Review and editor comment from "Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections" - Second Round

Antonio Juarez-Martinez et al.

Please, find a point-by-point response to the answers of reviewer 1 and the editor.

The manuscript "Parametrizing tidal-water intrusions in long-term Antarctic ice-sheet projections" by Antonio Juarez-Martinez and coauthors presents a novel treatment of grounding zones in the ice-sheet model Yelmo. Authors use remote sensing data to extract an estimate of grounding zone extent by calculating height above floatation. They test the role of this new parameterization with a sensitivity approach to estimate sea-level projections from the Antarctic Ice Sheet by year 3000. I have reviewed this manuscript twice. The authors have done a great job in addressing my earlier comments. I believe the manuscript has largely improved since the initial submission and it is very close to a publishable version. I do not have major remaining comments but only a few minor issues. If these are properly addressed, I am happy to recommend for publication.

Thank you very much for your review and your comment.

Line 69: This is technically not a correct statement as several studies (Robel 2022, TC, Parizek 2013 JGR Earth Surface, Poinelli 2026 in review) have tested the sensitivity of sea-level projection to the extent of grounding zone length. Please revise this sentence.

We have changed the sentence in this way: "An increased model sensitivity would have implications for future ice-volume and sea-level projections, but except for a few limited exceptions (e.g. Parizek et al., 2013; Robel et al., 2022; Poinelli et al., 2026) this has not been studied in depth."

Line 123: I assume you extend the friction coefficient all the way into the grounding zone, since in your parameterization GZs are characterized by values H_t>0, i.e., in regions that would be grounded in traditional configurations. How, then, is the friction coefficient treated within the grounding zone?

The friction coefficient is calculated for every cell that is grounded or partially grounded, based on the initialization method described in Lipscomb et al., (2021). For friction we do not yet explore the possible impact of tidal-water intrusions. Nonetheless, at most grounding lines, the friction is already near zero since the ice is typically sliding with fast velocity and generating water at the base of the ice sheet. So it can be expected that the impact of tidal intrusions on friction would be less than its impact on basal melting rates.

Line 142: I don't understand this sentence, 'motivates this parameterization'. You don't seem to have an ocean model underneath floating ice, which would deal with buoyancy redistribution due to freshwater discharge.

We agree with this point. We just meant to explain the reason for the non-local character of the parameterization, that is, that sub-shelf melting at a given location is influenced by processes occurring at far away locations (non local). To make this clear we have modified the paragraph as follows:

“The resulting buoyancy-driven circulation redistributes heat and freshwater throughout the cavity, influencing ocean temperatures and melt rates far from the location where the initial melting occurred, which explains the non-local character of the process and the parameterization”

Line 235-240: please clearly define what values of H_t you use in the sensitivity study below. At this point the reader thinks that you test 15-130 m but later on you are using 0-150 m, please define the range used in your experiment in this paragraph.

This could be a misunderstanding. Our assessment of possible Ht values conclude that positive values are needed, in the range 15-130 m. Nevertheless we will clarify this point here to clearly state that the range [0,150] m is the range of values tested, and [15,130] m are the range of values deduced from observations.

Line 266: “For CCSM4, the WAIS, in contrast, does not” -> “In contrast, using CCSM4, WAIS does not”

We will revise this sentence as suggested.

Line 272: What do you mean with “early stages”? Perhaps the first hundreds of years? Please elaborate.

We mean the first decades as in Figure S8 of the Supplementary Material where we show the effects until year 2025. We will be more specific in the revised text replacing “early stages” by “first decade”.

Line 275: “most clearly seen”. Why is that? Please state the reasons of these differences, deeper bathymetry? Steeper slope?

In these regions, the thermal forcing is slightly higher than for other ice shelves at the beginning of the simulations (Figure A1 in Juarez-Martinez et al., 2024), and the thermal forcing correction at the end of the spin-up (Figure R1 below) shows positive values that contribute to greater basal melting.

