Review 2 from "Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections"

Antonio 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.

Thank you for your comments, feedback and suggestions to improve the manuscript. Please find a point-by-point response to your comments below.

General comments

My first main concern is that the physical motivation of the model is insufficiently developed. Specifically, the authors assume that L ∝ (∂xHaf)^−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 Haf 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 b' is computed as a simple average of the subgrid points 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.

We agree that the physical motivation of the model and its relation with Robel et al., (2022) need further clarification on our side. To this end we propose to add an appendix in the new version of the manuscript with the following explanation:

> Robel et al. (2022) describe a generalized theory of the physics behind layered seawater intrusions. They use the Ice Sheet System Model (ISSM) to study the impact of allowing such intrusions in an idealized setting with an imposed intrusion length L. By assuming that the basal melt rate $\dot{m}$ decreases linearly from the grounding line at $x = 0$ upstream until reaching zero at $x = L$, basal melt can be determined from this equation:
>
> $$\dot{m} = \dot{m}_{GL}\left(1 - \frac{x}{L}\right)$$

where $\dot{m}_{\mathrm{GL}}$ is the basal melt rate at the grounding line and the term $\left(1-\frac{x}{L}\right)$ can be thought of as representing the fraction of that location that can be considered floating, w_f as in Eq. 5 above. We seek to transform this equation to represent the floating fraction w_f as a function of the ice thickness above flotation H_{af} instead of L, since the former quantity is easier to diagnose in complex topographies as it only relies on the bedrock elevation, the ice thickness and sea level, which are all known quantities that are strongly related to L in reality (Robel et al., 2022). Given horizontal coordinates defined such that $x=0$ at the grounding line, and $x=L$ at the terminus of a given intrusion, we know that $H_{\mathrm{af}}(x=0)=0$ at the grounding line by definition, and we can define $H_{\mathrm{af}}(x=L)=H_t$. With these quantities defined, we can define the gradient of H_{af} as:

$$\frac{\partial H_{af}}{\partial x}=\frac{H_t}{L}$$

In essence, this relationship results from the fact that we relax the condition of allowing basal melting only for $H_{\mathrm{af}}<0$ (under floating ice, downstream of the grounding-line at $H_{\mathrm{af}}=0$) by allowing melting also for $H_{\mathrm{af}}<H_t$ (under grounded ice, up to a limit value of $H_{\mathrm{af}}=H_t$). For a given value of H_t, the length of the region over which we allow basal melting will increase with decreasing gradient of H_{af}.

Regarding the resolution concern, we have performed a subset of additional simulations with 32 km and 8km for a few Ht values using the same parameter values as for the 16 km runs for the model CESM2-WACCM. Simulations with 8 km are still difficult to perform due to a lack of computational resources, so we would keep our manuscript based on 16 km projections.

As shown in Figure R2-1, the sea-level contribution (SLC) relative to Ht = 0 increases as Ht increases, which is the main conclusion of our manuscript. Quantitatively, the sensitivity with respect to Ht does depend on the spatial resolution to some extent, but the relationship is robust.

We will mention the limitation upfront after the numerical setup is explained, and clarify in the Discussion the comparison for different resolutions in terms of SLC, adding this figure to the Supplementary Material.

