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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