“Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections” - Replies to reviews 1, 2 and 3, and list of changes in the manuscript

Antonio Juarez-Martinez et al.

Here, we provide a point-by-point reply to the comments from the three reviewers (starting with reviewer 1, followed with reviewer 2, and ending with reviewer 3). The main changes incorporated into the manuscript are:

1) A more comprehensive description of the parameterization, experimental setup, results and discussion has been incorporated, particularly regarding technical aspects such as model resolution and topographic datasets.
2) Regarding the results, the analysis has been expanded to include simulations from two GCMs, allowing for a more comprehensive assessment of the effects of the parameterization.
3) An Appendix has been added to provide the physical motivation behind the parameterization.
4) A Supplementary Material document has been included to provide additional information and supporting results moving the figures in the preprint Appendix there.
5) The manuscript has been thoroughly revised to improve the language, including the rephrasing of several sentences for clarity and readability.
6) Several figures have been improved to enhance their quality and facilitate interpretation.

Review 1 from "Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections”

Antonio Juarez-Martinez et al.

The manuscript “Parametrizing tidal-water intrusions in long-term Antarctic ice-sheet projections” by Antonio Juarez-Martinez and coauthors presents a set of future simulations of the Antarctic ice sheet using Yelmo as ice-sheet model. The authors introduce melting beneath grounded ice by modifying the sub-grid melt parameterization with the goal of reproducing the concept of the grounding zone. I previously reviewed this manuscript for GRL and felt that its format was better suited to a technical journal such as The Cryosphere. I am pleased to see that the authors have addressed most of my earlier comments. Overall, I believe the paper requires significant revisions. I still have concerns about the methodology as well as some results. Please find my line-to-line comments below.

Thank you for your comments. We indeed used the GRL reviews to improve the manuscript already, but we see that it can benefit from further improvements. Please find a point-by-point response to your comments below.

Line 24: This line seems a bit off here perhaps worth incorporating it with the previous paragraph.

We agree. We will include it within the previous paragraph in the next version of the manuscript.

Line 89: Eq. 22 in this manuscript or in Robel et al 2022? Also, I am not sure this equation is referenced anywhere else in the manuscript, so please consider removing.

With “therein” we referred to Robel et al., (2022). We will nevertheless remove it as suggested.

Line 104: If the resolution is 16 km, how can you represent km-scale intrusions?

First, intrusions for which observations are available shown in Figure 2 of our manuscript are often on the order of several kilometers rather than km-scale. For instance, in Berry Glacier the intrusion length is roughly 15 km, while in Thwaites Glacier it can reach more than 30 km in some sectors.

Concerning the representation in our model, the parameterization has been implemented to represent the effects of tidal-water intrusions on sub-shelf melting along the lines of other subgrid-scale treatments of sub-shelf melting or basal friction at the grounding line (Leguy et al., 2021). In our framework, each grid cell is subdivided into a set of smaller subcells (15 × 15 in these experiments), with topographic quantities bilineary interpolated to each subcell location. This allows us to introduce spatial heterogeneity within a single coarse-resolution cell. At the subgrid level, we are able to represent the effects of intrusions taking place at spatial scales below our resolution by representing their impact on basal melting in the grounding zone. While in this case the intrusions themselves are not explicitly resolved in a geometrical sense, their influence on ocean-ice interactions is incorporated via the parameterization. This means that the model captures their integrated effect on melt rates without simulating the detailed structure or evolution of individual intrusions.

Nonetheless, we have performed additional simulations with 8 km and 32 km, and the main conclusion of our manuscript holds in the sense that a higher value of Ht leads to a higher sea-level contribution relative to the reference case (see Fig. R1-1). As simulations with 8 km are still difficult to perform due to a lack of computational resources, we would keep our manuscript based on the 16 km projections.

We had modified the description of the text significantly compared to our original GRL submission to make the subgrid character of the implementation in Yelmo clear, however we will emphasize this further in the revised manuscript. Also, we will mention the concern about resolution after the numerical setup is explained, and in the Discussion, we will clarify the comparison for different resolutions in terms of SLC, adding this figure to the Supplementary Material.

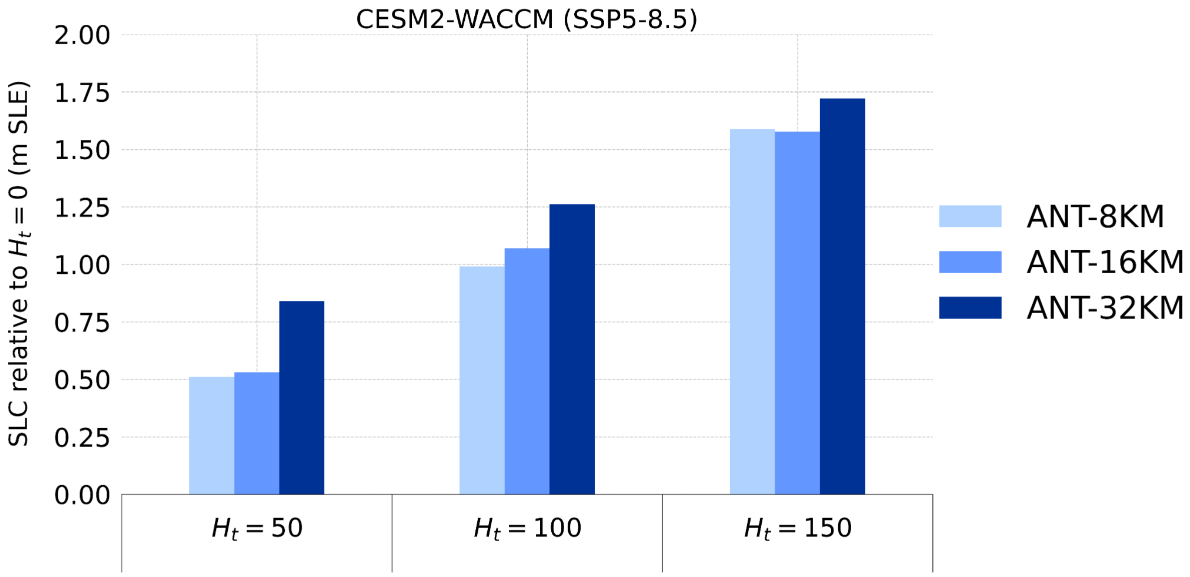

Figure R1-1. SLC relative to Ht = 0 (m SLE) for the three resolutions: 8, 16 and 32 km at year 3000, for different values of Ht, for the CESM2-WACCM climate forcing.

Line 160-161, ‘seems to be consistent’, ‘observations in the broad sense’. These statements are somewhat vague and lack quantification. Given the importance of this assumption, it should be supported by a clear and logical argument. I apologize if I have overlooked this point, but if so, I believe the authors should expand their discussion. It is not clear why the intrusion length would be expected to scale inversely with the gradient. From the figures, there seems to be a dependency to the inverse of H_af? Why the gradient?

We agree the language can be improved and the derivation should be made more clear. For this reason we will add an appendix to show the geometric relationship. We will modify the text from "Such an assumption seems to be consistent with theory (Robel et al., 2022) and, as we will show, with observations in the broad sense." to "Such an assumption is consistent with theory (Robel et al., 2022) - see Appendix for details." With "observations in the broad sense" we referred to the main figure, including GZs around the AIS, where observations are shown together with isolines, but it is not necessary to mention it, so we will remove it here. The text in the Appendix will be the following:

> 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}.

