Author's response: review (2) of the manuscript: "Seasonal variability in Antarctic ice shelf velocities forced by sea surface height variations" by Mosbeux et al.

This paper describes the effects of including annual sea-surface height changes when modelling the flow of ice shelves. By changing the height of the ice shelf, two changes to the flow are considered, firstly that raising the ice surface reduces the sea-ward driving stress and slows the flow, and secondly that raising the base of the ice causes the grounding line to retreat land-ward and reduces the basal drag, allowing the flow to accelerate. These contributions are quantified through the use of Elmer/Ice and the authors find that the change in grounding line position has a large impact on ice shelf velocity. It is a nice idea for a paper. But the models of grounding-line position are not correct for the timescales involved.

Looking at figure 2, the authors seem to be considering elastic flexure at the grounding line to be a major component of the ice shelf response, so that the direction of the surface perturbation close to the grounding line is opposite to that over the majority of the shelf (it is not clear what "relative uplift", l.307, actually means - relative to what? but I take it this is the effect being sketched). I cannot see any indication in figure 4 that this occurs - as the authors state, seasonal variations are much slower than the Maxwell timescale for ice, so the viscous relaxation should outweigh any elastic flexure, so the response of the shelf will primarily be that due to hydrostatic balance. In any case a rise in mean sea surface height should correspond to an inland migration of the grounding line (as stated in l.313); I should like to see figure 2 redrawn to remove the implication that the opposite occurs.

This brings me to my major concern - that the authors are using models for grounding line migration that were developed for a very different timescale, on which ice behaves primarily elastically. They attempt to justify this by reference to a paper that also uses this models for fortnightly behaviour - but that is an order of magnitude closer to the Maxwell timescale than the seasonal variations are. Elastic stresses within the ice will be negligible on seasonal timescales. I cannot really see why anything except hydrostatic balance would be appropriate here, and I cannot support publication of this paper while model (ii) is being given serious consideration.

We thank the Reviewer for their nice and clear summary of our paper and for acknowledging the originality of our work. We also thank them for their important insights concerning the limitations of our elastic parameterization of the grounding line migration. We agree with them that the elastic model is inappropriate to use on seasonal timescales; our intent, however, was to attempt to bridge the gap between short-term (tidal-band) SSH effects where elastic is clearly dominant, and long-term SSH trends where viscous deformation dominates. We intend to leave it in as a high end-member of the possible effect of SSH variations on the ice flow, while being transparent about its limitations. We propose to make this clearer in the manuscript by including a few hypotheses that could explain the larger reduction in basal shear stress in the grounding zone needed to reproduce the observations of seasonal flow variations. The most important point is that the fully hydrostatic parameterisation leads to a small migration, whatever other assumptions we make.

Concerning the effect of SSH variations on the grounding line migration, the Reviewer is correct that Figure 2 depicts two effects at the same time: (1) an uplift/lowering of the ice front due to a relative positive/negative gradient of the SSH anomaly and (2) a downstream/upstream migration of the grounding line due to the negative/positive SSH anomaly at the grounding line. Figure 4 shows that these effects often combine. For example, over the period January-March, we see in the Tinto et al. (2019) model that $\Delta SSH > 0$ close to the ice front while it is <0 over most of the ice shelf and at the grounding line. We think that the sketch is representative of the seasonal variability of the general mechanism we are observing for RIS (phasing of the two forcing mechanisms may vary on other ice shelves), with or without accounting for the elastic flexure of the ice shelf. However, it is true that we cannot assert that one effect necessarily leads to the other (e.g., a positive ΔSSH at the ice front does not necessarily imply a negative ΔSSH at the grounding line). We propose to make this clearer in the caption of Figure 2 by adding: *"Notice that the effects of ice shelf slope and grounding line position (positive/negative ΔSSH at the ice front and negative/positive ΔSSH at the grounding line) do not always act in the same direction, and sometimes their effects cancel. In that case, the net effect either accelerates nor decelerates the ice flow, depending on which effect is stronger".*

Our large grounding-line migration assumes elastic flexure of the ice shelf under the SSH load, following Tsai and Gudmundsson (2015). Without this larger migration or, more precisely, without the equivalent large change in basal shear stress in the grounding zone, the modelled ice flow response is smaller than what we observe in the GNSS time series; however, it is still significant, as shown by the hydrostatic parameterisation B2 in Figs. 7 and 8). Our argument is that, even if the ice shelf tends to go back in a few days to a hydrostatic position after a perturbation of the sea surface height (ΔSSH), the perturbation on the subglacial-network (wet and slippery bed/tills) could last much longer and weaken the basal shear stress for a longer period. Therefore, the elastic parameterisation of the grounding line can be viewed as a proxy to model a plausible range of seasonal variation of the subglacial hydrologic system and the associated basal shear stress variation.

