Authors' Response to Reviews of

Impact of the Nares Strait sea ice arches on the long-term stability of the Petermann Glacier Ice Shelf

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RC: ***Reviewers' Comment***, AR: Authors' Response

1. Response to Reviewer #2

AR: Our response is structured as follows:

- Reviewer's Comment (RC) in bold italics.
- Authors' Response (AR) in regular font.

1.1. Overview

RC: ***The manuscript describes the output from a regional, three-dimensional numerical model of the ocean. It includes two lateral boundaries in the north and south where water properties (temperature and salinity) and velocity vectors are specified and complex coastlines. The model geometry includes a floating ice shelf (Petermann Gl.) as well as realistic bottom topography both under the floating glacier and everywhere else within the modeling domain. The presence of ice (sea ice and glacier ice) is prescribed (not modeled) as a surface boundary condition with a surface stress (vertical momentum flux) condition that depends on the prescribed (not modeled) ice concentration and ice velocity. The ice is allowed to melt because of conductive and turbulent heat flux from the ocean to the ice. Much of this model set-up and selective comparison to limited data is published as Prakash et al. (2022). Reading this publication, I consider essential to critically evaluate the present manuscript. I recommend against publication for the following three major at least 10 minor reasons:***

AR: We thank the reviewer for taking the time to read through our work and for providing feedback. Below, we provide a rebuttal and argue why we believe our manuscript should be considered for publication.

1.2. Major Comments

RC: ***A. Prior ice-ocean models of the region exist (Shroyer et al., 2015; Shroyer et al. 2017) and substantial overlap exists with few new or original results. Unlike the present model, the prior models include a prognostic (as opposed to a prescribed) sea ice model. Basal melt rates are predicted under the floating portion of the glacier by both present and prior models. Both present and prior models evaluate the differences between sea ice in the adjacent ocean and fjord that is mobile vs sea ice that is not mobile. I cannot discern fundamentally new results or insight in the present model application that basically repeats prior modeling with a different model, that, I feel, has more weaknesses than strength over prior models.***

AR: Thank you for the comment. Indeed, prior ice shelf-ocean models of the region exist (Shroyer et al., (2017)). Shroyer et al. (2017) presented the response of PGIS basal melt to seasonal shifts in sea ice cover (landfast in winter to mobile in summer). Their paper was one of the many papers that provided motivation for our work, as has been acknowledged several times during the course of this study.

However, instead of modelling the seasonal transitions, we model the response of PGIS basal melt to long-term large scale shifts in the regional sea ice cover, including investigating the impact of sea ice thickness as an additional, and (based on our findings) an undeniably relevant parameter. The formulation of major comment A indicates to us that some misunderstanding (eg. regarding the treatment of sea ice/the IceNudge module) may have occurred that may have led to misinterpretations and eventually, conclusions that should be revised once the initial misunderstanding is sorted out. The IceNudge module gives us the ability to prescribe sea ice regimes reminiscent of late 1990s-early 2000s to present/likely future. By doing so, we were able to investigate the response of PGIS basal melt to climate warming induced long term regime shifts in the regional sea ice cover. We have stitched up these shifts together by combining information from the extensive literature on regional sea ice cover from early 2000s until 2022 (as described in l. 93-114 in the methods chapter, and further discussed in l. 433-437 and l. 468-475). The upwelling of AW in the Nares Strait under mobile ice is consistent between the two models. The mechanism put forward in Shroyer et al. (2017) is also seen in our model (also evidencing the capabilities of the Ice Nudge module). This, to our knowledge, is the only overlap, as has been acknowledged. However, even here, we have convincingly demonstrated that the effect of this upwelled AW on PGIS basal melt, which is restricted to the outer (<200 m) regions of the ice shelf in Shroyer et al. (2017) is incorrect (and is also known to contradict modelled and observed findings reported in Cai et al. (2017) and Washam et al. (2020)). This major caveat and explanations behind it have been detailed in our manuscript (see l. 452-461), but seems to have been overlooked in the review process. Similarly, it seems to have been overlooked that our model features a realistic ice shelf basal topography (lacking in Shroyer et al. (2017)), a much improved sub-ice shelf bathymetry (lacking in Shroyer et al. (2017) which also includes a laterally resolved inner sill which was mapped in 1-D by Tinto et al. (2015) (lacking in Shroyer et al. (2017)), and allows us to vary sea ice thickness, which is highly relevant for PGIS basal melt (also lacking in Shroyer et al. (2017)).