Line 187: While I have no objection to the use of Bedmap3, it is important to note that this dataset represents an average of bed elevations derived from multiple interpolation techniques across varying time scales. In contrast, the DInSAR product from Rignot's group is tied to specific timeframes (typically days to weeks/months). As such, a direct comparison between thickness inferred from Bedmap and grounding zone extent derived from Rignot's group should be carefully qualified, with the associated limitations clearly discussed.

This is a good point. We will add text about this caveat in the Discussion section.

Line 189: H_t = [15, 130] m. I think this is a very important result, but it is only introduced in the methodology. I am not aware of any other work that has quantified grounding zone in a similar, pan-Antarctic, fashion. If this is indeed the first study, please consider putting this finding in the results (followed by a discussion) and stress out the importance, also noting the limitation I expressed above.

Thank you for the comment. We have followed your suggestion and included this result in the Results section, after reorganizing the Methodology.

Line 203: It appears that you are using BedMachine v2 to initialize the model. While this version is somewhat dated (a more recent v4.1 release is now available), it would be helpful to clarify this choice. How does this approach compare with the use of Bedmap for the parameterization described above?

We agree on this issue. From the outset, we used BedMachine v2 to initialize the model as in other studies of our group. To test the sensitivity to the choice of the topography dataset we have performed the simulations using Bedmap3. Our conclusions are robust (Figure R1-2). Differences between projections with BedMachine-v2 and Bedmap3 at the end of the period are below 4%. To be consistent with the calibration, we have changed the current results to the ones using Bedmap3. 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. Please note that in the attached figures, we have added a new GCM forcing (CCSM4-RCP8.5) in relation to the comment on the saturation issue.

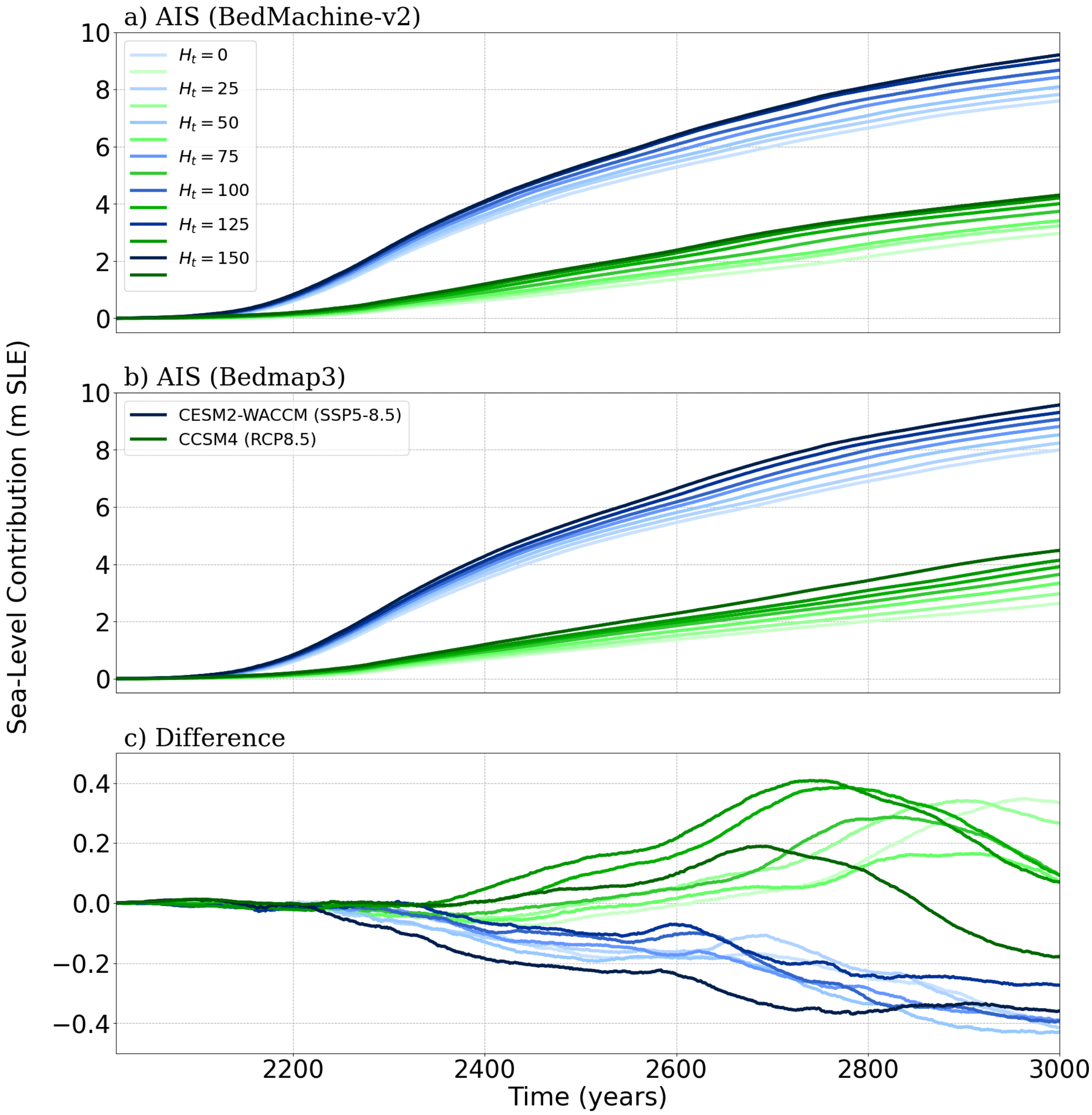

Figure R1-2. Sea-level contribution (SLC; m SLE) for the AIS using BedMachine-v2 (a) and Bedmap3 (b) as topography dataset. In c), absolute difference between a) and b).

If we use either dataset, the resulting relationship of higher sea-level contributions for higher values of Ht is consistent. Nevertheless, in order to obtain a better estimate of Ht (i.e., for the comparison or calibration of our parameterization), we choose to now use the more recent dataset Bedmap3.

Moreover, we have compared the distribution of the Ht inferred by combining our parameterization with the DinSAR data for BedMachine v2, v4, and Bedmap3 (our default now for the projections). As can be seen in Figure R1-3, the Bedmachine datasets tend to give lower values of height above flotation on average for all the points in the grounding zones (GZs) considered in the study, but they largely demonstrate the same skewed distribution towards positive values of Haf. This can be seen in the superimposed histograms in Figure R1-4, which allow us to state that the overall need here is to choose positive values of Ht. We will add this figure to the Discussion section and discuss the uncertainty in the datasets used for the topography, and the possible influence it could have in the selection of Ht.