We understand the Reviewer's concern, and agree that our large scale migration model was missing some important context and justification. We still stress (both here and in the revised manuscript) that this parameterisation is a high end-member of the potential impact of ΔSSH on the ice flow. We agree that we should explore other mechanisms that could explain the large variations in basal shear stress needed to reproduce the observed amplitude of ice flow change. We describe <u>two interlinked explanations</u> below and plan to add them to the manuscript:

1. Our first explanation is linked to the relatively low value of the basal friction coefficients we inferred at the grounding line during the model initialization. Our model initialization relies on the optimization of the friction coefficient C in the Eq. (2) of the manuscript. We write this equation here:

   $$\boldsymbol{\tau}_b = C\,|\boldsymbol{u}_b|^{\frac{1}{m}-1}\,\boldsymbol{u}_b\ ,$$

   with C being the friction coefficient, $\boldsymbol{u}_b$ the sliding velocity, and exponent $m \in [1-\infty]$ where increasing values of m are characteristic of a more plastic bed. This law does not include a direct dependency on the effective pressure (like a Coulomb law would; e.g., Brondex et al., 2019; Urruty et al., 2022). However, as the friction parameter C is determined through inversion, it should include the dependency on the effective pressure and reduce the value of C at the grounding line to match observations (e.g., Urruty et al., 2022). The inferred friction represents an <u>average annual value</u> of the friction coefficient. The distribution of the seasonal variation around this annual average cannot be exactly determined without a proper knowledge of the subglacial hydrologic system, which is not realistically represented in ice-sheet modelling. However, one can assume that the variation could be larger than the variation we estimate through our hydrostatic parameterisation (i.e., a change in C directly proportional to the grounding line migration ΔL). Seawater intrusion at the ice-bed interface and in sediments has been shown to have a high impact on the ice flow response (e.g., Robel et al., 2022). Subglacial models depending on subglacial water pressure decrease the effective pressure significantly near the grounding line, leading to an increased sensitivity for a given power in the sliding law (e.g., Kazmierczak et al., 2022). Although the consequences of ΔL on this effective pressure is difficult to estimate, we believe that incorporating this mechanism in our modelling would lead to a larger impact of ΔSSH on the ice flow, even for the purely hydrostatic case.

Seawater intrusion could also be enhanced by a highly retrograde slope (e.g. Byrd Glacier in Fig. R2 of this Response to Reviewers). Retrograde bed slope will enhance both the migration of the grounding line and the intrusion of seawater in the subglacial hydrologic system.