We believe that the strength of our model, with all amendments and improvements implemented, is evidenced, among others, by the fact that we are able to provide a novel disentanglement of the drivers of melt (thermal driving vs. friction velocity). Here, contrary to established views which have attributed increase in basal melt to increased oceanic heat supply to the fjord, we show that melting under the deeper regions of the PGIS base under a (negligibly) thin sea ice cover is dictated by shear driven turbulence, without the need for any noticeable increase in thermal driving. This, too, seems to have been overlooked in the review process.

RC: ***B. The present model uses temperature and salinity at the boundaries that are both poor and unrealistic. This "cold bias" is prominently discussed in Prakash et al. (2022), but it is introduced in the present manuscript almost as an after-thought in passing on Line-375. Moored and synoptic observations indicate salinity and temperature of about 34.7 psu +0.2 C for the Atlantic-influenced waters both in Petermann Fjord and below the ice shelf (Muenchow et al., 2016; Washam et al, 2018). The model provides "… annual mean temperature and salinity of the water masses that overflow the inner sill… " that are 34 psu and -0.7 C (Line-230). So, the modeled ocean is almost 1.0 C too cold and 0.7 psu too fresh at a depth where seasonal and interannual variations are less than 0.1 C and 0.01 psu! So, the model includes huge biases that I find unacceptable. Note that Prakash et al. (2022, page-27, Line-6/7) " … suggest applying a depth-dependent bias correction to the upstream A4 T-S fields such as in Shroyer et al. (2015)." Please follow your own suggestion.***

AR: Thank you for providing an observational angle in comment B. We have acknowledged that there is a disagreement between modelled and observed θ-S values, which has now been addressed. Revised results from corrected far-field boundary conditions (with modelled θ-S from the fjord consistent with observations) and Γ_T value (now 0.018 as compared to 0.05 from before) can be found in our response to Reviewer 1 (as those were not requested here). We would like to stress that one of the strengths of our model is that it can accurately describe the relevant mechanisms of PGIS-ocean interaction (as is discussed in Prakash et al. (2022)). Departing from such a base, additional corrections can indeed be implemented if the nature of a specific study requires so (e.g. aiming at modelling "realistic" conditions), but they will not change the nature of modelled PGIS-ocean interaction, nor have a major impact on the conclusions drawn from the modelled results, as is clearly stated in Prakash et al. (2022), and as is evidenced in our new results from the bias-corrected runs. As mentioned above, we feel that if some aspects of the model development advances had been fully acknowledged, the issues raised in comment B may have appeared as minor, rather than major in nature.

RC: ***C. The present model attempts to compensate for the "cold and fresh bias" by artificially increasing the vertical turbulent exchange coefficient to produce melt rates that are somewhat agreeable to observations of melt rate. So, the model includes a wrong (northern) boundary condition (from the nested A4 model) and a wrong "turbulence model" to make things right. From my observational perspective two wrongs rarely make a right and I thus loose trust in this model as, it appears, the model can "nudge" or "fix" anything as there is always a parameter or dial that one can change to obtain any desired result. I do not endorse this practice, especially since the authors know the proper way to remove these biases and perhaps use realistic turbulent exchange coefficients.***

AR: Thank you for this honest statement. We recognize the problem of non-existing scientific trust between researchers working with numerical models on the one hand, and observational data on the other hand (and which is not restricted to the field of oceanography).

