Response to Referee#3

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 will prepare a revised version of the manuscript, addressing the points raised by the Referee. Please find sbelow the *Referee's comments in italics* and our response in blue.

Sincerely,
Johannes Feldmann et al.

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.

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 will provide a deeper discussion of the available knowledge about (West) Antarctic bed uplift and tipping dynamics. As also proposed by the Referee, we will 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.

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.

We thank the Referee for pointing out that the structure and clarity of Sect. 3.1 could be improved. We will revise this section according to the Referee's suggestions.

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 an aspect which we are currently analyzing in more depth with additional simulations. We aim to provide a more detailed discussion once the simulations are finished.

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.

We find this an interesting comparison and will incorporate it in the revised version of the manuscript.

Line by line comments

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

Will be done.

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

The reference is the B-tipping threshold and we will add this information.

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

We will do so!

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 will rephrase this sentence for more clarity.

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 will carefully revise the abstract in this regard.

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

Thanks for the hint. We will add more up-to-date literature here in order provide a more recent estimate of WAIS' potential sea level contribution.

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 will add the missing references where indicated by the Referee for more compeleteness and consistency.

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 will rephrase the sentence for more clarity.

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 AntarcticIce 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 will revise this paragraph for more clarity and also explicitely discuss how the strength/weakness of the solid Earth structure (and it's lateral variations) is expected to interact with the timing of potential (West Antarctic) ice-sheet retreat, based on the existing literature.

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 will revise our statement accordingly.

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 will add more detail here.

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

We will do so!

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 will revise this section accordingly.

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.*

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.

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

We will revise these sections and add more detail for more clarity, according to the Referee's suggestion.

L97: Should "vertically" be "radially"?

Yes, will be replaced.

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

Will be done.

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

As indicated in Fig. 2b by the yellow arrows, 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. We will try to make this clearer. The bed slope indeed has unit 1 but we will add that a slope of $3*10^{-3}$ means a change in bed elevation by 3 m per km ($3*10^{-3}$ = 3 m/km).

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

We will do so.

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?

We will be more explicit in our statement here and provide more detail and a clearer link to the results shown in Fig. 4.

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 will state this explicitely in the figure caption.

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

We will make sure to point to the visualization of D_{BD} to in the figure caption.

Fig. 5: Please define Te in the caption.

We will do so.