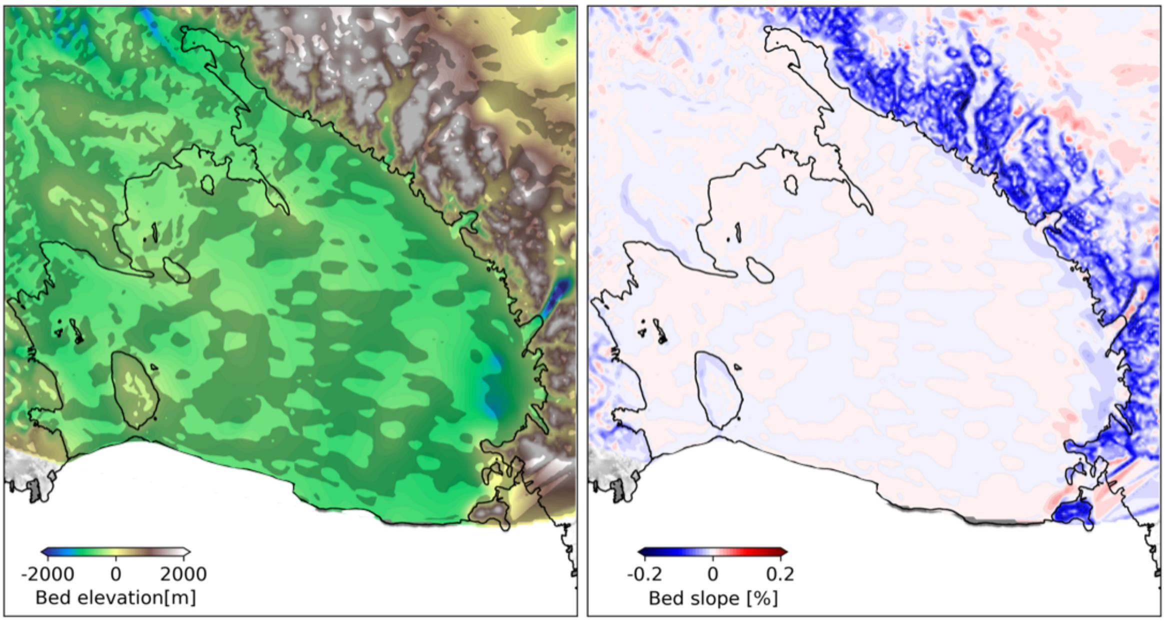

Figure R2. (a) Bed elevation with highlighted retrograde slope (sloping upward in the flow direction). (b) Grade (blue) and retrograde (red) bed slope in percentage (%). The mapping is based on the Bedmap2 dataset by Fretwell et al., (2013) and the direction of the ice flow computed during the initialisation phase.

2. Our second explanation is directly linked to the potential effect of the grounding line migration on the subglacial water system. Such a mechanism would assume that *SSH* can vary over a short period (i.e. a few days) with a longer lasting effect on the subglacial hydrologic system in the grounding zone. We added a figure below that shows how the perturbation can evolve over September 2016 in MetROMS (Naughten et al., 2018; Fig. R3 of this Response to Reviewers). Each of these snapshots are separated by about 6 days, which is on the order of Maxwell time for ice, where both elastic and viscous effect matters. We can see that, while the average SSH of the month is positive in Naughten et al. (2018), the model rapidly switches from a low positive – low negative anomaly to a much stronger positive anomaly. This rapid change can lead to large elastic migration of the grounding line, in agreement with other studies (e.g., Tsai and Gudmundsson, (2015; Rosier et al.2015, 2016, 2020; Warburton et al., 2020). We recognise that in our case, the model does not switch back as fast to a negative anomaly as it would for a tidal loading. However, even if the grounding line quickly relaxes to its hydrostatic position due to the viscous relaxation, the perturbation of the hydrological system and the consequent weakening of the basal shear stress could last longer and extend over a long distance upstream the grounding line. Full treatment of the subglacial water system is out of

the scope of this study, but could help validate our theories in the future. To some extent, this weakening is parameterised in our model.

Our large scale parametrisations of the grounding line migration ($\Delta L_C$and ΔL_{B2L}) tend to accommodate these two hypotheses, with intrinsic limitations to the exercise. We made this clearer in the manuscript by adding some of the details we gave here and emphasized that the basal drag change at the grounding zone is both a parameterisation of the grounding line migration itself and of the potential effect of the migration on the subglacial water system.

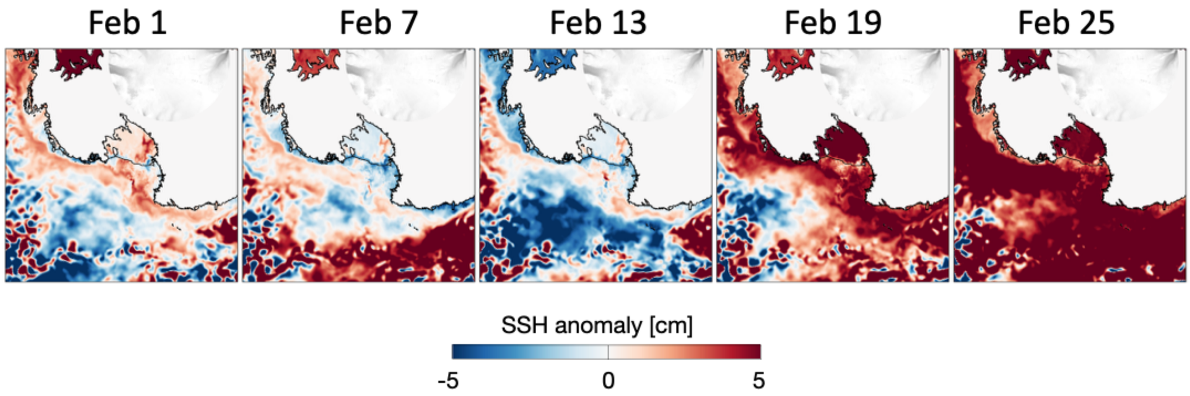

Figure R3. Snapshots of daily ΔSSH (with respect to annual mean) in September 2016 in MetROMS (Naughten et al. 2018). The snapshots are for the 1st, 7th, 13th, 19th, 25th of the months.

