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.

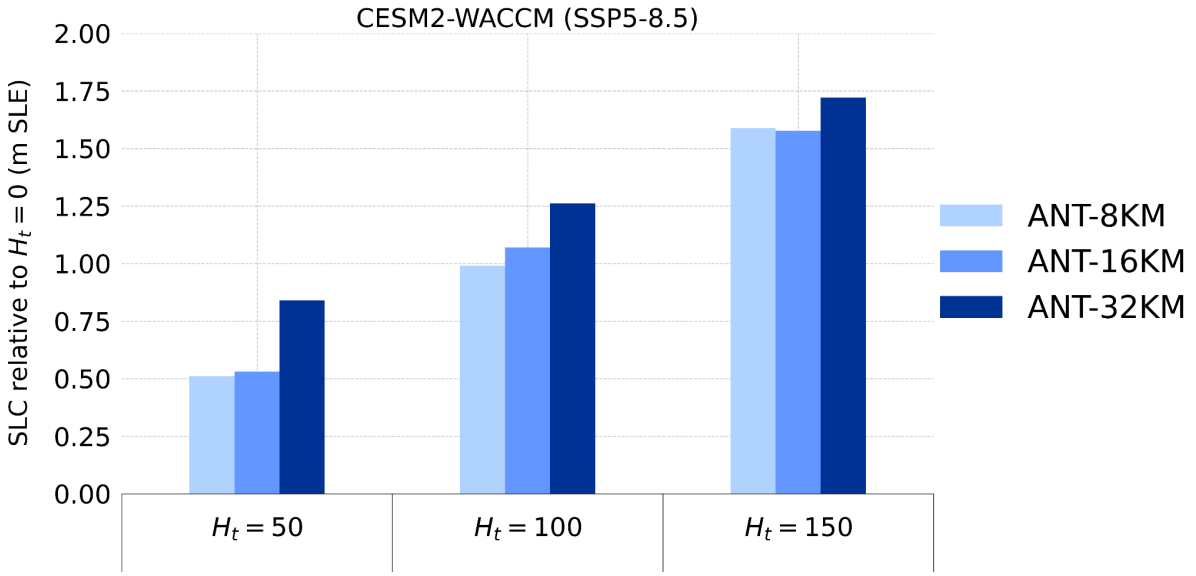

Figure R1-1. SLC relative to Ht = 0 (m SLE) for the three resolutions: 8, 16 and 32 km at year 3000, for different values of Ht, for the CESM2-WACCM climate forcing.

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.

