you would impose $u_{\text{viscous}} = 0$ there (on average). This might offset your displacement and be responsible for

the large changes in displacement magnitude depending on T_e , which what I mentioned in the general comments.

As suggested by the Referee, we added further details on the configuration of the LC model. This includes the size of the LC domain (the extent of the LC grid is 4 times the extent of the ice-sheet model domain in both horizontal dimensions) and the periodic boundary conditions (L. 158-168). There we also refer to experiments of instantaneous removal of the spun-up ice sheet to test the sensitivity of our results to the choice of the size of the LC model domain (including a configuration with a considerably larger LC grid, i.e, a grid size factor of 10 instead of 4). Note that these simulations are inspired by and very similar to those suggested by the Referee in point 6 below. The simulations are described and analyzed in Sect. 1 of the Supplement with new Figs. S13-S15 added. Based on the results we

1. show the bed displacement patterns resulting for the thinnest and the thickest lithosphere that differ strongly in horizontal extent and magnitude (Figs. S13 and S14),
 2. demonstrate that despite this spatial difference, the total spatially integrated bed displacement differs by less than 1 %, which acts as a sanity check for the LC model computation and the validity of the results in this study (Fig. S14), and
 3. show that the response in final equilibrium bed uplift deviates by less than 5 % between the two LC grid sizes, suggesting that our specific LC model configuration provides an appropriate compromise between model accuracy and computational cost (Fig. S15).
6. *To validate the setup with respect to this concern, I would like you to perform a standalone run of the GIA model with $T_e = 80$ km, assuming a rectangular ice load centred in $(x, y) = (0, 0)$, with $L_x = 1200$ km, $L_y = 320$ km, thickness = 1 km at $t = 0$ and remove the load instantaneously after. Please specify what is your GIA domain and upper-mantle viscosity so that I can verify that we obtain similar results.*

We followed the Referees suggestion to run such perturbation experiments in order to evaluate the LC model response in our setup (see our answer to point 5). However, we chose to start from the actual geometry of our setup, i.e., the initial ice-bed equilibrium on which our ensemble simulations are based on. This gives us results that directly apply to the used setup rather than to a more artificial (ice-block) configuration. We hope the Referee understands our decision.

7. *L. 143: Based on what you say here, I think it would be really nice if you would highlight the plausible solid-Earth parameter combinations in Figs. 5 & 6. This would ease the interpretation.*

Thanks for this suggestion! Done.

8. L. 173: *I feel like it's a pity to not show your results from branched-off simulations and step experiments in Fig 4.a and Fig. 5 (triangles and circles). This would allow the reader to see the results rather than a post-processed version of it. Please include this.*

We indeed also considered including these curves when preparing the manuscript but refrained from doing so as we found that the additional curves strongly reduce clarity of the figure. In fact, since the x-axis displays the forcing (and not time), both the branch-offs and the step experiments would simply introduce vertical lines to the graph, which in our opinion would distract from the core message of the figure (i.e., the hysteresis loop and the tipping thresholds + potential ranges). We hope the Referee understands our decision.

9. L. 206-208: *I understand that what you describe here is just a consequence of the differences in the fully uplifted bathymetries. Based on Fig. S2, it looks like the decisive factor here is whether the fully uplifted centreline bathymetry has any emerged part. Could you confirm/discuss/stress this? This also conditions the possibility of overshoot, which you mention at l. 329, but I think not with sufficient emphasis: the decisive role of GIA here is because it modulates the nonlinearity in basal melt ($= 0$ if $z > 0$; > 0 if $z \leq 0$).*

We cannot confirm the Referee's hypothesis here. That is, the bathymetry in the center does not necessarily have to lift up above sea level ($z=0$) to induce GL re-advance. This can be seen from Fig. 4b: the GL retreats completely to $x=0$ and while the ramp-up still continues and the local bed elevation is still below $z=0$, the GL re-advances. Since the centerline bed elevation is below zero at all times during the forcing ramp-up, the re-advance takes place under increasing sub-shelf melt rates, which rules out the possibility that the discontinuity in basal melt (which would be evoked by bed uplift above $z=0$) mentioned by the Referee plays a role here. We now also illustrate the re-advance by red circles and arrows on the left-hand-side of Fig. 4b, along with a brief explanation in the figure caption, to which we refer in L. 366-367).

To provide more detail we added two new figures to the Supplement (Figs. S9 and S10) which show the evolution of the bed topography field and GL position for the ramp-up and ramp-down of an exemplary ensemble member on the basis of which we discuss the overshoot mechanism (L. 364-372). Fig. S9h-p shows the GL re-advance from $x=0$ once the bed depth inside the bed trough is shallow enough to allow for the remnants of the ice body sitting on the lateral parts of the inland sill (with bed elevation always above zero) to re-ground into the channel.