We are grateful that the Reviewer raised this concern, as we think it will considerably strengthen our manuscript. We believe that our explanations and planned modifications to the manuscript will address most of the Reviewer's concerns.

A further concern regarding the model for hydrostatic grounding line position being used (since this is rather key to the remaining results) - the result of the Tsai and Gudmundsson paper, that downstream migration is 9 times less than upstream migration, assumes that the ice surface gradient is constant across the grounding line, while the gradient in ice thickness changes abruptly (by this factor of 9). If one makes the opposite assumption, that the ice thins uniformly through the grounding zone (e.g. Sayag and Worster 2011, Warburton et al. 2020), then the hydrostatic migration distance is completely symmetric. With access to all the data needed to test these assumptions, I would be more reassured if the authors calculated the hydrostatic migration distance "from scratch", rather than wholesale apply this massively idealised formula.

Thank you for raising this source of misunderstanding. The main point of the hydrostatic migration is that it always leads to small grounding line migration, whatever the assumption you make when parametrizing it. The parameterisation we use in the manuscript is an asymmetric hydrostatic grounding line migration (Equations B1 and B2), and does indeed come from the fact that we use the same theory as Tsai and Gundmundsson (2015) (their equations (1)-(3)). For completeness, and to clarify our approach for the Reviewer, we rewrite here the trigonometrical construction on which this parametrization relies, as well as our understanding of the Warburton et al. (2020) assumption. The steps are as follows:

(i) At the grounding line, the ice is lifted due to floatation and the upward buoyancy force in the water column is compensated by the downward gravitational force in the ice column:

$$F_i = F_w \Leftrightarrow \rho_i\, g\, H = \rho_w g\, h_w = \rho_w (z_{SL} - z_b),$$

where z_{SL} is the sea level and z_b is the bed elevation.

(ii) Adapting Tsai and Gudmundson (2015), upstream the grounding line (GL), we can approximate the bed elevation at the point of migration of the GL (zb,L) by:

$$z_{b,\Delta L} = z_{b,GL} + \beta\, \Delta L,$$

with the bed slope (equal to the ice base slope if located upstream the GL) and L the GL migration we try to estimate. Similarly, the ice thickness upstream the grounding line can be estimated as:

$$H_{\Delta L} = H_{\mathrm{GL}} + (\alpha - \beta)\, \Delta L\ ,$$

(iii) From there, we can rewrite:

$$\frac{\rho_i}{\rho_w}(H_{GL} + (\alpha - \beta)\, \Delta L) += \Delta SSH - Z_{b,GL} - \beta \Delta L\ ,$$

and estimate

$$\Delta L^+ = \frac{\Delta SSH}{\frac{\rho_i}{\rho_w}(\alpha-\beta)+\beta}\ .$$

(iv) For the downstream migration, as the Reviewer stated, our assumption leads to a reduction of the ice base slope by a factor $1/(1-\frac{\rho_i}{\rho_w})\sim 9$ and therefore a potential grounding line migration:

$$\Delta L^- = \Delta L^+ \times (1 - \frac{\rho_i}{\rho_w}).$$

(v) In Warburton et al. (2020), a constant thickness (named D in their manuscript, second line of sec. 2.1) is assumed. We can read "*For simplicity, we consider a constant ice thickness D across the grounding zone*" in their manuscript. If we are correct, this means that they assume no thinning through the grounding zone. In this case, and given a constant bed slope, the Reviewer is right that there would be no asymmetry in the grounding line migration but this would also mean that upstream the grounding line, $\alpha = \beta$. The upstream migration would therefore be:

$$\Delta L^+ = \frac{\Delta SSH}{\beta},$$

and a similar downstream migration $\Delta L^- = \frac{\Delta SSH}{\beta}$ only if we assume that the bed is constant both upstream and downstream the grounding line.