We experience that the interpretation of our tuning process that has been put forward in comment C is not entirely factual and hence take the opportunity to provide clarification in the following. Only few estimates of basal melt rates exist at PGIS, those that are either representative of the annual mean melt (thus lacking seasonal response), or are acquired over sparse sampling sites over a few months for a given year (thus lacking both spatial (2-D melt maps) and temporal (year-round) variability). When drawing comparisons, differences between the model and observations also need to be considered. These include inconsistencies between the model sea ice regimes and period as compared to the reported estimates. In addition, similar to Shroyer et al. (2017), our experiments do not include subglacial discharge at the GL. These factors limit direct comparisons, and, it is imperative to highlight the context in which our modelled melt rate values should be interpreted, as the objective of our study isn't to provide (quasi-)realistic values of PGIS basal melt (akin to remotely sensed/ observational estimates). Given the incomplete picture of PGIS basal melt rate estimates and model limitations, the objective concerning tuning of the modelled basal melt rates was to arrive at a qualitatively reasonable contemporary modelled (reference) melt rate for a given set of far-field ocean boundary conditions. Departures from the reference values are numerically robust, that is, there exists no artificial (numerical) oscillations or spurious variability in our modelled results. As suggested by Reviewer 1, we will provide documentation of model stability in detail in the revised manuscript. Physically, departures from the reference values solely reflect how long-term changes in regional sea ice cover influence PGIS basal melt; while other mechanisms such as melt rate increase through subglacial discharge and its interplay with sea ice cover changes falls beyond the scope of this work, but needs to be considered in addition when assessing the response of PGIS basal melt to a future warming climate. We sincerely hope that our clarification provides an opportunity to learn from each other, independent of the approaches and tools we usually follow and adopt.

1.3. Minor Comments

RC: ***In addition to the above major weaknesses, there are a number of more minor concerns***
1. The manuscript is too long and unnecessarily complex. I could not discern much substantial difference between the "Thick-Mobile" and the Thin-Mobile" case. What differences there are, I feel, may fall into the domain of model uncertainty, noise, and poorly constrained observational and/or model parameters.

AR: Regarding the length of the paper – we agree, and will shorten it wherever deemed necessary, in a revised version. Regarding the thick vs thin mobile cases, we believe that Figure 8 convincingly demonstrates the significant impact that the sea ice thickness has on PGIS basal melt. We further believe that attributing the differences to model uncertainty, noise and poorly constrained parameters lacks factual grounds.

RC: ***2. The manuscript is too long and contains trivial co-ordinate transformations. Just state that co-ordinates are rotated into along- and across-channel components rather than spelling out what I perceive as trivial algebra (Lines-143 through Line-182). I understand that the present model is finite element that that this may not be as trivial computationally as it is in finite-difference models or observations.***

AR: Thank you for the comment. This was put in the main text as to our knowledge, these diagnostics are not available yet for FVCOM anywhere in the public domain. However, accessibility is guaranteed irrespective of whether it is included in the main text or an appendix to it, so we will move it to the appendix.

RC: ***3. I am confused and disturbed by streamlines that start at boundaries and end in the interior. Figure-3 offends my sense of mass conversation and lateral boundary conditions. Would you not expect a zero velocity along the coast?***

AR: Thank you for the comment, and for noticing this. We realized that we had forgotten to transform the velocities (u,v) from the model output to u_x and v_y when generating this Figure, and thus, the streamlines were not forming a closed contour. This has been addressed now, and the revised plot has been presented in Sect. 2.

RC: ***4. Figure-4 has large spatial oscillations that result from a poorly used graphics package. Please learn how to draw contours properly. Furthermore, I prefer distances in km as opposed to Longitude. The strong bottom-intensified slope current under the western ice shelf during the summer (Fig.-4b) caught my attention. Strangely, no such current exists in the fjord where bottom slopes may be similar, well, I cannot tell, because Longitude rather km is used for distance.***

AR: We believe that an inclusive way of communicating science should be adopted, and have therefore generated our figures (including contours) on Matlab using colorblind friendly packages. Both the software and the packages used to generate the figures are widely used and accepted and generally appreciated by the scientific community. Nevertheless, this has been revised (see Sect. 2). Which units are used to express distances will perhaps always be a matter of personal preference. We have chosen to stick to geographical coordinates. Regarding "strong bottom-intensified slope current" in Greenland fjords, please see and compare against Figure 4 (mean flow normal to a cross section – similar diagnostic as Figure 4 here) in `https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC014435`, which clearly shows bottom intensified (inflow) slope current; similar to our results. Note also, the similar depth and width (slope).

"...In the fjord mouth (Sections 4 and 5) the mean flow is weaker and intensifies with depth, with current cores of around 15cm/s concentrated against sidewalls at around 400 m depth..." -Fraser et al., 2018.

RC: ***5. On Line-310 it is stated that "... upwelled AW [Atlantic Water] from the adjacent Nares Strait enter the fjord... " How does this work? Would not winds from north to south in Nares Strait that may cause the AW to upwell along Greenland in the east also lower sea level along Greenland relative to Canada? Would***

then Petermann Fjord not respond with a large outflow the way an estuary would with lower sea level at is mouth?