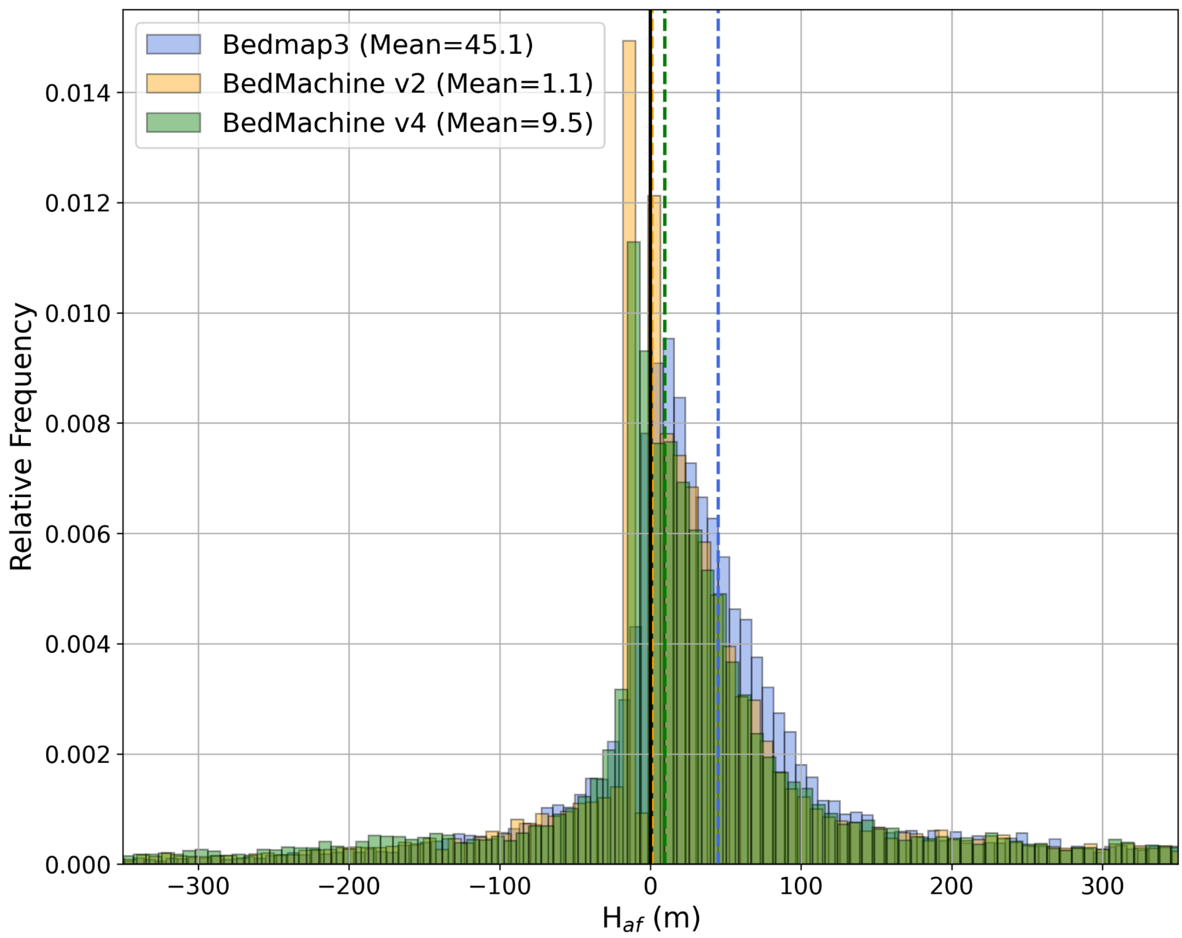

Figure R1-3. Histograms of Haf (m) for three different topography datasets (Bedmap3, BedMachine v2, and BedMachine v4), using all the six GZs regions from the manuscript. Dashed lines indicate the average value for each topography.

Line 234: While this statement may hold by the year 3000, the results do not show significant changes in WAIS even by 2100, differently than Robel et al 2022, for example. Does this imply that seawater intrusion is not a key factor for WAIS, despite its rapid retreat and grounding zone expansion over the past decade? If so, does this suggest that thermal forcing plays a more dominant role with respect to intrusion length? I feel the discussion around these points needs to be expanded.

On the contrary, important differences in the sea-level contribution (SLC) from the WAIS between Ht values are found at 2100. However it is true that due to the large vertical scale in Figure 3 this was not obvious. In particular, in Table A2, the WAIS SLC by 2100 goes from 0.04 m SLE for Ht=0 to 0.08 m for Ht=150 m. In Figure R1-4 (zoom over the figure in the manuscript), these differences can be seen more clearly. Motivated by your concern, we will add this figure to the new manuscript. The WAIS is therefore clearly vulnerable to the intrusions on these timescales.

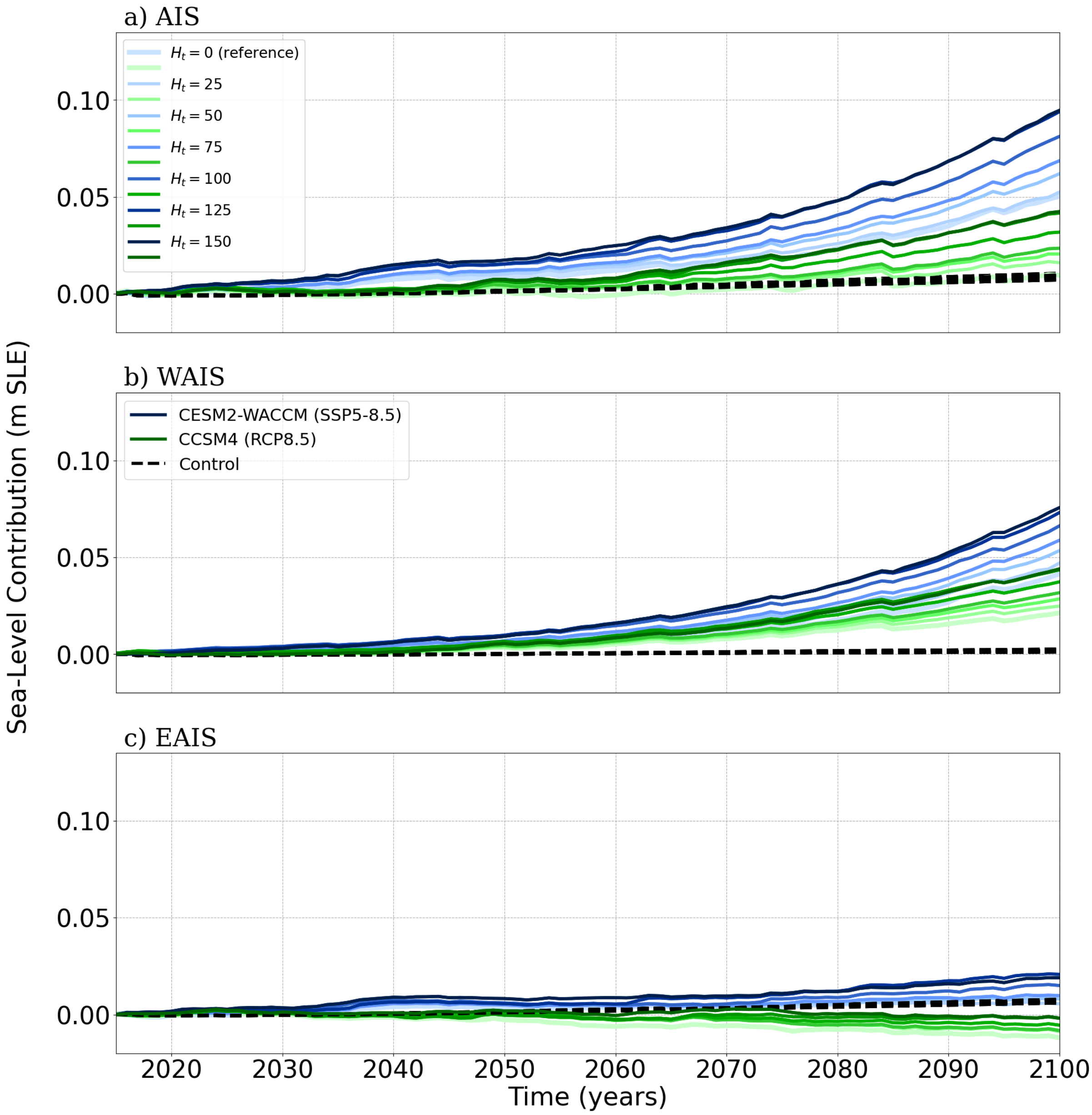

Figure R1-4. SLC (relative to 2015, m SLE) for the different values of Ht (in m) used until 2100. In a) for the whole AIS, in b) for the WAIS, and in c) for the EAIS. Dashed lines represent the control simulations corresponding to the different values of Ht, overlapping as variations are very small during the simulation period.