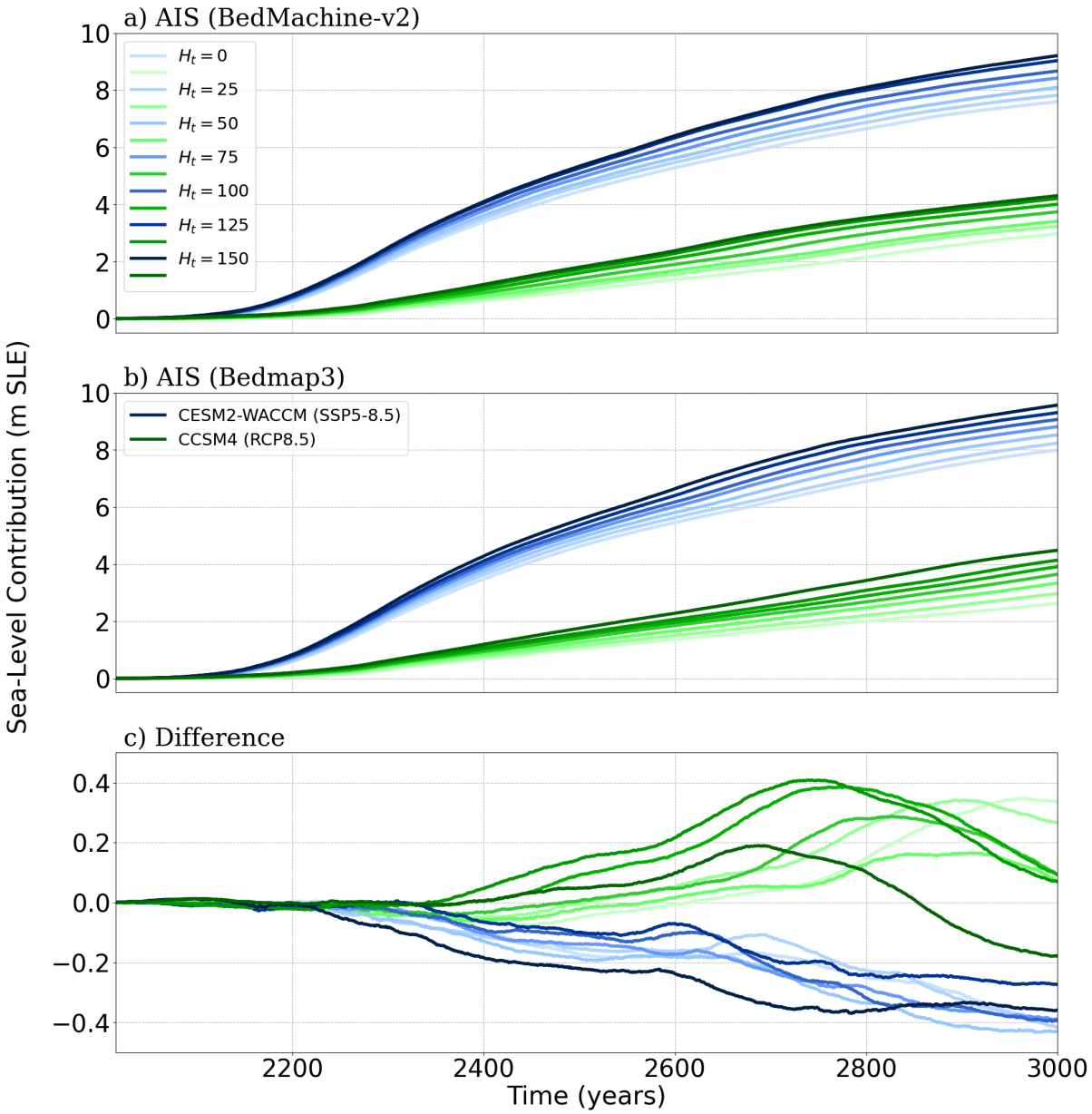

Figure R1-2. Sea-level contribution (SLC; m SLE) for the AIS using BedMachine-v2 (a) and Bedmap3 (b) as topography dataset. In c), absolute difference between a) and b).

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.