AR: Thank you for the comment. As described in the manuscript, the exchange between PF and Hall Basin is facilitated by the cyclonic gyre at the fjord mouth. Also, as stated in the manuscript, and also in the comments above, please see Shroyer et al. (2017) where this mechanism was first modelled and detailed.

RC: ***6. Please provide uncertainty estimates to your estimates of heat fluxes such as summarized in Table-2 as it is done with observational estimates of these same fluxes.***

AR: Thank you for the comment. Our model is of deterministic type. Uncertainties could be quantified if ensemble runs were conducted, either with the same model under a range of parameter variations, or with different models, using the same parameters. However, this is beyond the scope of our work. Note that international model intercomparison projects exist which address this issue in more depth.

RC: ***7. In my view the authors mistakenly equate heat flux into the fjord and/or glacier cavity with basal melting (Line-368, Line-373). First, most of the heat flux into the fjord leaves it. Only a small fraction of the heat is actually used to melt the ice which in the present model is largely caused by the artificially increased vertical turbulent exchange coefficient. Second, did the authors actually check, if mass is conserved? Does the volume flux add to the amount of melting?***

AR: We experience that the issues raised here result from missing what is already stated in lines 481-482 in the manuscript, evidencing that we do not equate heat flux with basal melt. Thus, the first raised concern dematerializes. We can also confirm that mass is conserved. Returning to the challenge of bridging the world as seen in models and through observations (see our reply to major comment C), we are pleased that our net heat flux is consistent with the observation based findings reported for PF by Johnson et al. (2011) and Heuze et al. (2017) (also detailed in the manuscript). More importantly, large amount of heat entering and leaving the fjord without triggering melt is also consistent with Johnson et al. (2011) and Heuze et al. (2017).

Please see Johnson et al. (2011) who clearly state this on multiple occasions in their paper. Here we highlight some noticeable occurrences:

"However, melting of the ice shelf does not seem to be limited by the amount of heat entering the fjord, with more heat currently available than is required to effect the observed melting (see sections 3 and 7.2)...." ... "These observations suggest that other factors, yet to be determined, besides oceanic heat input must also play a role in controlling the melt rate."

Similar has been reported by Heuze et al. 2017:

""Since in both the 2009 and 2015 surveys we saw that the availability of ocean heat did not seem to be the factor limiting the melt rate of the ice shelf."

Not only do these findings lend substantial credibility to our modelled results, they also explicitly state that "...other factors, yet to be determined, besides oceanic heat input must also play a role in controlling the melt rate". Thus, the increased melt that is seen in our results as the sea ice becomes thin and mobile, dictated by enhanced shear driven turbulence, is certainly not an artifact, particularly after we have clearly shown that our results are valid post correction as well (i.e. with a reduced "realistic" Γ_T that relates to the "realistic" fjord temperatures). As stated before, the retention of the validity of our results was expected. Rather, as we have indicated in our manuscript, this is a novel finding of our study.

RC: ***8. The comparison of model predicted basal melt rates with observations appears to me less systematic than it could be. The authors appear to pick whatever value from whatever paper for whatever season that fits***

their purpose. Perhaps this part of the discussion can be strengthened by more clearly delineate seasons, space, and vastly different observational techniques (snapshots vs. moored observations or remotely satellite vs fixed radar stations).

AR: Thank you for the comment. Please see major comment 2 above, where this has been appropriately addressed. We also suggest reading the revised version of l. 412-426 which has been improved following the feedback provided by Reviewer 1 (Major Comment 2).

RC: ***9. Line-438/439/440: The Rueckamp et al. (2019) reference clearly states that the "still attached" new ice island has already "dynamically detached," that is, from a practical or physical perspective, this segment of the ice shelf is already gone.***

AR: We are not sure whether any kind of action is expected in reply to this comment. We disagree with the interpretation given in the comment, but would like to state that we focus on modes of melt at PGIS, which will hold/remain relevant for the post-future calving geometry as well. Our results clearly imply that irrespective of where the PGIS calving front is positioned, a considerable part of the outer (shallower) region of the ice shelf will be subjected to intense basal melting during summer, if the sea ice, in addition to being mobile, thins.