10. L. 215: *Not sure that the ice shelves are the reason for the readvance: even without buttressing, you observe a similar behaviour, as shown by Schoof (2007); Pattyn et al. (2012).*

We agree with the Referee that in the two mentioned studies there is no buttressing involved. In both studies, the hysteresis of the ice sheet results from the response to a

step-wise increase and subsequent decrease in the ice softness. In our experiments the ice softness is held fixed. Our sole forcing parameter is ice-shelf basal melting which induces grounding-line migration through buttressing variations due to the change in ice-shelf thickness and size. The forcing only affects the ice shelf and thus it is the change in buttressing that induces ice-sheet migration and triggers the MISI (retreat and re-growth). Regarding the specific situation mentioned by the Referee: when the basal melt rates are ramped down, also here the only part of the ice-sheet-shelf system that is affected by the forcing is the ice shelf, which re-grows and thickens, increasing the buttressing effect, allowing the grounding line to re-advance and cross the retrograde bed slope eventually.

11. L. 339-340: *You should mention that this solid-Earth parameter combination is unlikely.*

Done (L. 400-401).

12. *In the discussion, you should mention that MISI can be stabilised by other mechanisms, e.g. upstream increase of precipitation (Sergienko, 2022). This is important, since in the present setup you prescribe constant accumulation and therefore miss this effect.*

We thank the Referee for bringing up this important study and finding which we added to the Discussion section (L. 639-642).

13. *Really neat parallel with binge-purge mechanism and nice link to Holocene readvance*

Thanks for this appreciative comment!

References:

Sergienko, 2022: No general stability conditions for marine ice-sheet grounding lines in the presence of feedbacks.

Whitehouse et al., 2012: A new glacial isostatic adjustment model for Antarctica: calibrated and tested using observations of relative sea-level change and present-day uplift rates: A new GIA model for Antarctica.

Milne et al., 2018: The influence of lateral Earth structure on glacial isostatic adjustment in Greenland.

Technical corrections

We thank the Referee for providing these corrections, which we incorporated in the revised version of the manuscript.

L. 1: “may be characterized by” → “will likely be subject to”

Done.

L. 2-5: This sentence is long, a bit confusing and would benefit from being split into two sentences. “West Antarctica” → “West-Antarctic Ice Sheet”; “most” → “more”.

Done.

L. 7-8: “basal ice-shelf melting” → “sub-shelf melting”

Done.

L. 8: “solid Earth structures” → “solid-Earth structures,”

Done.

L. 10: “B-tipping threshold” → “bifurcation point”

Done.

L. 14: “that for half of the ensemble members rate-induced tipping (R-tipping) occurs” → “that rate-induced tipping (R-tipping) occurs for half of the ensemble members”

Done.

L. 16: “effective critical tipping threshold” → “effective tipping threshold”

Done.

L. 27: mention a percentage of the WAIS contribution to the AIS ice loss.

Done.

L. 34: “van den Akker” is missing a hyperref. This applies to many of your citations throughout the paper, e.g. “Kachuck” (l. 53), “Fürst” (l. 83), “Pattyn” (l.86). This also applies to all the references you are making to figures (e.g. l. 41) and tables (e.g. l. 113). Please check this thoroughly.

We checked the hyperrefs of the mentioned studies which work in our PDF. We would like to leave the hyper-referencing of Figures and Tables to the typesetting phase.

L. 35-36: cite Swart and Li at the end of the sentence.

Done.

L. 38: “on bed” → “on a bed”

Done.

L. 41-43: “One important stabilizing factor in the MISI context is the buttressing effect

of ice shelves that are laterally confined or grounded on topographic highs" → "The buttressing provided by laterally confined ice shelves is an important stabilizing factor of the MISI".

Done (modified to also mention the topographic highs).

You often use the phrasing "MISI-type retreat", as in l. 49. I would simply write "MISI" in most cases, or "MISI-driven retreat". Also at l. 132.

Done.

L. 72: "for" → "of"

Done.

L. 117: "(Cornford et al., 2020, Eq. 3 of)" → "(Eq. 3 of Cornford et al., 2020)".

Done.

L. 127: "reaches equilibrium after several 10,000 model years" → just mention the exact number of model years.

Done.

L. 134-135: this is a good sanity check for your model setup but I am not sure if it needs to be mentioned in the paper. Up to you.

Removed.

L. 202: "does only occur" → "only occurs"

Done.

L. 209: "several 100 m" → "several hundreds of metres"

Done.

L. 228: delete "that could counteract GL retreat"

Done.

L. 240: "solid-Earth structure,"

Done.

L. 307: "10km" → "tens of kilometers"

Done.

Fig. 1: "GL retreat" → "GL retreat on retrograde bed"

Done.

Fig. 3: Would read better in kyr

Done.

Fig. 9: “reverse MISI” → “MISI”. The former sounds a bit odd to me, since it’s just MISI, following Schoof (2007).

Done.

Some references don’t include all the required information. For instance, the journal is missing for Adhikari et al. (2014) and some weird information (bandiera_abtest: a Cg_type, Group Subject_term, Subject_term_id...) is given for Adusumilli et al. (2020). Please check the whole reference section thoroughly.