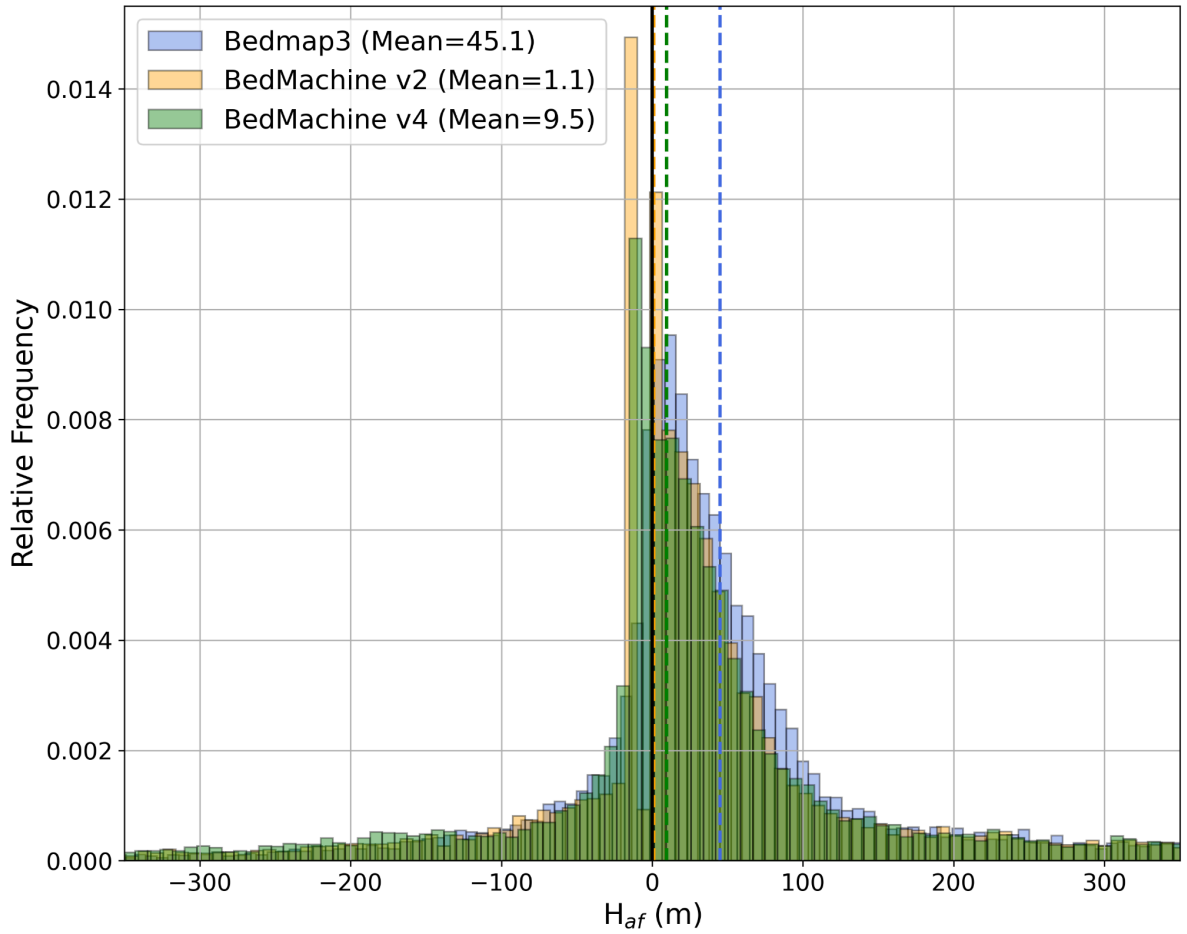

Figure R1-3. Histograms of Haf (m) for three different topography datasets (Bedmap3, BedMachine v2, and BedMachine v4), using all the six GZs regions from the manuscript. Dashed lines indicate the average value for each topography.

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.