Line 270: Rignot 2024 and Robel 2022 report estimates of roughly a twofold increase in sea-level contribution for Thwaites and the Amundsen Sea Embayment when introducing actively melting grounding zones. In contrast, your results suggest that this sector of Antarctica is more strongly influenced by climate warming than by intrusion length, leading to a different conclusion. Could you clarify how these findings can be reconciled, as they appear contradictory?

As discussed above, the twofold increase in sea level by 2100 is exactly the same in our results. However, it is true that by 2300 all simulations with CESM2-WACCM exhibit a saturation in SLC. The latter is due to the fact that the oceanic forcing in CESM-WACCM is so strong that the WAIS collapses in all runs by 2800, regardless of the Ht value. Thus, in the long term the system reaches a similar end state in all cases.

To better illustrate this sensitivity we have performed additional simulations using another forcing (CCSM4). Qualitatively the main conclusions remain valid, but there are quantitative differences, especially in relation with the vulnerability of the WAIS. Figure R1-5 shows the evolution of the SLC (using Bedmap3 as topography to be consistent with the previous comment), but including the climate forcing obtained with CCSM4 under the high-emission scenario RCP8.5. This model appears to have less sensitivity in the Southern Ocean to the CO2 radiative forcing, producing a lower magnitude of oceanic forcing. Thus the saturation effect due to the strong forcing in CESM-WACCM is prevented (Purich and England, 2021; Zhu et al., 2021, Juarez-Martinez et al., 2024). This allows us to see differences in the long term over the WAIS when using different Ht values. We therefore will replace Figure 3 with these results including both GCMs. In this way the quantitative agreement with Robel et al., (2022) and Rignot et al., (2024) will be clear.

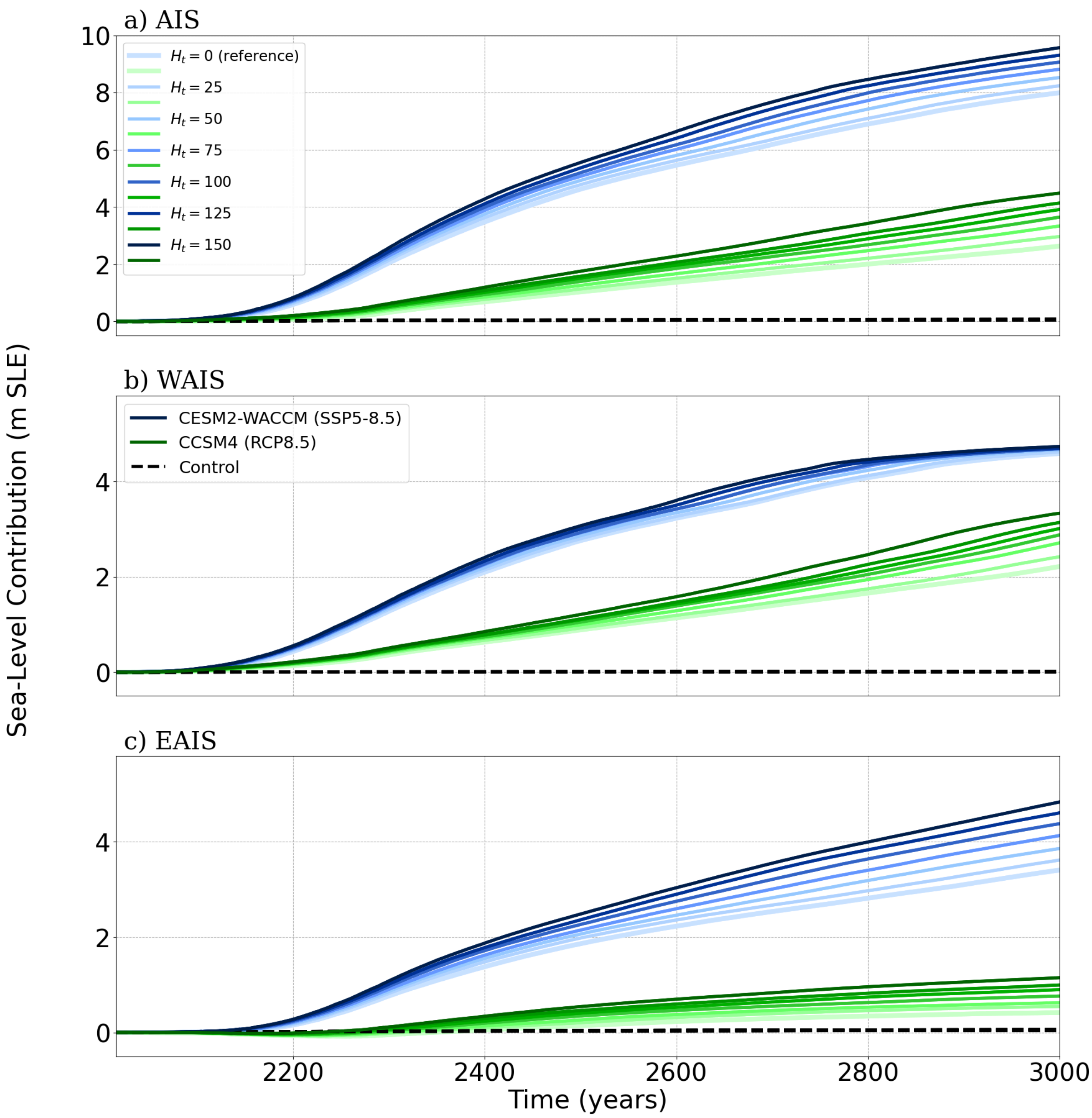

Figure R1-5. SLC (relative to 2015, m SLE) for the different values of Ht (in m) used for CESM2-WACCM (blue) and CCSM4 (green). In a) for the whole AIS, in b) for the WAIS, and in c) for the EAIS. Dashed lines represent the control simulations corresponding to the different values of Ht, overlapping as variations are very small during the simulation period.

Line 272: This result is only valid for EAIS only, right? WAIS seems to show no impact from tidal intrusion.

The 20 % value here refers to the whole AIS. With the discussion above, we hope that the issue on the WAIS sensitivity is now clear.

