

Dear authors, dear editor,

I am pleased to see that the revision process helped the authors to spot an error in their code, which they honestly reported. Although this is simply “how things should be”, I’d like to praise them for stating this clearly and complying with good scientific practice. The resulting changes in the numbers are significant but do not change the overall message of the paper: the physical mechanisms at play are quantitatively affected, but remain significant and qualitatively the same. Other modifications like re-running some experiments with a 1-year time step is a well-appreciated effort.

The concerns raised in the first round of review were largely addressed, and I believe that no additional simulation is needed. However, the manuscript still contains inexact phrasing and lacks discussing some important limitations. These modifications are easy to perform and therefore do not present a major obstacle. Nonetheless, I insist on the fact that they need to be addressed before publication.

I hope the specific comments below will make the paper stronger and wish the authors the best of luck for the remainder of the publication process.

Must fix before publication

L. 20: You state 60%, which is not fully coherent with the rest of the text: -7 to -54% (l. 335), 7 to 62% (l. 503). At l. 518 you narrow this down to 7 - 40% for realistic solid-Earth conditions. The latter is clearly the number that should be stated in the abstract and the other values (outside the realistic range) should agree with each other.

L. 98-101: Swierczek-Jereczek et al. (2025) do not only study R-tipping in the context of MISI, they also proposed the mechanism that you are studying here (timescale separation between ice retreat and GIA response). This needs to be stated. L. 479-483 would also benefit from this reference to show that not only the occurrence of R-tipping but also its mechanism is robust across both studies.

L. 136: Here, you introduce ELVA as an acronym, used on some occasions in the rest of the paper. However, you sometimes switch to LC instead. Both are fine, you should just stick to one of them (you do the same with model/module).

L. 225: How long are the branch-off simulations, in particular for $\eta = 5 \cdot 10^{22}$ where $\tau_{SE} = 475$ kyr? If less than $\sim 3-5 \tau_{SE}$, the inferred m_B -tip might not be fully reliable and needs to be discussed in a slightly more quantitative way than what you do at l. 610.

Furthermore, τ_{SE} was estimated based on a single value of T_e and for a single horizontal extent of the load, although they both have a key importance on the uplift rates, as you show (also Bueler et al., 2007 for the horizontal extent). This needs to be critically discussed.

L. 232: With this sampling interval for equilibrium runs, you have plenty of points to plot in Fig. 4.a (and optionally a subsampled set of them in Fig. 6.a-p). No need for vertical lines as you state in your answer to the first round of review, just a scatter plot of the final equilibrium states in your basal melt/GL position phase space, instead of the few illustrative markers you have for now. This is important to visualize the difference between the results provided by the ramp forcing and the “true” equilibrium points. If the 0.1 m yr^{-1} interval crowds the plot too much, feel free to evenly sample at

e.g. 0.2 m yr^{-1} . This would tie in nicely with the scatters you show at the end of the branch-off simulations in Fig. 3.

L. 470-471: You state a doubling to tripling for the B-tipping difference in East-Antarctica although Fig. 7 and Fig. S5 show maximal values of +62% and +112%, respectively. Please correct this.

L. 494. " 0.2 K yr^{-1} per decade" $\rightarrow$ 0.2 K per decade . Furthermore, I am not sure if Schmidtke et al. (2014) is the best citation here, since circumpolar deepwater intrusion has been changing in recent years.

Table 1: Young's modulus and Poisson ratio... of what? You should also state the lithospheric and upper-mantle density you are using.

Fig. 7: Panels should be a and b.

Fig. A3 shows that the results are far from converging with the resolution yet. Here again, nailing this would be too much effort to ask, but please discuss this more critically. For now, you only state "The 2-km resolution has been successfully employed for modelling dynamics of fast-flowing ice in similar computational setups in previous studies (e.g., Feldmann and Levermann, 2023).", which needs to be contrasted with what you show in the appendix.

Minor suggestions

L. 56 and following: I think the paper would benefit from a clarification early on. The integral of equilibrium vertical bedrock displacement is the same regardless of the lithospheric thickness. The latter however controls how localised the response is, which in turn controls vertical displacement rates. This is important to understand that lithospheric thickness modulates timescales that are key for the R-tipping mechanism.

L. 701: The DOIs for the videos would have been convenient for the second review too...

Figs. 4 & 5: the dots indicating grounding-line positions are way too small to be seen. Please make them bigger and optionally reduce the number of plotted time slices if it crowds the plot too much.

All figures: please make sure that the font size is roughly the text size in the final version.

One note on LC: naming it "Lingle-Clark" is quite unfair to Cathles (1975), who provided most of the derivation. Lingle and Clark (1985) just applied these equations to ice sheets. I think the model should be named CLC to be accurate and acknowledge the important and original contribution of Cathles.