Done.

January 31, 2026

The present work studies how the interaction between ice-sheet dynamics and the visco-elastic response of the solid Earth affects the tipping dynamics of marine-terminating ice sheets in idealised geometries. A bifurcation point is found via quasi-equilibrium forcing in ocean melt rates. If the forcing rates are increased, a glacier collapse occurs for melt rates below the bifurcation points for certain parameter combinations, thus illustrating R-tipping behaviour. The last message of the study exhibits self-sustained oscillations driven solely by the non-linear interaction between ice flow and solid Earth.

Overall, I find that the message of the paper is successfully conveyed. Idealised geometries are fundamental to isolate mechanism and understand the underlying physical processes. Results presented are clear and well structured, providing valuable conclusions for the scientific community.

Nevertheless, there are a number of major remarks that I would like the authors to address and further elaborate on the manuscript. Particularly, the work falls short in certain aspects that I have listed in the Major remarks section. Moreover, I have included a list with other minor remarks at the end of the document.

We would like to thank Referee#2 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 addressed the points made by the Referee in a revised version of the manuscript.

1 Major remarks

Motivation.

Line 69 in the introduction reads: "While the concept of R-tipping has been examined for other tipping elements [...], to date it remains largely unstudied in the MISI context".

However, there are previous studies that have already explored the idea that timescale separation between solid-Earth and ice sheets permits R-tipping. For instance, Swierczek-Jereczek et al. (2025): "Here we propose that sectors subject to MISI can undergo R-tipping: when forcing rates are high and the bedrock uplift is slow, the grounding-zone retreat is not stabilised as efficiently as in equilibrium simulations, triggering MISI at warming levels below the bifurcation point". Currently, the manuscript under review is motivated by the apparent absence of previous studies where MISI is investigated within the context of R-tipping. Instead, I would suggest framing the motivation in terms of a lack

of experiments in idealised geometries where physical mechanisms can be isolated.

[DONE] We thank the Referee for this valuable suggestion and revised this part of the Introduction accordingly (L. 82-102).

Spin-up.

According to Section 2.2.2, bed deformation is switched off during spin-up and is later justified by: "[...] enabling bed deformation after the system has equilibrated does not lead to any changes in the ice and bed geometries". Why is this the case? I guess the GIA model is set up to reflect load changes referring to the ice thickness equilibrated with a fixed bed. I understand that this allows simulations to start from the same grounding line position, but it is only one choice and should be detailed in the text.

I propose an additional approach to potentially enrich the discussion: performing the spin-up with bed deformation. Equilibrium grounding lines will therefore be different for each combination of T_e and η . It is quite informative to observe the equilibrated ice sheet profiles with their corresponding relaxed bed geometry (analogous to Fig. 4b). This will give an idea of how far the grounding line position with a fixed geometry lies from the GIA counterpart. The spin-up could start from a 2 km block of ice (as currently performed) or perhaps from a thin layer of ice that grows until equilibrium (Pattyn et al, 2012). In total, this would only introduce 32 additional simulations, feasible considering this idealised domain.

We followed the Referee's suggestion and carried out 32 spinups that account for bed deformation, leaving the rest of the model configuration identical to the original fixed-bed spinups. We compare the centerline profiles of ice and bed resulting from this alternative spinup procedure to the original one in Supplementary figures S2 and S3 and added a discussion in L. 584-600 of the Discussion section.

Lithospheric thickness T_e .

I find striking the steady-state bed displacement sensitivity to lithospheric thickness. To my understanding, this parameter mainly dictates the horizontal extent and the transient response of the vertical displacement, thus only driving minor changes in the steady-state displacement (Coulon et al., 2021; Swierczek-Jereczek et al., 2025). Figure S2 shows the contrary: the equilibrium displacement is apparently dictated by T_e . Why is this? I encourage the authors to elaborate on such behaviour, seemingly contradicting previous studies. The consequences of this behaviour are notable, since the reported R-tipping is therefore highly sensitive to T_e . I would suggest moving Fig. S2 to the main text since the ultimate physical mechanism is therein illustrated.

The strong sensitivity of our results to the lithosphere thickness is indeed very notable and we thus carefully checked our results and revised the manuscript, providing deeper insight into the model response and a more thorough discussion of our findings. To check for the validity of our results we reviewed the choice of our model configuration and also the

model code. Regarding the model configuration, we carried out test experiments which are similar to those suggested by the Referee#1 and found that the chosen model setup is appropriate for the needs of this study (see new Sect. 1 added to the Supplement and Figs. S13-S15). However, we found an inconsistency in the code of the bed deformation model in considering the ocean water load that indeed led to an overestimated response in bed uplift to the ungrounding of ice which partly explains the very high sensitivity of our results to the lithosphere thickness. Consequently, we took the effort of repeating all simulations with the corrected code. As expected, the corrected results generally show lower tipping thresholds of ice-sheet collapse, in particular for the smallest T_e value, meaning that our results now are generally less sensitive to the lithosphere thickness than the previous results. Note that besides these quantitative changes, the qualitative picture of our results more or less stayed the same (compare heatmaps [old Fig. S4 versus new Fig. S5] and hysteresis curves [old Figs. 5, S3 versus new Figs. 6, S4]). In particular, the regimes of R-tipping occurrence remain largely the same, as well as the existence of GL overshoots and the occurrence of self-sustained oscillations.