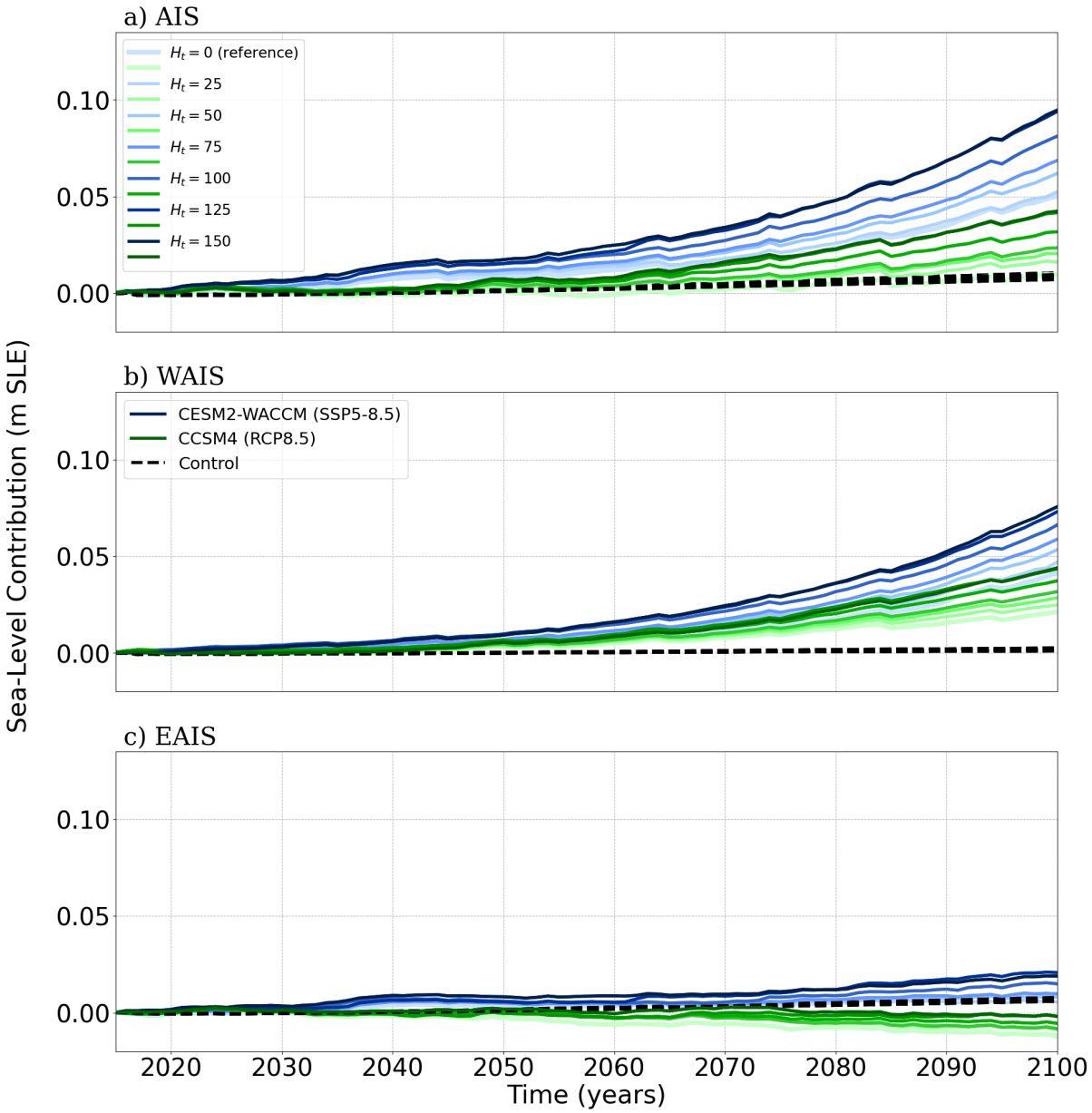

Figure R1-4. SLC (relative to 2015, m SLE) for the different values of Ht (in m) used until 2100. In a) for the whole AIS, in b) for the WAIS, and in c) for the EAIS. Dashed lines represent the control simulations corresponding to the different values of Ht, overlapping as variations are very small during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

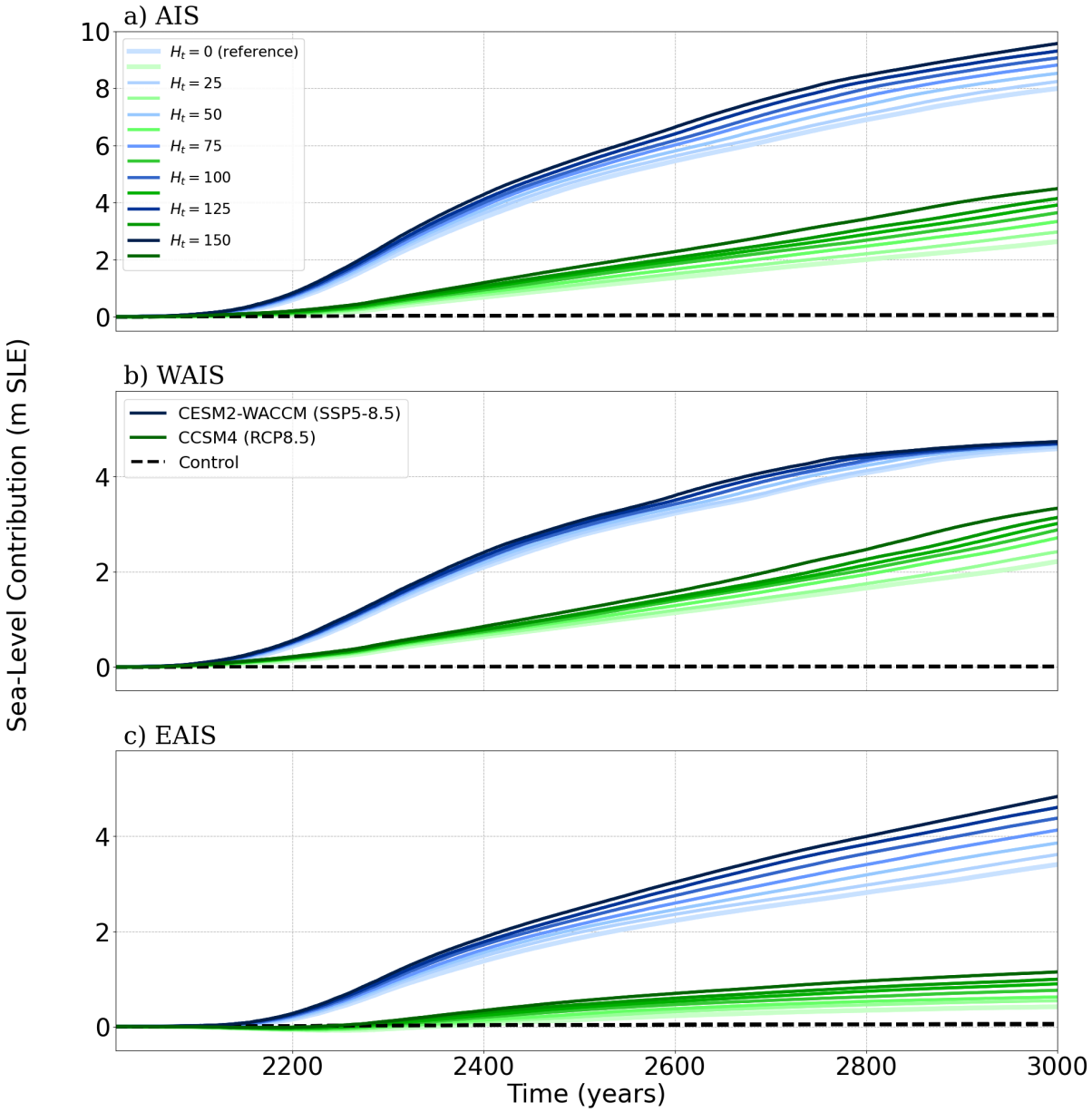

Figure R1-5. SLC (relative to 2015, m SLE) for the different values of Ht (in m) used for CESM2-WACCM (blue) and CCSM4 (green). In a) for the whole AIS, in b) for the WAIS, and in c) for the EAIS. Dashed lines represent the control simulations corresponding to the different values of Ht, overlapping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