Line 281: This paragraph is somewhat confusing, particularly regarding the interpretation for WAIS. It is not clear whether WAIS is considered vulnerable to seawater intrusion or not. If not, this is in direct contrast to Robel et al 2022 and Rignot 2024. As written, the discussion seems ambiguous. Could this be related to the choice of initial conditions (e.g., ice thickness)? Clarifying this point would help strengthen the overall argument.

We agree that this point needs to be clarified. Indeed, this is related to the initial conditions. Specifically, we performed separate spin-ups for each value of Ht. These lead to different initial states (see Figure R1-6), which might slightly influence the results. But as explained above, this does not mean the WAIS is not vulnerable to the intrusions (Figure R1-4).

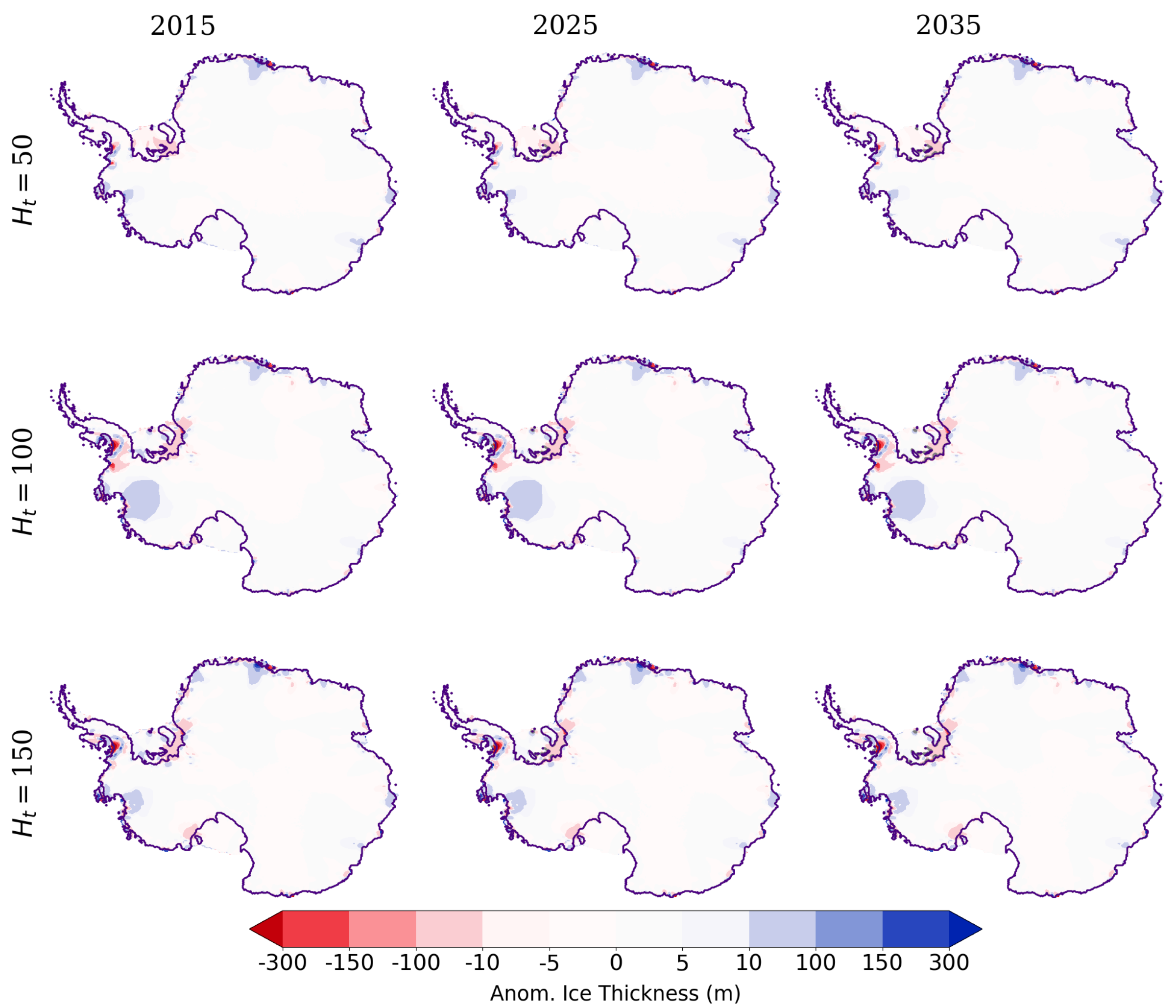

Figure R1-6. Same as Figure 4 in the manuscript, but for the initial years. In particular, the first column shows the anomalies in the initial state.

Line 286: I am still unclear about the rationale behind the parameterization and its dependence on the inverse of the H_af gradient. Could the authors clarify why the gradient is used as the parameter rather than the simple height above flotation? What is the physical or conceptual reason for this choice?

As explained above, we do not quantify L. We only use the height above flotation Haf and the value of Haf up to which we allow melting (Ht). Hence Ht, a threshold value of Haf, is the only parameter. This formulation implies an inverse relationship of the spatial gradient of Haf to L. For a given value of Ht, the larger the gradient of Haf, the faster Ht is reached. Therefore a smaller Haf gradient tends to produce longer intrusion length L, but we never quantify this variable. Nevertheless, we can get an idea of L based on such relation. As mentioned above, we now clarify this derivation in a new appendix, which we believe should give the information needed.

Line 299: As in my previous review, I am not sure if 'calibrated; is a correct term here. I'd rather say 'compared' to observations.

Thank you for the suggestion. We will change it for the new version.

Line 301: I do not see a discussion of the Greenland glacier comparison in the main text. Figure A1 is included, but without accompanying explanation, it is difficult for the reader to interpret its relevance. Including two Greenland glaciers seems unusual given that the study focuses on Antarctica, as Greenland fjords have different ocean circulation, thermal forcing, bed topography, and other properties. Could the authors clarify the purpose of this comparison and how it supports the main message of the study?

We assume that the reviewer is referring to Figure A2. We agree that this figure is currently not explained and could potentially be removed. However, it was included to illustrate that the parameterization can be applied to any ice sheet. In particular, comparisons across different grounding zones and isolines in Greenland may provide a way to evaluate its applicability. We will add a paragraph clarifying this point in the revised manuscript, and move the figure to the Supplementary Material following another reviewer's suggestion.

Line 310: Like my earlier comment. How does a 16 km model resolution allow you to draw conclusions regarding ocean-induced melt over km-scale intrusions? I understand that sub-grid melt parameterizations are used, but it is important to quantify the associated errors. Simply stating that "coarse resolutions tend to produce higher estimates" is insufficient—can the authors provide a more detailed assessment of the limitations and uncertainties introduced by the coarse resolution?

For this please see our response to your comment on line 104, at the beginning of the document, which includes an assessment with other resolutions (32 km and 8 km).

Line 317: The statement "For this reason …" is somewhat misleading. The very large climate forcing you are using may effectively mask the impact of intrusion lengths on WAIS. In that case, can you be confident that the effect of intrusion length is truly being isolated?