Nevertheless, our results still show a clear dependency of the tipping threshold on the lithosphere thickness. However, we don't think that our results contradict the outcomes of the studies mentioned by the Referee due to essential differences between our experiments and those from the mentioned studies which strongly limit comparability: Note that we aim for quasi-equilibrium conditions in our simulations, i.e., we apply an extremely slow forcing rate, allowing us infer the system's bistability regimes and potential R-tipping ranges. This is fundamentally different compared to the approach used in Coulon et al., 2021 (mentioned by the Referee above), who apply different RCP forcings to make sea-level projections. The use of such forcing scenarios (with forcing rates orders of magnitude larger than the rates prescribed in our study) naturally not only implies a dependency of the results on the response timescale associated with the viscosity of the solid-Earth structure but also on the applied forcing rate itself. In fact, for weaker forcing scenarios (lower forcing rates) Coulon et al., 2021 find that the influence of the lithosphere thickness increases and the influence of the viscosity timescale decreases, which is in accordance with our results. Regarding the other study mentioned by the Referee, as far as we know, the lithosphere thickness T_e is not varied in Swierczek-Jereczek et al., 2025, i.e., they use the same spatially variable T_e field in all of their simulations and thus do not make statements on the influence of T_e in their results.

We now discuss these aspects in the Discussion section, along with the discussion of further studies mentioned in the same context by Referee#1 (L. 492-523). Furthermore, as suggested by the Referee, we moved Fig. S2 to the main manuscript (now Fig. 5) and emphasize the physical reasoning underlying our results (L. 250-260). For this purpose we now also show and describe the strong difference in the horizontal extent and the magnitude of the response for a very thin and a very thick lithosphere in plan-view plots (Sect. 1 and Figs. S13 and S14 in the Supplement).

Shift in the critical melt rate threshold.

The study defines the shift relative to the fixed-bedrock geometry $mB\text{-tip, fixed}$ (Eq. 2 and Fig. 6). Nevertheless, one must look at simulations with identical parameter choice to illustrate the contribution of forcing rates (i.e., T_e and η). Hence, Eq. 2 must be modified to reflect that the threshold shift is compared between simulation with identical GIA parameter values and different melt rates:

$$\Delta mR\text{-tip}(T_e, \eta) = (mR\text{-tip}(T_e, \eta) - mB\text{-tip}(T_e, \eta)) / mB\text{-tip}(T_e, \eta) \quad (1)$$

With this new definition, the threshold shift only reflects the contribution of the rate of forcing, rather than the influence of considering bedrock displacement.

We see the advantage of the Referee's suggested quantification and thus changed Eq. 2, re-calculated the threshold shifts, and updated the heatmaps (Figs. 7 and S5, now with two separate colorbars for B-tipping and R-tipping, respectively) and the respective passages in the text. Note that we removed the ratio of B-tipping and R-tipping threshold shifts from Figs. 7 and S5 since with the new formulation of Eq. 2 we do not find this ratio to be meaningful anymore.

GIA domain and boundary conditions.

The horizontal scale of the bed displacement might reach beyond the edges of the domain. If boundary conditions impose a null vertical displacement at the edges, it would introduce a bias (particularly relevant at large T_e). Ideally, the vertical displacement should vanish near the edges to avoid this bias. I suggest an easy test to quantify this potential error by simply evaluating the horizontal gradient of the vertical displacement. If these are significantly non-zero as the edge is approached, the GIA domain definition should be extended further, specially along the y -axis. If boundary conditions are different (e.g., Robin-type $\partial u / \partial n + u = 0$), the choice should be justified and detailed in the text.

This is indeed an important point brought up by the Referee. In our simulations the extent of the GIA domain is 4 times the extent of the ice-sheet model domain in both horizontal dimensions. The GIA model applies periodic boundary conditions at the edges of the GIA domain. Following the Referee's suggestion, we found that the vertical displacement does not vanish at the GIA domain boundaries in the case of a very thick lithosphere ($T_e=200\text{km}$). We carried out test experiments (as suggested by Referee#1) with a larger GIA model domain (factor 10 instead of 4) for which the vertical displacement vanishes at the domain boundary also for $T_e=200\text{km}$ (compare new Figs. S13 and S14). However, we find that the deviation in the response in bed uplift between the two GIA model domain sizes is 5 % at maximum (located in the setup center, see new Fig. S15), suggesting that the size of our original GIA model configuration is sufficient, while reducing the computational cost of the simulations. We describe these findings in more detail in newly added Sect. 1 on page 14 of the Supplement. We added the missing details on the GIA model domain and the boundary conditions to the model description where we also briefly discuss the influence of the GIA

model domain on our results (L. 158-168).