Line 306: While Rignot et al. (2024) is an observational study, Robel 2022, Poinelli 2026, and Parizek 2013 already demonstrate the impact of grounding zones on sea-level projections by 2100, particularly for Thwaites and the ASE. In Table B2, you state that WAIS sea-level contribution exactly doubles by 2100 in all cases—is that correct, by the way? However, in the text (line 248), you describe the differences as “small”. Could you please consolidate these statements into a clear and consistent message? What is the impact over the first 100 years of simulations?

Yes, it is correct, the SLCs for Ht=0 and Ht=150 are 0.04 and 0.08 m SLE, respectively. We will revise this sentence to read in the new version: “Differences are already apparent in 2100, although their absolute magnitude remains small (~0.04 m SLE).”

The impact can be seen in Figure S7 of the Supplementary Material, where the SLC when using the two GCMs is represented for the different values of Ht until year 2100, showing an increasing contribution as Ht increases.

Line 314: same as before, please define “short term”. Looking at table B2, the impact seems to be 2x by 2100. Are these values correct?

Yes, they are correct. These are different values of Ht and they are small. In 2300, or later, the relative difference is reduced. We will be more explicit in the text replacing “short term”, with “before 2100” for this case.

Line 321: How do you explain this gain in mass and what do you mean with a “more realistic initialization”? Can you elaborate and discuss the uncertainty introduced by the initialization?

This mass gain can be explained by the thermal correction at the end of the spinup, where we obtain different values (Figure R1) at different sectors depending on the Ht value.

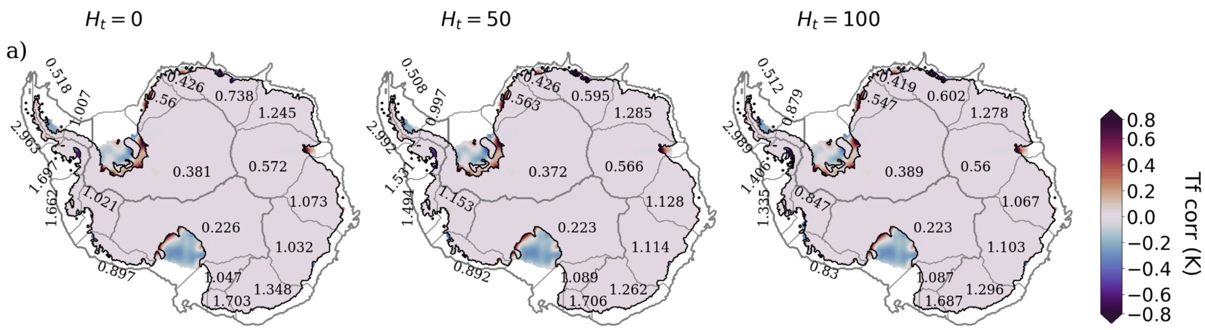

Figure R1. Average thermal forcing correction (K) in the different basins at the end of the spin-up for different values of H_t.

A more realistic initialization refers to the idea that our initialization would be potentially improved if we had better observations of current and past basal melt rates. Our current initialization ensures stability by running for four thousand years without further correcting the two inverted parameters (thermal forcing and basal friction) after the optimization. This leads to small differences in the initial field of basal melting (Figure S1) derived from the fact that optimizing with different values of Ht can lead to small overcorrections in a non-monotonical way. We have corrected the sentence as follows: “This suggests that an even more realistic initialization, (e.g., involving a more accurate ocean temperature target)”.

Line 353: I commend the authors for running the additional simulations at different resolutions, and I am happy to hear that the results are robust. However, 8 km is still a relatively coarse resolution, and recent observations and modeling studies have shown that much shorter seawater intrusions can be important. My concern is that, at 8 km resolution and with the parameterization adopted here, the model may not fully capture the extent of these intrusions, particularly in areas with small H_t For example, what is the effective grounding-zone extent represented by the model in regions where H_t is small? Looking at Figure S1, the 50–150 m isolines appear to be in substantially similar locations, but how are differences in grounding-zone extent captured when the model resolution is as coarse as 16 km, even with a sub-grid parameterization? My guess is that this resolution may limit the ability of the model to reproduce the wide range of grounding-zone extents observed in reality. I do not have any objection to the approach adopted here, which is understandable and likely necessary for continental-scale simulations given the computational expense. However, I think this limitation and its potential implications for the results would be worth discussing further.