Thank you for pointing this out. We have performed additional simulations using other forcings (see Figure R1-5). Qualitative conclusions presented in the current version of the manuscript remain valid, but there are quantitative differences, especially in relation to the vulnerability of the WAIS, which has been mentioned in previous comments. Figure R1-4 shows the evolution of the SLC but including also CCSM4 under the high-emission scenario RCP8.5. This climate model produces a lower oceanic forcing, therefore the collapse of the WAIS is delayed compared with CESM2-WACCM, and the impact of Ht is more obvious because the saturation effect due to the strong forcing in CESM-WACCM is prevented. We therefore will replace Figure 3 with these results (CESM2-WACCM and CCSM4). In this way the quantitative agreement with Robel et al., (2022) and Rignot et al., (2024) will be clear.

Line 320: The final paragraph appears to be off-topic and does not contribute to the main argument. Its relevance to the study is unclear and may need to be removed or substantially revised.

We agree, we will remove this paragraph.

Line 326: The quantification of H_t between [15-130] m This is a very important result, but it is currently given too little attention in the text. I recommend moving its discussion from the Methods and Conclusion sections directly into the Results section, where it can be properly highlighted and interpreted.

Thank you again for the suggestion. We will reorganise the text to include it in that section.

References:

Juarez-Martinez, A., Blasco, J., Robinson, A., Montoya, M., and Alvarez-Solas, J.: Antarctic sensitivity to oceanic melting parameterizations, The Cryosphere, 18, 4257–4283, https://doi.org/10.5194/tc-18-4257-2024, 2024.

Leguy, GR, Lipscomb, WH and Asay-Davis, XS (2021) Marine ice sheet experiments with the Community Ice Sheet Model. The Cryosphere 15, 3229–3253.

Purich, A. and England, M. H.: Historical and future projected warming of Antarctic Shelf Bottom Water in CMIP6 models, Geophys. Res. Lett., 48, e2021GL092752, https://doi.org/10.1029/2021GL092752, 2021.

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.

Robinson, A., Alvarez-Solas, J., Montoya, M., Goelzer, H., Greve, R., and Ritz, C.: Description and validation of the ice-sheet model Yelmo (version 1.0), Geosci. Model Dev., 13, 2805–2823, https://doi.org/10.5194/gmd-13-2805-2020, 2020 (code available at: https://github.com/palma-ice/yelmo, last access: 20 April 2026).

Zeising, O., Eisen, O., Hofstede, C. et al. Hard rocks and deep wetlands beneath Thwaites Glacier in Antarctica. Commun Earth Environ 7, 366 (2026). https://doi.org/10.1038/s43247-026-03502-2.

Zhu, J., Otto-Bliesner, B. L., Brady, E. C., Poulsen, C. J., Tierney, J. E., Lofverstrom, M., and DiNezio, P.: Assessment of equilibrium climate sensitivity of the Community Earth System Model version 2 through simulation of the Last Glacial Maximum, Geophys. Res. Lett., 48, e2020GL091220, https://doi.org/10.1029/2020GL091220, 2021.

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 $H_{\mathrm{af}}\left(x=0\right)=0$ at the grounding line by definition, and we can define $H_{\mathrm{af}}\left(x=L\right)=H_t$. 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.

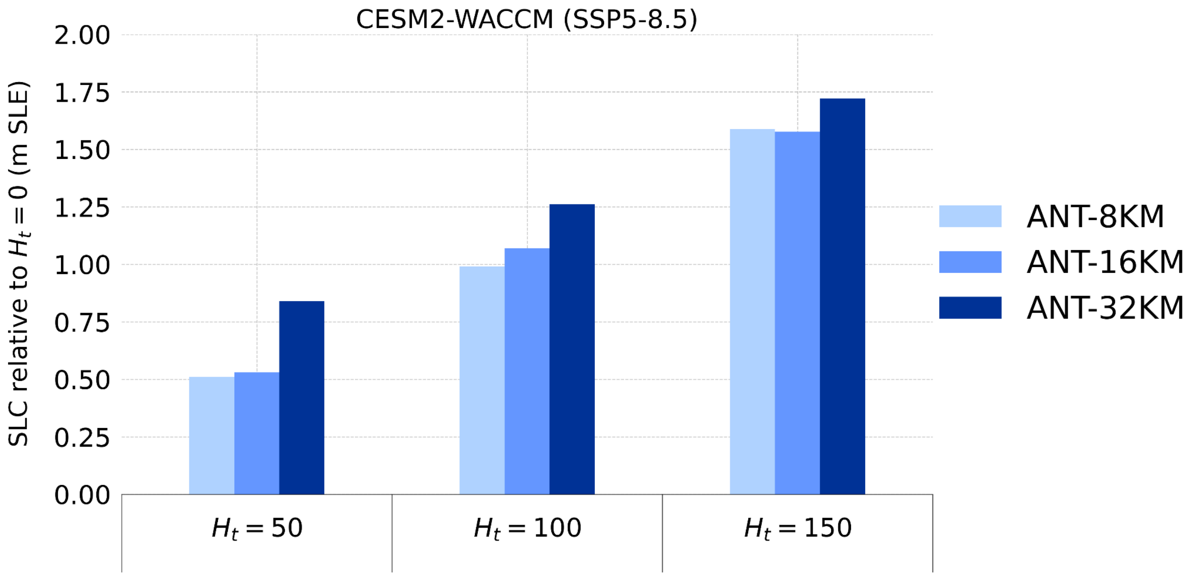

Figure R2-1. SLC relative to Ht = 0 (m SLE) for the three resolutions: 8, 16 and 32 km at year 3000, for different values of Ht, for the CESM2-WACCM climate forcing.

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 → *TF*.

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.