Additional stabilizing feedbacks.

The are additional stabilizing feedbacks that could be discussed in the manuscript. For example, heterogeneous precipitation (Fig. 2 in Sergienko, 2022) and the temperature dependency of ice viscosity (Fig. 6 in Moreno-Parada et al., 2025). Sergienko (2022) demonstrated that the feed-back between precipitation, atmospheric surface temperature and ice-sheet surface elevation provides additional stability. Moreno-Parada et al. (2025) showed that the bistability of the system is increased (i.e., a widen hysteresis loop) if the modelled ice temperature and viscosity are fully coupled, due to velocity corrections as the thermal structure adjusts to the changing ice thickness. Both mechanisms are absent in this study, since ice softness and surface accumulation are assumed to be constant.

We thank the Referee for pointing us to these important stabilizing feedbacks that we missed to discuss in our study. We added a brief discussion of both effects in the revised version of the manuscript (L. 635-642). Note that we removed a speculative half sentence in the beginning of the same paragraph.

Grid resolution.

All simulations are run with $\Delta x = 2$ km following Feldmann and Levermann (2023). Have you performed a convergence analysis where bedrock displacement is included? Given that the model domain is idealised, higher resolutions are feasible. I would suggest taking two parameter choices of the pair T_e and η (corresponding to the presence and absence of R-tipping behaviour) to perform a number of simulations for, at least, $\Delta x = 0.5, 1, 4$ km. This would be ideal to compare both the hysteresis loops and the forcing rates exhibiting R-tipping as a function of Δx , ultimately justifying (or not) the resolution choice if bedrock displacement is considered. The convergence test will further provide robustness to the results.

As suggested by the Referee, we conducted a small grid-resolution study, choosing the two representative (η, T_e) -pairs ($1e+21$ Pa s, 80 km) and ($5e+19$ Pa s, 80 km) – the former exhibiting R-tipping, the latter not. The results show that the resolution of course has an influence but that results on the 2-km grid are quite close to the 1-km results, quantitatively as well as qualitatively, i.e., in terms of B-tipping thresholds, (non-)occurrence of R-tipping and the R-tipping range. We now show these results in the new section B of the Appendix, introducing Fig. A3 and including a brief discussion (L. 707-718, referred to in L. 117-119). Note that conducting the whole hysteresis loop is relatively costly in terms of computational time already for the 2-km simulations and thus we are not able to provide results for the Referee's suggested grid size of 0.5 km.

2 Minor comments

For the self-sustained oscillations, it would be informative to estimate if a parameter combination of η and T_e characterizing the Laurentide Ice Sheet gives rise to such an oscillatory behaviour.

This is indeed an interesting comparison. The parameter ranges we found in the literature (Zhao, 2013, Argus et al., 2021) are $T_e=100-180$ km, $\eta=0.2-0.5 \cdot 10^{21}$ Pa s, thus not fitting the parameter combinations of our simulations that lead to self-sustained oscillations. We added a statement on that in the Discussion (L. 570-572).

The caption of Figure 8 should read: "The two dotted horizontal lines mark the range of retrograde bed slope for a fixed bed."

Done.

Figure 9: "reverse MISI ". I would simply use MISI. The instability does not imply a specific direction, but rather to the absence of stable equilibrium positions over retrograde slopes (Schoof, 2007).

Done.

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Moreno-Parada, D., Robinson, A., Montoya, M., and Alvarez-Solas, J.: Description and validation of the ice-sheet model Nix v1.0, *Geosci. Model Dev.*, 18, 3895–3919, <https://doi.org/10.5194/gmd-18-3895-2025>, 2025.

Referee#3

The study presents results from idealized experiments designed to investigate the effect of bed deformation on tipping points in a controlled and systematic framework. The authors demonstrate that bed deformation can significantly increase the timescale over which tipping occurs and can modify the critical thresholds for collapse. By isolating the interaction between ice dynamics and visco-elastic Earth response, the experiments provide a clearer mechanistic understanding of how solid-Earth processes influence marine ice sheet instability. These experiments therefore constitute a valuable contribution to the field, particularly in advancing conceptual understanding of tipping dynamics beyond complex projection-based simulations.

I believe the manuscript requires improvement in the introduction and in the presentation of the results. In addition, the conclusions regarding the importance of idealised experiments should be clarified. I elaborate on these points below and provide detailed, line-by-line comments for the introduction and methods sections.

We would like to thank Referee#3 for their willingness to review our manuscript, the helpful comments and the constructive suggestions to improve our study. We are glad for the Referee's overall very positive assessment of our study. We prepared a revised version of the manuscript, addressing the points raised by the Referee.