RC: ***10. Line-511: The "Nares Strait sea ice arches" (mobile sea ice) add 2 m/y basal melt to the 24 m/y (Line-243), so the entire paper is about a 10% effect. The supply of heat to the fjord or glacier cavity matters little, but both the vertical turbulent mixing and the grounding line discharge of freshwater (not included in this model) may dominate over this 10% effect.***

AR: Thank you for the comment. The paper is about more than a 10% increase. However, we realized that we have not made this explicitly clear. Thank you for bringing this to our attention. This can also be addressed when presenting our results regarding PGIS basal melt rates across the three experiments. Please see an explanation below:

As stated in the major comments, we investigate how long-term large scale shifts in the regional sea ice regime have impacted PGIS basal melt. Thus, the most pertinent comparison would be to contrast the PGIS averaged winter mean basal melt under the thick landfast sea ice cover (most conservative/representative of late 90s and early 2000s scenario) against the PGIS averaged summer mean basal melt under the thin mobile sea ice cover (present/likely future scenario). Note that this is also analogous, at least in a way (since it does not include sea ice thickness as a parameter), to how Shroyer et al. (2017) draw their comparison (landfast in winter vs mobile in summer). To that end, we find a twofold increase in melt. Furthermore, PGIS averaged summer mean increase between TKL and TNM is 18%, and annually, as pointed out here, it is 10%.

Regarding the speculation on subglacial discharge that has been put forward here, please note that these mechanisms should not be viewed as competing processes. Rather, the discharge enhanced buoyant meltwater plumes are likely to act in concert with the loss of landfast and thick sea ice cover to further strengthen the overturning circulation and entrain more AW in the PGIS cavity during summer (Caroll et al., 2015; Cai et al., 2017). Thus, the modelled summer mean melt rate anomalies presented here are likely lower bounds of the true anomalies that should be expected in the presence of subglacial discharge.

2. Figures

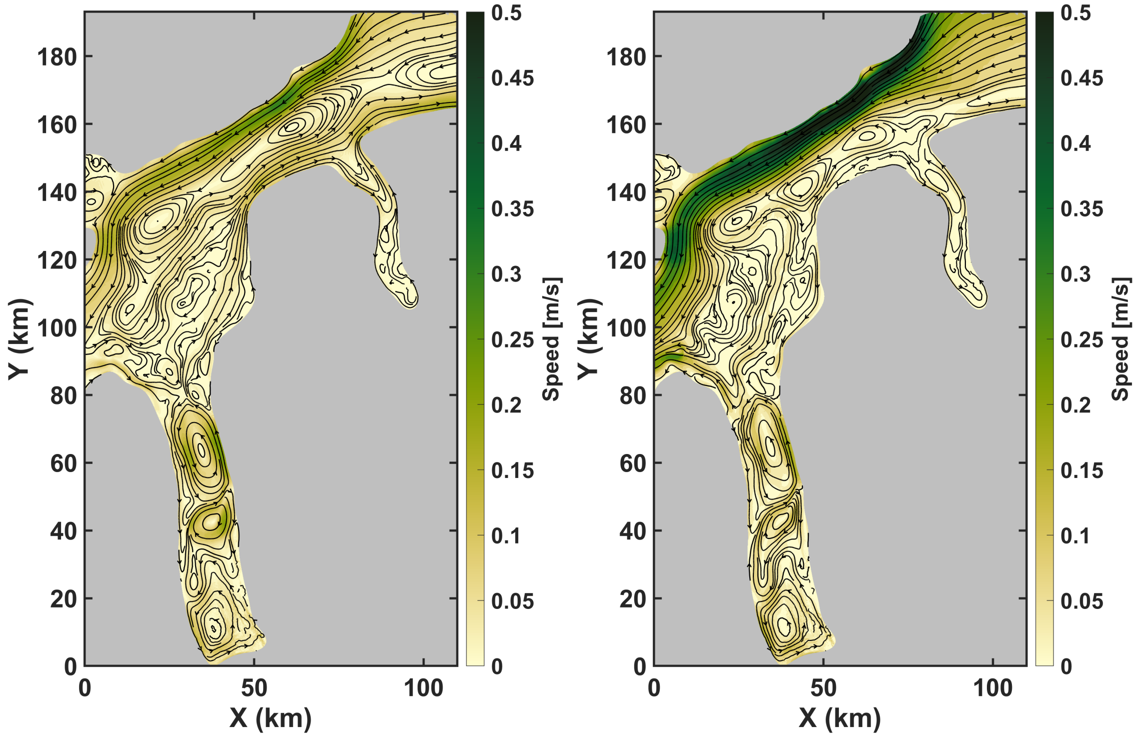

Figure 1: Revised Figure 3. Winter = left. Summer = right.