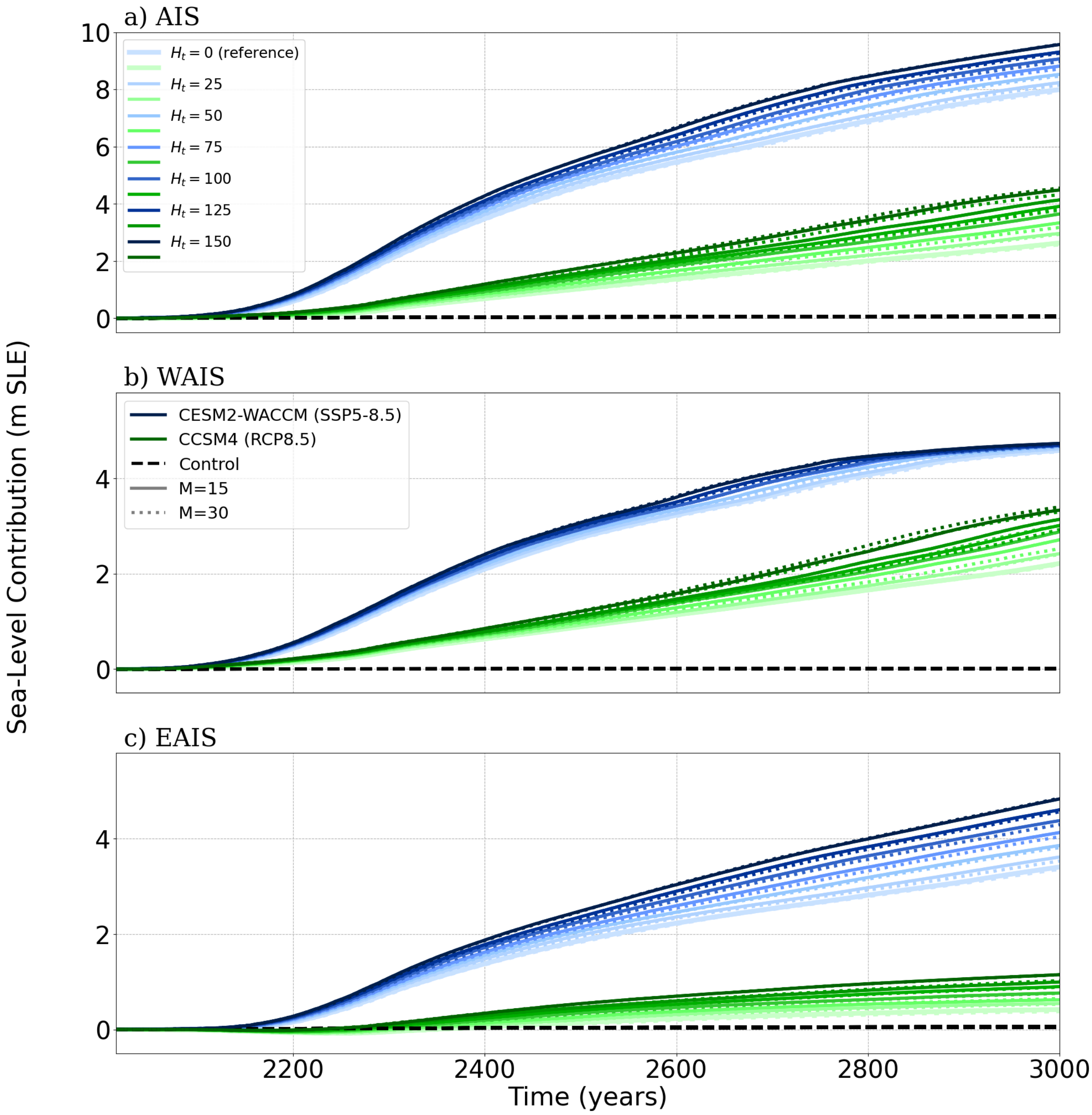

Figure R2-2. SLC (m SLE) comparison using different values of M for the PMPT parameterization.

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.

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Review 3 from "Parameterizing tidal-water intrusions in long-term Antarctic ice-sheet projections"

Antonio Juarez-Martinez et al.

The manuscript shows an interesting way to parameterize GZ melt rates using a single parameter, height above flotation, computed from existing datasets (BedMap3 and BedMachine). They compare the range of values this parameter can take using recent DinSAR observations from various sources. Using the range space, they run multiple projection scenarios using GCM output from SSP5-8.5 (SMB anomalies and ocean forcing) until 2300, keeping it constant thereafter until 3000. They compare the outputs of all these scenarios and show that by 3000, across all scenarios, the collapse of the WAIS and the spread of the EAIS sea-level contribution. They conclude by saying that incorporating melt within the IGZ, can increase the SLC by around 20%. While this work shows an interesting way to incorporate the GZ melt, I would like the authors to address some concerns before suggesting it for a publication:

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

Major Concerns:

The authors use the BedMap3 to estimate Ht (the threshold for height above flotation). When running the projection simulations, they use model data to update the length L (if I understood it correctly). While this seems to be good, as this is the 1st time something like that is done, the authors do not talk about the assumptions that go in using this method such as using the criteria of floatation to pick the grounding line (and not the contact condition), use of stream/shelf model instead of stokes flow model as in the GZ all terms in the stress tensor are important. While I understand that it is not easy to quantify the impact of these assumptions, it is strongly advised to state them in a couple of sentences in the discussion section.

First, we realize that the description of our parameterization was not clear enough. We actually do not directly vary L, which is implicit. We only use the height above flotation Haf and the value of Haf up to which we allow melting (Ht), determining with them the fraction of shelf melt that is applied. Hence, Ht is the only parameter. This formulation implies an inverse relationship of the spatial gradient of Haf to L. For a given value of Ht, the larger the gradient of Haf, the closer to the margin Ht is reached. Therefore a smaller Haf gradient tends to produce a longer intrusion length L. Nevertheless, L can be inferred based on the former relation. We now clarify this derivation in a new appendix, which gives the information needed:

> 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}.

Second, continental ice-sheet models, as ours, generally use the criterion of floatation to define the grounding line rather than the contact condition, but given the fact that processes such as tidal intrusion and ice-shelf deflections are not simulated by such models, even though they are not identical these two methods become equivalent in the practical sense.

Finally, in terms of dynamics our ice-sheet model uses a higher-order approximation (we use DIVA, depth-integrated viscosity approximation; Goldberg et al., 2011; Robinson et al., 2022), which contains more stress terms than zero-order stream/shelf ice-sheet models. Yet, it is indeed not a full-Stokes flow model, subjected then to the limitation that not all stress terms are represented, which is important in the grounding zone. We will add a comment on this limitation in the Discussion.

As someone who has worked on Operation IceBridge (OIB) data on Thwaites Glacier and Petermann Glacier, I note that as ice moves through the ice grounding zone, it falls below flotation and then rises above flotation (refer to Gadi et al., 2025) before reaching flotation. This deflection is a permanent feature of the ice shelf, in addition to short-term fluctuations in surface elevation (±1 m) caused by changes in sea surface height (oceanic tides and atmospheric pressure). Figure 1 is a very simplified representation of this, and it is important to write this somewhere clearly and state the same. And I am not sure whether they see the same kind of behavior using their model data or BedMap3 (I have not used these datasets for this purpose). Using this criteria and defining the weighing function may work for this pertaining study as they do not use a full stokes system, but if you do, from my collection of talking to a colleague (long ago), the model also shows that ice falls below flotation by 1m and then rises by 1m in the flexure zone. Once again, I am not asking the authors to do more runs, I would advise them to write this either in Section 2.2 where they describe the approach or add a paragraph or couple of sentences on this as I think this is important to be said.

Gadi, et al. "Contrasting melt regime in the Ice Grounding Zone of Thwaites Glacier, West Antarctica." Proceedings of the National Academy of Sciences 122.48 (2025): e2512626122.

Thank you very much for the reference to Gadi et al., (2025) which was not known to us and is highly relevant for our study. Indeed, our model does not simulate the ice-shelf deflections in response to tides. We agree on the fact that Figure 1 is a very simplified representation of the real processes taking place. Our parameterization aims to account for the effect that tidal migrations could have on basal melting, and to do so in a way that is tractable for continental-scale modeling. We will include further explanations in the section explaining the parameterization (please note this has been moved following a request of reviewer 1) and in the abovementioned Appendix to further clarify our purposes and the physical concepts that motivate our work. In addition, we will add an explanation on the processes above that are lacking, together with a reference to Gadi et al., (2025).

I have read through the discussion section to see whether the authors address many of the things that were missing. While they seem to write about the subglacial environment, the authors do not discuss the missing ice-ocean feedback, such as changes in ice-shelf shape due to melting and their impact on ocean circulation, which would further affect melting. Once again, I understand it is hard to quantify the impact of leaving these, but a sentence or two on this would be great to caution the readers about the limitations.

We will add a comment to emphasize the fact that our model indeed lacks such feedbacks.

The authors state that, in 193-195, the approach works for Greenland glaciers (two of them). I would like them to caution that Greenland has stronger seasonality than Antarctica and that the approach does not assume any subglacial environmental conditions. It is fine to keep this sentence, but maybe add a statement on this.