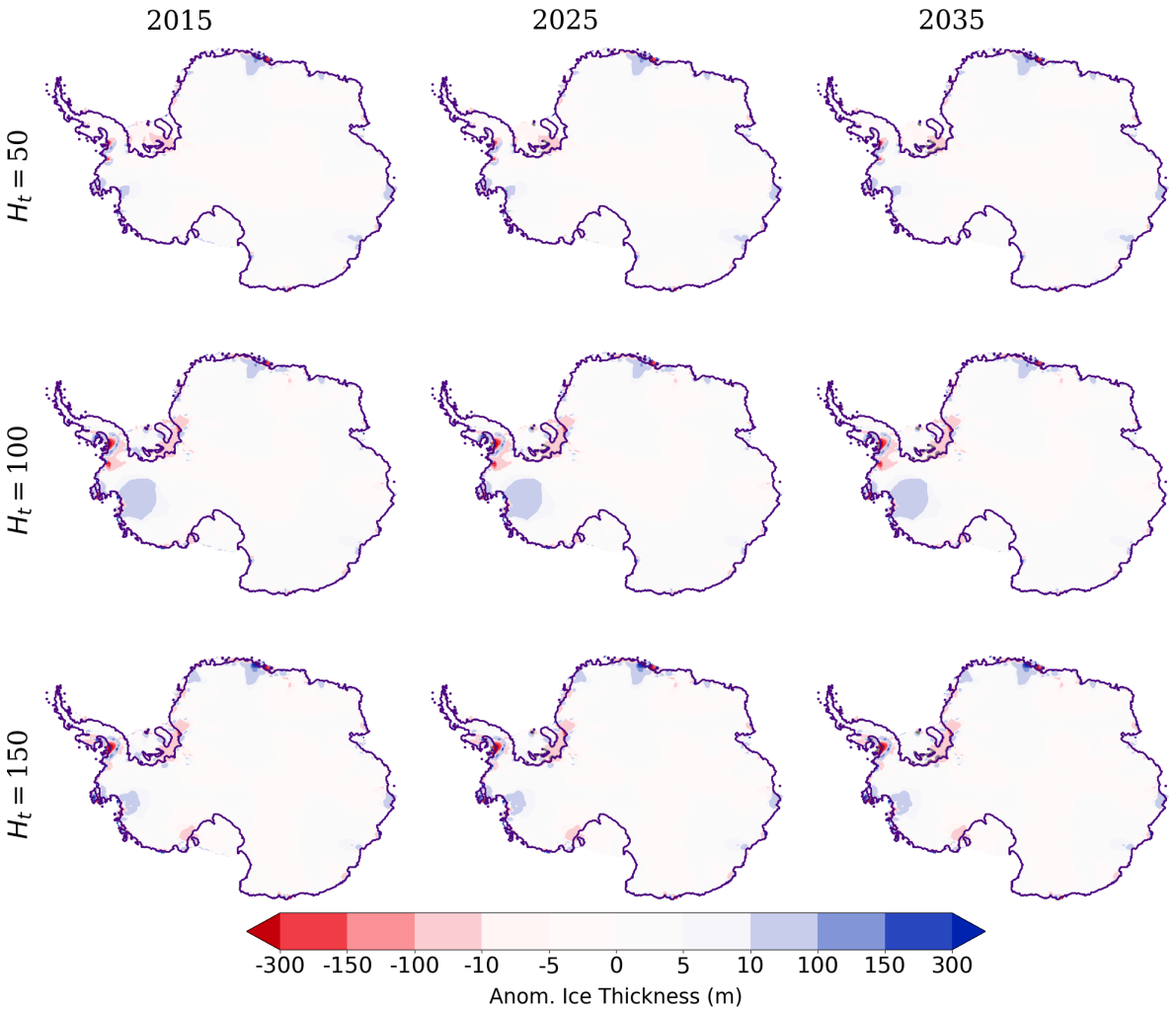

Figure R1-6. Same as Figure 4 in the manuscript, but for the initial years. In particular, the first column shows the anomalies in the initial state.

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.