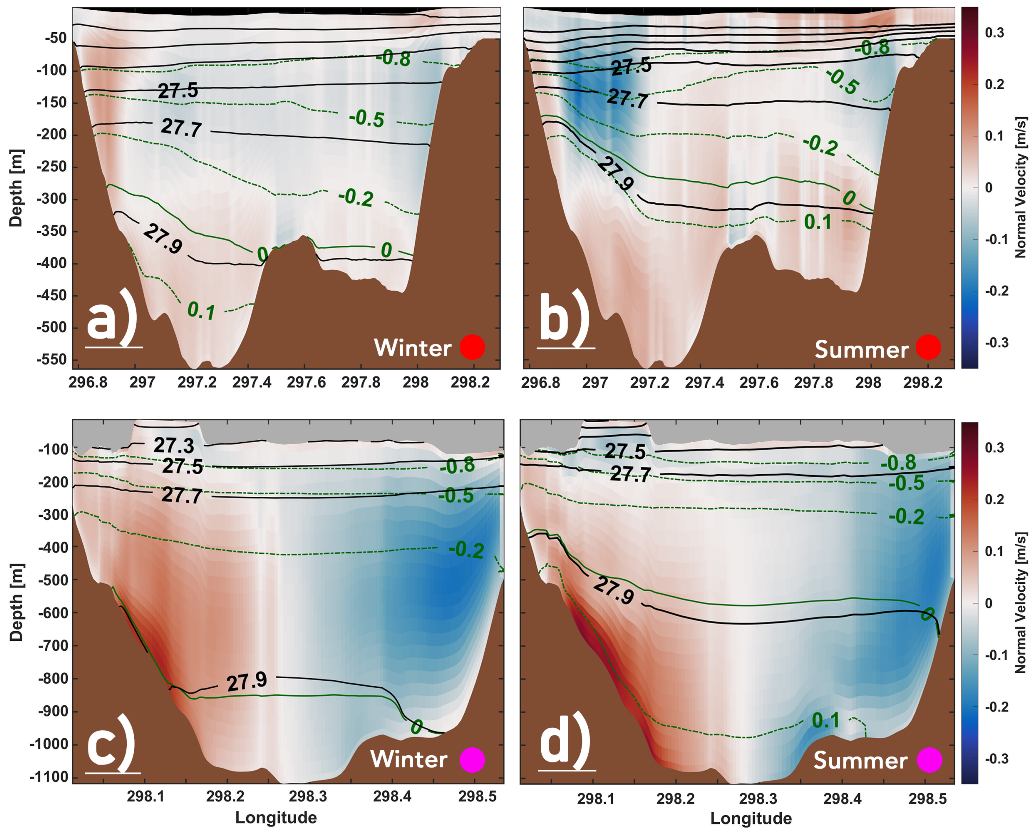

Figure 2: **Revised Figure 4**. Left column = winter. Right column = summer. Panels (a) and (b) = fjord mouth section. Panels (c) and (d) = calving front section. Results are obtained from the new bias-corrected runs.

3. References

Below, we provide references that have been included in addition to those that are already provided in the manuscript.

1. Cai C, Rignot E, Menemenlis D, Nakayama Y. 2017. Observations and modeling of oceaninduced melt beneath Petermann Glacier Ice Shelf in northwestern Greenland. Geophysical Research Letters 44:8396–8403.

2. Carroll, D., Sutherland, D. A., Shroyer, E. L., Nash, J. D., Catania, G. A., & Stearns, L. A. (2015). Modeling turbulent subglacial meltwa-ter plumes: Implications for fjord-scale buoyancy-driven circulation. Journal of Physical Oceanography, 45(8), 2169–2185.

3. Fraser, N. J., & Inall, M. E. (2018). Influence of barrier wind forcing on heat delivery toward the Greenland Ice Sheet. Journal of Geophysical Research: Oceans, 123, 2513–2538.

4. Washam P, Nicholls KW, Münchow A, Padman L. 2020. Tidal modulation of buoyant flow and basal melt beneath Petermann Gletscher Ice Shelf, Greenland. Journal of Geophysical Research: Oceans 125:e2020JC016427.