We have moved the figure regarding Greenland glaciers to the Supplementary Material following comments by the other referees, but will include a sentence on the larger seasonality.

Minor Concerns:

75-85 It is important to note that the work of Wilson or Robel et al address the intrusions of seawater but not the tidal pumping mechanism; whereas the lines that follow up with observational studies are tidal migrations of GL. A sentence to differentiate this would be good.

Thank you for your comment. We will add a comment to differentiate this.

Section 2.1 has many knobs within the equations 2-3, it would be great if the authors can state which of these they are the most dominant and, how would they think that might influence the projections so that the community can learn from their expertise.

We have added a comment in the new version of the manuscript in the Discussion following referee 1 comment, in which we state that analyzing the sensitivity of all the parameters that we use for ice-sheet modeling is not our intention for this manuscript. Instead, we propose a fixed experimental setup to test only the influence of the proposed parameterization compared to a reference case. Doing a complete analysis of the parameters would be out of scope for this article.

Nevertheless, we think the knobs of our setup rely on the uncertainty in gamma_0, which we have already analyzed in our previous work (Juarez-Martinez et al., 2024). The higher this parameter the more melting is produced. Therefore using a higher value of gamma_0 would influence the total sea-level contribution (SLC) in the projections. Nevertheless, we do not expect to change the results concerning the relative SLC with respect to the reference case (Ht=0).

Regarding basal drag, the availability of different laws that are apparently valid also adds uncertainty to the projections as they affect ice dynamics (Blasco et al., 2021). We will include an explanation of the main "knobs" or parameters that are uncertain and subject to tuning and how their value might impact the projections in the new version of the manuscript.

In equation (3), I do not understand why an all-drainage-based averaged forcing is added to the thermal forcing. Could the authors state a sentence explaining why? Also the choice of constant gamma_0 seems like a constant exchange coefficient, and in literature, it has been noted that this predicts larger melting than velocity dependent parameterization, in which case (if I understand correctly), a sentence on this would be nice

The basal melting parameterization we use is the standard ISMIP6 parameterization (Jourdain et al., 2020; Seroussi et al., 2020). The shelf-average term takes into account the non-local melting dependency on thermal forcing observed in ice-shelf cavities around the AIS. Physically, basal melting at one location modifies the buoyancy of the cavity through the production of relatively fresh meltwater. 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. As a consequence, local melt rates depend not only on the local thermal forcing but also on the cavity-wide thermal state and circulation. The shelf-average thermal forcing term provides a simple representation of these cavity-scale ocean processes, capturing the observed sensitivity of melting to conditions integrated over the entire ice shelf rather than solely to local ocean properties. In summary, sub-shelf melting at a given location induces a circulation that can affect other shelf locations (non-local parameterization). We will add a short sentence explaining the non-local character of the parameterization in more detail.

As to the gamma_0 parameter, it is indeed a constant exchange coefficient as is the case in other studies, including ISMIP6 projections (Seroussi et al., 2020, 2024). We will include a sentence to explain the larger resulting melting with constant gamma_0 than with a velocity dependent parameter (Burgard et al., 2022).

203 I am not sure why BedMap3 was used in Figure 1, and while computing the basal friction coefficient, the BedMachine dataset is used. Is there any specific reason why this was done?

Thank you for the comment. Following a comment by referee 1, we have replaced BedMachine by Bedmap3 and redone all simulations.

200 If I understand correctly, the ice sheet model runs using climatological monthly mean values of atmospheric fields and climatological ocean fields for the run 1979-2019?

The 1979-2019 climatology is used for the atmosphere; these values are used for the spinup procedure to achieve a present-day state of the AIS and initialize from there the model. For the ocean, the present-day climatology (temperature and salinity fields), constructed by Jourdain et al. (2020) for ISMIP6 from existing observational datasets, was used. This combines the NOAA World Ocean Atlas 2018 dataset (WOA18p; Locarnini et al., 2019; Zweng et al., 2019) and the Met Office EN4 subsurface ocean profiles (EN4; Good et al., 2013) from 1995 to 2017, together with the Marine Mammals Exploring Oceans from Pole to Pole (MEOP) dataset record from 2004 to 2018 (Roquet et al., 2013, 2014; Treasure et al., 2017). We will state this more clearly in the manuscript.

215-220 While the atmospheric values are provided as anomalies, the ocean forcing is derived from GCM output annually? If so please rewrite this?

The atmospheric values are provided as anomalies added to the reference values used during the spinup. Regarding the ocean forcing, the datasets from ISMIP6-2300 provide the annual-mean thermal forcing which Yelmo reads, which we add to the baseline in the reference dataset from Jourdain et al., (2020). We will explain this better in the new version of the manuscript.

213 Please mention that by observations, the authors mean BedMachine dataset and not DinSAR-derived velocities? A4 does not state what observations (use Bedmachine dataset) as in A5.

We will clarify this in the new version of the manuscript. Nevertheless, as mentioned before, we have replaced BedMachine with Bedmap3.

225-226 is perhaps interesting as it shows that the ice-shelves become sensitive when the ocean forcing increases with different H_t values; this is important to state somewhere in the discussion.

We understand that the referee refers to the control, which shows no significant change for the different values of Ht. But the ocean forcing does not increase with the different Ht values. The different Ht values allow more or less melting to penetrate upstream. This melting is translated from the oceanic forcing through the Jourdain et al., (2020) parameterization, and is common for all the projections (all the projections use the same file both for CESM2-WACCM and CCSM4).

232 21% compared to H_t=0 case?

Yes, compared to the reference case Ht = 0. We will clarify this point.

245: FRIS is a cold ice-shelf cavity implying lower melt rates, though it has strong tides. If the authors wanted to show that increasing H_t increases basal melting, why not show a warmer ice-shelf cavity such as PIG or thwaites? This is a suggestion.

PIG and Thwaites are also shown in Fig. A7, but the largest cumulative melting (for the forcing used) in this area is seen in Ronne-Filchner. We will mention all three.

248-250 When the authors say that H_t is the most important parameter controlling the evolution, I would suggest that they need to make it clear that this is for the scenario considered and no subglacial environment is considered.

In the next version of the manuscript we will include an additional GCM-forcing (CCSM4, also under RCP8.5) showing this is still the case. However, we will rephrase this sentence in the manuscript to emphasize that we do not permute other parameters, and to note the lack of a subglacial environment in our simulations.

282-284: I am lost with that sentence and would like authors to re-phrase it

We will change this sentence to make it more clear. The point of that sentence is to highlight that the initialization that we have performed (spinup) slightly influences the results at the beginning of the simulations.

306-308: The % reported is for Petermann gletscher, for TGT and TEIS, these numbers are different (50%), so it is important to state that submarine melting does not happen all throughout its length than the %; instead if the authors want to use the %, it would be nice to state the glacier name in that sentence.

We will add the glacier name to clarify the number.

Figure A1: The mean and median for H_af values are…

Thank you for noticing this, we will delete “for”.

Figure A3: Adding labels b-g in panel (a) would make it clear where these locations are in (same as Figure 1)

We will add these labels to make this more clear.

Figure A4: Please write Bedmachine dataset instead of observations

We will add Bedmap3 instead of observations.

Figure A7: Write the location FRIS in the caption

We will indicate in the caption this corresponds to the WAIS and includes the FRIS, the Pine Island Glacier and Thwaites mentioned in the text.

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