## Review of "Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections" by Juarez-Martinez et al.

In this paper, the authors investigate the effect of including a scheme to account for the additional melt induced by tidal-water intrusions in projections of the Antarctic Ice Sheet. To that end, they develop and test a simple parametrization of basal melt aimed at representing this effect.

I enjoyed reading the manuscript, which is well written and presents clearly described numerical experiments and results. That being said, I have two main concerns that I would like the authors to address. I also have a series of minor specific questions and remarks, listed below. Once these comments have been addressed, I will be happy to recommend the paper for publication in *The Cryosphere*.

### General comments

My first main concern is that the physical motivation of the model is insufficiently developed. Specifically, the authors assume that $L \propto (\partial_x H_{\mathrm{af}})^{-1}$ without explaining why this hypothesis is physically justified. While a connection with Robel et al. (2022) is mentioned, the comparison is never properly discussed; the authors only state that the assumption 'seems to be consistent with theory'. It would be useful if the authors could provide a more explicit comparison with the model developed in the aforementioned study. Furthermore, the resulting parametrization –equation (6)– does not exactly follow this assumption; rather, it consists of a basal melt expression that is linear in $H_{\mathrm{af}}$ and switches from fully grounded melt to fully floating melt. Overall, the authors should better clarify the assumptions/motivations behind their model.

A second concern is the resolution, which is rather coarse (16 km). While such a resolution is perfectly acceptable for paleo-simulations, it may be questionable in future projections, especially for the coming centuries, given the limitations associated with such a resolution. The authors mention this important limitation at the end of their discussion, but it would be beneficial to discuss it upfront, when the numerical set-up is presented. This is particularly relevant because the effective basal melt $\dot{b}$ is computed as a simple average of the subgrid points $\dot{b}_j$. Given the non-linear dynamics involved, one may wonder whether such a representation is accurate, as some of the points might contribute more than others to the resulting ice-sheet response. Ideally, the authors should perform a small convergence test at different resolutions (e.g., 8 / 16 / 32 km) to assess the influence of resolution on their results. I am not asking the authors to conduct such a study for their full set of results, as this might require significant computing resources, but I think it would be reasonable to test this with a single 'representative' simulation.

### Specific comments

(1) [Line 49] It seems that a missing reference here is 'Tidal Grounding-Line Migration Modulated by Subglacial Hydrology' by Warburton et al. (2020).

(2) [Line 110] It is slightly confusing to use the same symbol $A$ on both sides of the equation in $A = EA(T')$; maybe use different symbols?

(3) [Line 117] Could you provide a justification for the value of $q = 0.2$? I would expect the more classical value $q = 1/3$.

(4) [Equation (2)] The till drainage described by this equation typically leads to a binary mask for the basal conditions, with only fully saturated and fully unsaturated regions (see, e.g., Bueler and van Pelt, 2015, figure 7a). This is because the model does not consider any horizontal transport component for the subglacial hydrology. Given the recent research addressing this question (e.g., Werder et al., 2013; Sommers et al., 2018; Kazmierczak et al., 2024), it would be useful to mention this limitation.

(5) [Equation (3)] $\mathrm{T_F} \rightarrow T_\mathrm{F}$.

(6) [Equation (4)] Should this be written as $L \propto \|\nabla H_\mathrm{af}\|^{-1}$ instead, given the two-dimensional nature of the grounded domain?

(7) [Line 168] I assume that the reduction is taken to be linear for simplicity. If that is indeed the case, please mention it.

(8) [Line 178] The value $M = 15$ seems somewhat arbitrary; I think the paper would be stronger if you could show some form of convergence of the quantities of interest, i.e., the sea-level contribution, as a function of $M$.

(9) [Line 192] Could this also be attributed to the coarse resolution, e.g., smaller bedrock features that might lead to specific patterns of seawater intrusion?

(10) [Line 193] Given the focus on the Antarctic Ice Sheet, I would move these Greenland glacier examples from the appendix to the supplementary material.

(11) [Line 211] This steady-state assumption is being increasingly questioned in the glaciological literature; perhaps mention in the future-work discussion the need to extend the results to a spin-up/initialization procedure that takes currently observed trends into account. Furthermore, this assumption might also explain the somewhat surprising results you discuss in lines 275–280.

(12) [Line 297] Here, I would add references to both Robel et al. (2022) and Kazmierczak et al. (2024); furthermore, it seems to me that the issue of soft/hard bed coverage is not only due to a lack of observations, but also to a lack of proper modeling of hard/soft bed behavior for sliding and hydrology.

(13) [Line 302] See previous comment; I would put the Greenland glaciers in the supplementary material.

(14) [Line 306] It would be useful to add an estimate of such timescales.

(15) [Line 309] Given the nonlinearities in the modeled system, I would replace 'mean' by 'effective'.

(16) [Line 333] 'critically' seems a bit strong, given the known importance of other factors influencing sea-level predictions, in particular external oceanic and atmospheric forcings as well as internal modeling choices (e.g., ice-flow model and resolution).

(17) [Notations] The notation for the subscripts is not consistent, with some subscripts written in italics, e.g., $b$, $c$, and $draft$, and others written in roman type, e.g., w, sw, and draft.

(18) [Figures] Please check that the figures are colorblind-friendly.

## References

Bueler, E. and van Pelt, W. (2015). Mass-conserving subglacial hydrology in the Parallel Ice Sheet Model version 0.6. *Geoscientific Model Development*, 8(6):1613–1635.

Kazmierczak, E., Gregov, T., Coulon, V., and Pattyn, F. (2024). A fast and simplified subglacial hydrological model for the Antarctic Ice Sheet and outlet glaciers. *The Cryosphere*, 18(12):5887–5911.

Robel, A. A., Wilson, E., and Seroussi, H. (2022). Layered seawater intrusion and melt under grounded ice. *The Cryosphere*, 16(2):451–469.

Sommers, A., Rajaram, H., and Morlighem, M. (2018). SHAKTI: Subglacial Hydrology and Kinetic, Transient Interactions v1.0. *Geoscientific Model Development*, 11(7):2955–2974.

Warburton, K. L. P., Hewitt, D. R., and Neufeld, J. A. (2020). Tidal Grounding-Line Migration Modulated by Subglacial Hydrology. *Geophysical Research Letters*, 47(17).

Werder, M. A., Hewitt, I. J., Schoof, C. G., and Flowers, G. E. (2013). Modeling channelized and distributed subglacial drainage in two dimensions. *Journal of Geophysical Research: Earth Surface*, 118(4):2140–2158.