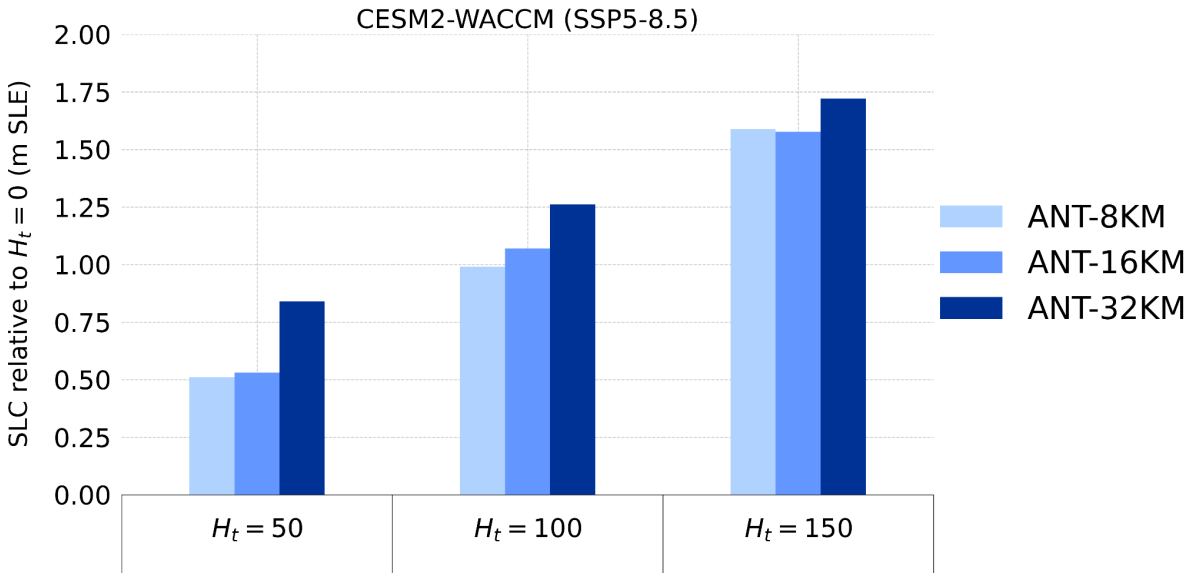

Figure R2-1. SLC relative to Ht = 0 (m SLE) for the three resolutions: 8, 16 and 32 km at year 3000, for different values of Ht, for the CESM2-WACCM climate forcing.

Please note that following a suggestion by reviewer 1, we have changed the topography used for the calibration of the Ht parameter from BedMachine v2 to Bedmap3. This does not significantly affect the results but leads to small changes in the figures relative to the previous version. Also, we have incorporated a new GCM forcing from ISMIP6-2300 to our simulations, CCSM4 under a RCP8.5 scenario, which produces a weaker oceanic forcing than CESM2-WACCM.

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).

Thank you for the reference. We will incorporate it conveniently for the next version of the manuscript.

(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?

Thank you for pointing this out. We will replace the A on the left side by A'.

(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.

This value has been used in other studies with Yelmo (Moreno-Parada et al., 2023; Blasco et al., 2024), following Zoet and Iverson (2020), who derived it from laboratory experiments. We will add this explanation to the new version of the manuscript.

(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.

We agree with the reviewer and we will modify the text to include this caveat and the corresponding references.

(5) [Equation (3)] TF → *T*F.

We have changed the notation.

(6) [Equation (4)] Should this be written as L ∝ // ∇Haf // ^−1 instead, given the two-dimensional nature
of the grounded domain?

For the purpose of the conceptual description and motivation of the parameterization, we think a one-dimensional equation might be easier to understand, taking into account that the coordinate is along a flowline as explained in the text.

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

Yes, this is a consequence of the assumption done for the weighting function in equation 5. We will make this more explicit in the new version.

(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.

We have performed additional simulations repeating the projections with M=30, and the results show no substantial difference with the original case M=15 (Figure R2-2). In Table R2-1, it can be seen that at year 3000, the result does not show any significant differences. We will add a comment on this on the Discussion.