General comments

The Introduction lacks a discussion of what the current literature suggests regarding the impact of bedrock uplift on tipping of the Antarctic Ice Sheet. For example, previous studies have shown that including bedrock deformation can delay the collapse of the West Antarctic Ice Sheet by more than a century compared to simulations using a rigid Earth. I would expect a more in-depth discussion of what is currently known about the influence of bedrock uplift on Antarctic tipping dynamics.

I consider this study relevant, and the use of an idealized setup provides new insights. However, the rationale for this idealized framework is not clearly articulated in the Introduction. The manuscript does not sufficiently explain why studying tipping dynamics in an idealized setting is necessary to answer the research questions. The authors should more explicitly describe which aspects remain unresolved by existing literature and why existing studies based on realistic Antarctic projections do not address their research questions. A clearer justification for choosing an idealized approach would significantly strengthen the motivation of the study.

We thank the Referee for the comments and suggestions on improving the Introduction of our manuscript. As suggested by the Referee, we now provide a deeper discussion of the available knowledge about (West) Antarctic bed uplift and tipping dynamics in the Introduction (L. 64-74). As also proposed by the Referee, we now better underline the strength and justify our choice of using an idealized approach in contrast to more realistic representations of the Antarctic Ice Sheet dynamics (L. 82-102, L. 429-440).

Section 3.1 presents general interactions inferred from the results without first providing sufficient quantitative descriptions of those results. For example, lines 193–194 conclude that, without bed deformation, the grounding line (GL) remains stable for low forcing magnitudes. I would expect this statement to be preceded by a description of the results specifying the forcing magnitudes for which the GL remains stable and the melt rates that trigger retreat. Similarly, lines 194–197 draw conclusions about simulations that include bed deformation, but these results have not yet been described in detail. I would first expect a quantitative comparison of bedrock uplift across the different Earth structures, followed by a clear explanation of how these differences influence ice-sheet retreat. Although some of this information appears in the second paragraph of the section, it is again presented in general terms. For instance, lines 205–206 mention that some simulations exhibit a grounding-line overshoot, but it is not explicitly stated which simulations show this behavior or which figures illustrate it. I believe this entire section should be revised to include more explicit, quantitative descriptions of the results, clearly linked to specific simulations and figures before broader interpretations are drawn.

Revising Sect. 3.1, we added more detail and numbers on the forcing magnitudes and now also provide clearer references to the figures which illustrate our statements, as requested by the Referee (L. 235–236, L. 237–238, L. 240–242). However, we think that it is a matter of taste whether the results are presented in the way we do, i.e. starting from the general picture in Sect. 3.1 to provide details and more quantification regarding tipping thresholds and GL overshoot in the following Sections 3.2, 3.4 and 3.5 (to which we refer explicitly in L. 241–242 and L. 262), or whether the results are presented the other way around (as suggested by the Referee). Since we think that the current overall structure of the section supports the readability, we would like to refrain from changing it and hope for the understanding of the Referee.

Coulon et al. (2021) show the minimal impact of lithospheric thickness on steady state displacement. The authors should further clarify why they found this large impact of lithospheric thickness in the presented results.

The strong sensitivity of our results to the lithosphere thickness is indeed very notable and we thus carefully checked our results and revised the manuscript, providing deeper insight into the model response and a more thorough discussion of our findings. To check for the validity of our results we reviewed the choice of our model configuration and also the model code. Regarding the model configuration, we carried out test experiments which are similar to those suggested by the Referee#1 and found that the chosen model setup is appropriate for the needs of this study (see new Sect. 1 added to the Supplement and Figs. S13–S15). However, we found an inconsistency in the code of the bed deformation model in considering the ocean water load that indeed led to an overestimated response in bed uplift to the ungrounding of ice which partly explains the very high sensitivity of our results to the lithosphere thickness. Consequently, we took the effort of repeating all simulations with the corrected code. As expected, the corrected results generally show lower tipping thresholds of ice-sheet collapse, in particular for the smallest T_e value, meaning that our results now are generally less sensitive to the lithosphere thickness than the previous results. Note that besides these quantitative changes, the qualitative picture of our results more or less stayed the same (compare heatmaps [old Fig. S4 versus new Fig. S5] and hysteresis curves [old Figs. 5, S3

versus new Figs. 6, S4]) . In particular, the regimes of R-tipping occurrence remain largely the same, as well as the existence of GL overshoots and the occurrence of self-sustained oscillations.

Nevertheless, our results still show a clear dependency of the tipping threshold on the lithosphere thickness. We now discuss these aspects in the Discussion section in the context of previous modeling studies (L. 492-523) and give a more thorough analysis of the underlying physical mechanism (L. 250-260, also moving Fig. S2 to the main manuscript; now Fig. 5). For this purpose we also illustrate the strong difference in the horizontal extent and the magnitude of the response for a very thin and a very thick lithosphere in plan-view plots (see newly added Sect. 1 and Figs. S13 and S14 in the Supplement).