Our results are robust with respect to spatial resolution. However our highest resolution is only 8 km which might still be too coarse to properly represent shorter seawater intrusions. In that case our results would underestimate the response to enhanced intrusions.

As mentioned in the text, a possible extension of the proposed approach would be to allow Ht to vary spatially rather than using a single constant value as we proposed in the current version of the manuscript. Such a formulation could account for regional differences in the characteristic extent of GZ processes and therefore represent shorter or more spatially confined seawater intrusions in regions where appropriate. This would retain the computational advantages of the sub-grid parameterization while providing greater spatial flexibility without requiring a uniformly finer model resolution. However, the choice of a spatially variable Ht would require additional observational or modelling constraints and is beyond the scope of the present study.

We have therefore added a discussion of this limitation and its potential implications for the representation of GZ processes and basal melting in the revised manuscript.

Line 400: You mention the collapse of WAIS under continued warming. This is a fair point, but do you find any difference in the timing of collapse between the experiments? In line 262, you state that WAIS collapses regardless of H_t, but at what time does this occur in the different simulations? Robel 2022, for example, found that complete deglaciation of Thwaites occurs hundreds of years earlier when grounding-zone processes are represented than in experiments without them. How do your results compare with this? If you do not find a similarly large difference in the timing, could you explain why? My suspicion is that model resolution may play an important role here, but please correct me if I am mistaken. I think this would also be a useful point to discuss.

Yes, in lines 310-313, we try to assess the collapse of the ice shelves of the AIS by defining a threshold (4 m SLE) to be reached in the two cases for Ht, for the case of CESM2-WACCM. “For instance, for Ht = 0 m, the level of 4 m SLE is reached in the year 2778, while with Ht = 150 m already in the year 2671.” Therefore, to reach 4 m SLE, there is a difference of more than 100 years. Focusing on Thwaites, for Ht=0, the retreat of the GL starts around 2200, more or less the same time for Ht=150, but the complete collapse of this glacier in one case is around 2400 for Ht=150 and nearly in 2500 for Ht=0. We will make sure to point out the link to Robel et al. (2022) in the revised text.

REFERENCES:

Parizek, B. R., et al. (2013), Dynamic (in)stability of Thwaites Glacier, West Antarctica, J. Geophys. Res. Earth Surf., 118, 638–655, doi:10.1002/jgrf.20044.

Poinelli, M. et al. Sea-level contribution from Thwaites Glacier doubles in an observation-constrained ice-sheet model, 25 June 2026, PREPRINT (Version 1) available at Research Square [https://doi.org/10.21203/rs.3.rs-9216227/v1]

Robel, A. A., Wilson, E., and Seroussi, H.: Layered seawater intrusion and melt under grounded ice, The Cryosphere, 16, 451–469, https://doi.org/10.5194/tc-16-451-2022, 2022.

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Editor comment:

L130: Please check the citation to Lipscomb et al. (2019), as this paper does not include the von Mises calving law. If you are using a von Mises calving law, please provide the appropriate reference and specify the threshold value used.

Once these corrections have been addressed, the manuscript can proceed to publication.

Best regards,
Cheng Gong

Thank you for the correction. We have used the von Mises-like eigenvalue scheme from Lipscomb et al. (2019) with these values: scaling calving parameter kt=0.0025 myr^-1Pa^-1 and the calving eigenvalue weighting coefficient w_2=25. Anyhow, we will also cite the von Mises law properly adding: “This calving law is analogous to the von Mises calving law Morlighem et al. (2016), but stress is used in place of the strain rate and the parameters are empirical coefficients.”.

Morlighem, M., Bondzio, J., Seroussi, H., Rignot, E., Larour, E., Humbert, A., and Rebuffi, S.: Modeling of Store Gletscher’s calving dynamics, West Greenland, in response to ocean thermal forcing, Geophysical Research Letters, 43, 2659–2666, https://doi.org/10.1002/2016GL067695, 2016.