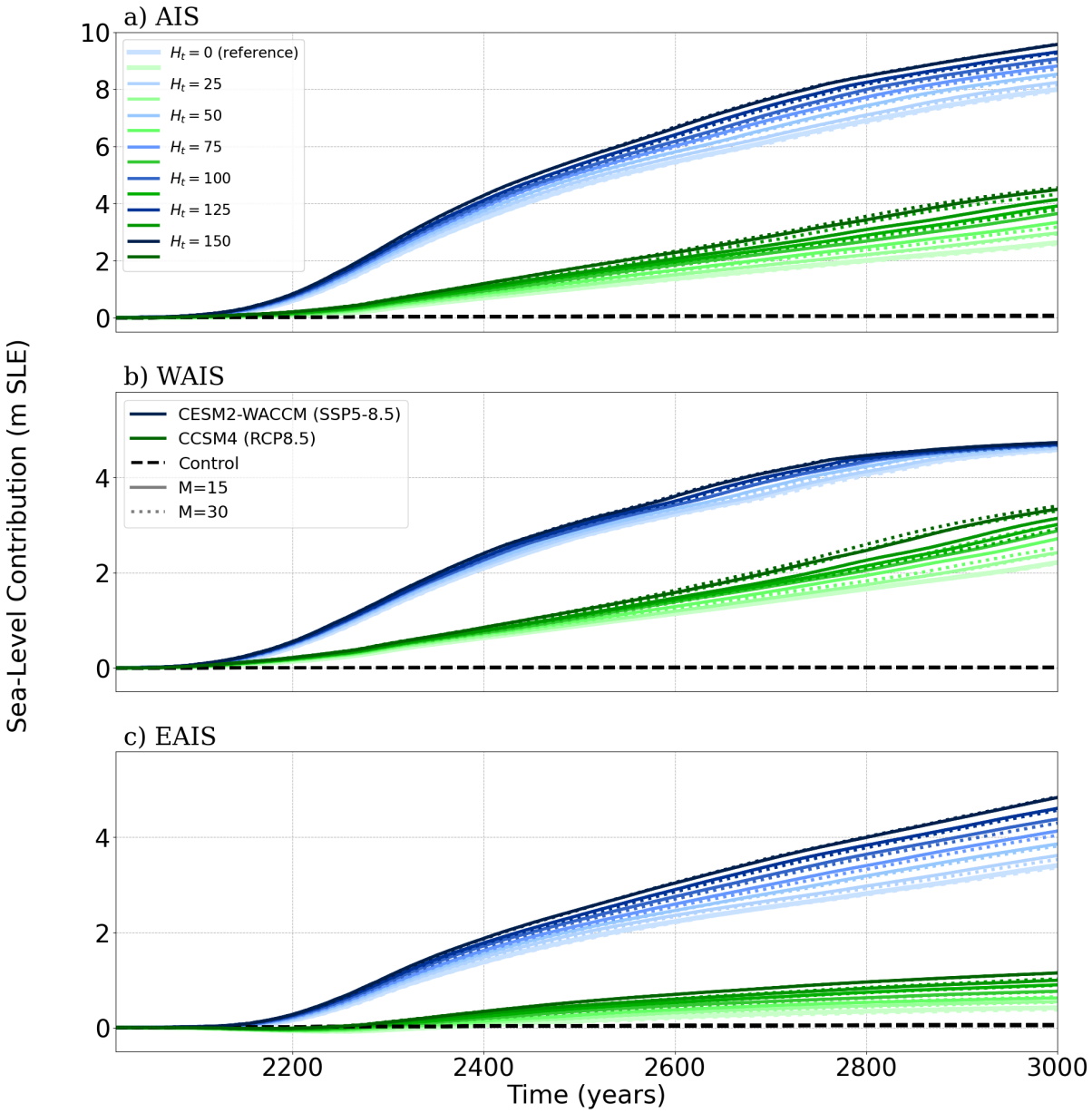

Figure R2-2. SLC (m SLE) comparison using different values of M for the PMPT parameterization.

AIS (Year 3000)	Ht=0	Ht=25	Ht=50	Ht=75	Ht=100	Ht=125	Ht=150
CCSM4 (M=15)	2.6	3.0	3.3	3.7	3.9	4.1	4.5
CCSM4 (M=30)	2.6	2.9	3.2	3.8	3.8	4.3	4.5
CESM2 (M=15)	8.0	8.2	8.5	8.8	9.1	9.3	9.6
CESM2 (M=30)	8.0	8.2	8.5	8.7	9.0	9.3	9.6

Table R2-1. Comparison of SLC (m SLE) for CCSM4 and CESM2-WACCM using different values of M at year 3000 at 16 km resolution.

(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?

This result is based on the topography used to compute the Ht isolines and independent of model resolution. We therefore think the mismatch reflects deficiencies in the topography datasets. Furthermore, we have made a comparison between datasets (BedMachine v2, v4 and Bedmap3) following a comment by reviewer 1, and there are substantial differences when using different topographies to calibrate the value of the parameter Ht. This highlights mainly the uncertainties in the inferred topographies. Also, note that in a recent paper by Zeising et al., (2026), they show that Bedmap3 presents better results than BedMachine, performing better in certain areas (with sharp bed roughness, steep slopes and grounding-zone transitions for instance), because it incorporates a much larger compilation of recent radar measurements and preserves more detailed bed morphology.

(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.

We agree. We will create a supplementary material file for that purpose.

(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 spinup/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.

We fully agree. We will add a comment on the Discussion regarding the need to consider a more realistic initialization in the future.

(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.

We agree with the reviewer. We will add the references suggested and include the problem of the lack of proper modelling.

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

We agree as mentioned before.

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

These timescales are usually of hours or days. We will include this in the new version.

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

We agree and will change this accordingly.

(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).

We will replace it with “strongly”.

(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.

Thank you. We have taken care of the notation and changed it to be consistent. We decided to use the italics when there is only one-character subindex and roman type when there are several-character subindexes.

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

We have checked, but we will make sure that the next version includes better colors.

References

Blasco, J., Tabone, I., Moreno-Parada, D., Robinson, A., Alvarez-Solas, J., Pattyn, F., and Montoya, M.: Antarctic tipping points triggered by the mid-Pliocene warm climate, Clim. Past, 20, 1919–1938, https://doi.org/10.5194/cp-20-1919-2024, 2024.

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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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