The discussion section frequently refers to realistic conditions but does not provide a comparison with results from realistic Antarctic Ice Sheet projections. The authors argue in lines 365–380 that large uplift rates generate a strong GIA feedback, as well as in the first lines of the conclusions, but this stabilizing role of GIA has already been demonstrated in projection-based studies which quantified the delay in grounding-line retreat and ice-sheet collapse associated with bedrock deformation (e.g. Gomez et al., 2024; Han et al., 2025; van Calcar et al., 2025). They also show that at Thwaites basin, the upper mantle viscosity varies with an order of magnitude between the grounding line and the edge of the basin inland. This would also impact the bedrock uplift on the scale of the idealized setup presented in this manuscript. Since the manuscript compares their results to realistic situations, the authors should also provide a comparison between the idealised results and these realistic results in terms of bedrock uplift and grounding line delay.

Such a comparison would indeed be very interesting. However, the essentially different approaches underlying the studies mentioned by the Referee on the one hand and our study on the other hand prevent such comparison. That is, the above mentioned studies make sea-level projections based on observed present-day forcing rates, but we infer tipping thresholds based on a very slow synthetic forcing to ensure quasi-equilibrium conditions required to infer the bistability regimes of the modeled system. We added a statement to the Discussion section regarding the possibilities of the approach we use in our study compared to previous studies (L. 432-440).

Line by line comments

L8: Please refer shortly that the LC model is used and with which viscosity range.

As suggested by the Referee, we refer to the LC model now in the abstract (L. 9). However, we would like to have the details, including the viscosity range, mentioned in Sect. 2 in order to keep the abstract concise and hope for the understanding of the Referee.

L16: The tipping threshold reduces compared to which reference state?

The reference is the B-tipping threshold and we added this information (L. 18-20).

L17: What is meant by “fast timescale”? Could you provide some information in the beginning of the abstract on the timescale of your simulations?

Besides the forcing timescale (L. 8-9), we now provide numbers on the timescale of the viscous solid-Earth response in L. 12 of the abstract. We also reworded the mentioned statement on the fast timescale (L. 20-22).

L20: Can “character” be described specifically? Does it mean viscosity?

In our simulations the solid-Earth structure is characterized by the lithosphere thickness and the upper-mantle viscosity. We rephrased this sentence for more clarity (L. 24-26).

L22-24: This conclusion of the abstract comes a bit out of the blue. It is not clear how a conclusion about the effect of magnitude and rate of emissions on Antarctic Ice Sheet stability can be drawn based on an idealized setup with perturbed basal ice-shelf melting. The abstract also does not mention what the perturbed basal ice-shelf melting is based on (realistic forcing?)

We removed this last sentence. We now also state that the forcing of basal ice-shelf melting is designed to aim for quasi-equilibrium conditions (L. 8-9).

L35: Were Bamber et al. (2009) the last and only projecting the WAIS contribution to global mean sea level?

Thanks for the hint. We added a more recent study.

L49-50: The list of references is a subselection of the number of publications showing that GIA is a stabilizing factor with a (potential) strong impact on MISI-type retreat and it is not clear why specifically these publications were selected. The list is a combination of studies with an idealized setup, based on observations, conducting projections and glacial cycles. In this context, missing references are Han et al. (2025), van Calcar et al. (2025), Gomez et al. (2024), van Calcar et al. (2023), Kachuck et al. (2020), Larour et al. (2019), Gomez et al. (2018), Pollard et al. (2017), Konrad et al. (2015), Gomez et al. (2015), and Adhikari et al. (2014). The same accounts for the list of references in line 53. Almost all of the above mentioned studies show the importance of Earth structure for bedrock uplift rates. Also the references in lines 369-370 appear a random subselection.

We thank the Referee for pointing us to studies we missed to cite in this context. Our aim here was to provide examples rather than to give a complete list of studies. We added most of the references mentioned by the Referee for more completeness and consistency (L. 52-54) and changed the list of studies in line 53 (now L. 57) to make sure that these references explicitly support our statement there. We now also give a more complete list of references in the

discussion section pointed out by the Referee (L. 430-432)

L52-53: What is meant by “length scale” of bed uplift?

Here we mean the horizontal extent of the bed response to ice-load reduction. We rephrased the sentence for more clarity (L. 55-57).

L53-58: In here, the Earth structure under Antarctica is described but it is not clear how it is related to the rest of the introduction. Is it mentioned because the regional variability in Earth structure might impact the tipping point dynamics? If so, this should be described here. Gomez et al. (2024) and van Calcar et al. (2025) show that lateral variations in Earth structure can impact the timing of the collapse of the West Antarctic Ice Sheet in projections.

Our intention here was to highlight that the GIA feedback can be expected to be more prominent in parts of the West Antarctic Ice Sheet compared to the East Antarctic Ice Sheet but that there are hints that also in East Antarctica locations of weak solid-Earth structure (and thus stronger GIA feedback potential) are possible. We rephrased this text passage for more clarity and added the literature suggested by the Referee.