(vi) In the end, it is true that both assumptions are relatively idealized, and in fact both yield a similar result. In the case where we assume $\alpha = \beta$, the Tsai and Gundmundson parameterisation leads to:

$$\Delta L^{+} = \frac{\Delta SSH}{\frac{\rho_i}{\rho_w}(\alpha-\beta)+\beta} = \frac{\Delta SSH}{\beta},$$

similarly to Warburton et al. (2020). The asymmetry on L- in Warburton et al. (2020) depends on the bed slope downstream the grounding line. Assuming a constant bed slope, there would be no asymmetry and the bed would be

$$\Delta L^{-} = \frac{\Delta SSH}{\beta}.$$

(vii) In our parametrization, the Reviewer is correct that we assume the surface and bed slope to be constant in the grounding zone. Tsai and Gudmunsson mention the fact that *"average surface and bed slopes are potentially different immediately upstream versus immediately downstream of the grounding line, the differences are unlikely ever to be ~10 times different, so this result suggests that grounding-line migration over the positive part of the tidal cycle (high tide) dominates the migration over the negative part of the tidal cycle (low tide)".* We think that this is especially true for small migrations such as the ones of our hydrostatic model ΔL_{B2} , i.e. about a few tens of meters except in some areas of the Siple Coast and some Trans-Antarctic glaciers (Figure R4a of this R2R), a length scale under our model resolution and on which we do not expect large surface and bed slope variations.

We agree that this is a limitation of our model and we will add this to our discussion. In the end, the ΔSSH-induced migration of the grounding line and the associated basal shear stress change of the hydrostatic case are small, with the Siple Coast also exhibiting a relatively low basal friction, limiting the effect of the migration and basal shear stress change on the ice flow (Figure R4b of this R2R). We emphasize to the reader about the small effect of ΔL_{B2} in our manuscript: "*We regard the ΔL_{B2} parameterisation, which yields small grounding line migration, as an approximation of ice shelf response to SSH gradients alone."* We will also add Figure 4 to the supplementary material to show the effect of our parameterisation. We hope that our explanations as well as this Supplementary figure will alleviate the Reviewer's concerns.

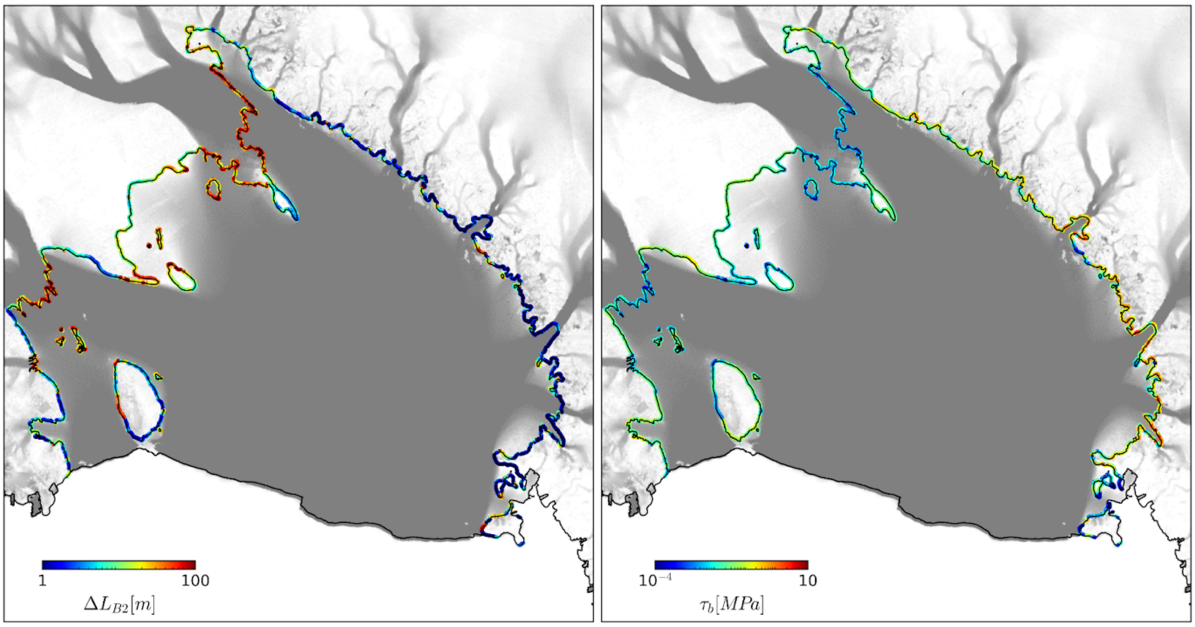

Figure R4. (a) Migration ΔL_{B2} of the grounding line for $\Delta SSH = 5 \times 10^{-2}$ m. (b) Basal shear stress τ_b at the grounding line averaged over the ensemble of simulation Ω_{15}.