L57-58: Not only the Earth structure beneath the East Antarctic Ice Sheet has strong regional variability, but also the mantle viscosity beneath West Antarctic Ice Sheet varies regionally with several orders of magnitude (e.g. Lloyd et al., 2020).

Thanks for the hint. We revised our statement accordingly and added the reference (L. 62-63).

L65-66: B-tipping was very well explained but such an explanation is missing for R-tipping. Especially because the title of the manuscript focuses on R-tipping.

We added a brief explanation of R-tipping (L. 80-82)

L72: An “Antarctic type” idealized model setup could also refer to a grounded ice dome with shelves at the edge. Please be more explicit that you simulate an Antarctic-type ice stream similar to Thwaites Glacier.

We rephrased the statement (L.93-98).

L72-74: Please mention briefly how you examine the effect of bed deformation on tipping dynamics. Which model do you use and which Earth structures are used? Since the present study does not include the lateral variations in Earth structure (which are mentioned previously in the introduction) this should be mentioned here.

We revised this section according to the Referee’s suggestion (L. 98-102).

L95-96: *It is stated that the viscosity is prescribed in the LC model with a reference to Appendix A (which I assume should be Appendix A1), but in this appendix, a characteristic time scale is derived. At this point in the text it is not clear why this time scale needs to be derived and why this is “intrinsic to the model configuration” (lines 566-567) since the main text states that the viscosity itself is prescribed.*

Our motivation of the statement made in the Methods section was to give a bit more detail on the two parts of the visco-elastic response. However, the wording used there and also in the Appendix was indeed misleading and we rephrased the statements for more clarity and also corrected the reference (now Appendix A1 instead of A) (L. 124-126, L. 683)

Furthermore, all the different timescales mentioned in table 2 (where Appendix A1 refers to) should be defined more extensively. For example, critical ramp-up timescales have not yet been introduced in the main text and yet it is mentioned in appendix A1 without clear explanation. Also explain in some detail the theory behind deriving the time scale. Appendix A1 also needs clarification on the dimensions of the loading applied for these extra experiments.

Regarding the critical ramp-up timescales and associated rates mentioned in Appendix A1, our aim was to show that the timescaling according to Eq. A1 does not only apply to the viscous response of the solid Earth but also to the ramp-up rates (inverse timescales) of R-tipping, without the intent to elaborate on that in the main text. However, we notice that this information may dilute the focus of our study which is why we removed the second paragraph from Appendix A1.

Regarding the dimensions of the ice load in the extra experiments: these experiments start from the spun-up ice-sheet state, i.e., the initial state of the hysteresis simulations, which we now explicitly mention for more clarity (L. 685-686).

Last, is there literature of other studies that use this method or is this a new method? Please place this method in context.

This method of inferring characteristic response times and magnitudes is not new and we now place it into context, providing references to earlier studies (L. 699-701).

L97: *Should “vertically” be “radially”?*

Yes, we replaced it.

L98: *Please describe what the choice of coupling interval of 10 years is based on.*

Done (L. 128-132)

L109: D_{BD} is 1600 meter deep in figure 2a, please explain the discrepancy. Also, the unit of the slope is missing.

D_{BD} does not refer to the deepest point of the bed depression in an absolute sense. It is the elevation difference between the deepest point of the bed depression and the tip of the coastal sill, as indicated in Fig. 2b by the double-headed yellow arrow. We now explicitly refer to this arrow in the text (L. 143) and in the figure caption.

Regarding the bed slope, it indeed has unit 1. For more clarity, we added to the text that a slope of $3 \cdot 10^{-3}$ means a change in bed elevation by 3 m per km (L. 144).

L114: Please describe the reason behind the assumption of isothermal ice and the effect of that assumption.

This and other simplifying assumptions of our model setup are discussed in the Discussion section to which we now refer to explicitly (L. 149-150).

L189-191: It is not clear how the described result is visible in figure 4b. It is also not clear for which experiment figure 4a and b show the results. What was the mantle viscosity and lithospheric thickness?

The reference to Figure 4b might indeed be misleading here, so we removed it. Furthermore, we revised the figure caption to make clear that all three panels of Figure 4 belong to the same hysteresis experiment (using the same parameter combination).

Fig. A1: Only two lines are visible, not all the lines of the legend.

All four dotted lines are actually shown but indeed enormously hard to distinguish since their scaled versions align very well (showing this is the purpose of this plot). We state this explicitly in the figure caption. Note that we now show results for an initial bed-depression depth of $D_{BD}=900$ m (in contrast to the previously used $D_{BD}=300$ m) to be consistent with newly conducted perturbation experiments shown in Figs. S13-S15 and discussed in new Sect. 1 on page 14 of the Supplement.

Fig. 2a: D_{BD} is not defined in the caption.

We reworded the figure caption accordingly.

Fig. 5: Please define T_e in the caption.

Done.