Smaller comments

Given the inherent non-linearities of ice shelf dynamics (and indeed grounding line motion), to what extent is it valid to compare the average of a function (mean velocities over a month) to the function of an average (ice shelf model forced by mean SSH)? The authors could consider applying the same process with much more of the daily signal kept in the forcing, and then average the output over a month, to see if this differs from the model output from the monthly average.

We agree with the Reviewer that complex processes related to ice shelf dynamics, changes in bed lubrication and grounding line migration, as well as tidal effects, are at play in the mechanism we observe and model. However, these non-linearities are hard to account for, and doing so is well beyond the goal of this paper. As observed by Warburton et al. (2020) in the context of tidal grounding-line migration: *"The origin of this nonlinear response of the surface velocity remains enigmatic".* This is also why we relied on a simpler parameterisation of the grounding line migration (and basal shear stress change).

For clarification, in figure 9, is this one set of simulations per month, with a continuous line drawn between these points, or is the model forced with daily values of a monthly running average?

Yes, the Reviewer is right. It is one set of simulations per month with a continuous line drawn between these points. For clarification, we will add dots for each month in the revised figure.

Additional references

- Feldmann, J., Albrecht, T., Khroulev, C., Pattyn, F., and Levermann, A.: Resolution-dependent performance of grounding line motion in a shallow model compared with a full-Stokes model according to the MISMIP3d intercomparison, J. Glaciol., 60, 353–360, https://doi.org/10.3189/2014JoG13J093, 2014.
- Felikson, D., Nowicki, S., Nias, I., Morlighem, M., & Seroussi, H.: Seasonal tidewater glacier terminus oscillations bias multi-decadal projections of ice mass change. Journal of Geophysical Research: Earth Surface, 127, e2021JF006249, https://doi.org/10.1029/2021JF006249, 2022.
- Kazmierczak, E., Sun, S., Coulon, V., and Pattyn, F.: Subglacial hydrology modulates basal sliding response of the Antarctic ice sheet to climate forcing, Cryosphere Discuss., 1–24, https://doi.org/10.5194/tc-2022-53, 2022.
- Larour, E., Seroussi, H., Adhikari, S., Ivins, E., Caron, L., Morlighem, M., and Schlegel, N.: Slowdown in Antarctic mass loss from solid Earth and sea-level feedbacks, Science, 364, https://doi.org/10.1126/science.aav7908, 2019.
- LeDoux, C. M., Hulbe, C. L., Forbes, M. P., Scambos, T. A., and Alley, K.: Structural provinces of the Ross Ice Shelf, Antarctica, Ann. Glaciol., 58, 88–98, https://doi.org/10.1017/aog.2017.24, 2017.
- Robel, A. A., Seroussi, H., Roe, G. H.: Marine ice sheet instability amplifies and skews uncertainty in projections of future sea-level rise, 116, 30, 14887-14892, Proceedings of the National Academy of Sciences, https://www.pnas.org/doi/full/10.1073/pnas.1904822116, 2019
- 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.
- Urruty, B., Hill, E. A., Reese, R., Garbe, J., Gagliardini, O., Durand, G., Gillet-Chaulet, F., Gudmundsson, G. H., Winkelmann, R., Chekki, M., Chandler, D., and Langebroek, P. M.: The stability of present-day Antarctic grounding lines – Part A: No indication of marine ice sheet instability in the current geometry, Cryosphere Discuss., 1–34, https://doi.org/10.5194/tc-2022-104, 2022.
- Warburton, K. L. P., Hewitt, D. R., and Neufeld, J. A.: Tidal Grounding-Line Migration Modulated by Subglacial Hydrology, Geophys. Res. Lett., 47, e2020GL089088, https://doi.org/10.1029/2020GL089088, 2020.