Author response to Reviewer 1 - Basal melt rates and ocean circulation under the Ryder Glacier ice tongue and their response to climate warming: a high resolution modelling study

Jonathan Wiskandt, Inga Monika Koszalka, Johan Nilsson

MS No.: egusphere-2022-1296
MS type: Research article

Please find below our response to the comments of reviewer one to our manuscript. The reviewers comments are written in cursive, our response in regular font. Within our suggested texts, changes are marked as crossed out for deletions and in bold for additions.

1 General comments

I enjoyed reading the submitted paper by Wiskandt et al. They applied MITgcm to an idealised ice shelf cavity representing the ocean circulation under the Ryder Glacier tongue. This is the first time this ice shelf cavity has been modelled and this study benefits from new observations outside of temperature and salinity outside the ice tongue to force the model.

I think the authors do a good job of justifying their modelling strategy based on similar previous studies of ocean flow near tidewater glaciers. I think it would be beneficial discuss the sensitivity of grid resolution more, particularly in terms of the vertical diffusivities and vertical grid resolution as I expect that changing these parameters could have a quantitative, if not qualitative, effect on their results. An interesting result arising from their experiments is that at higher temperature forcings the change melt rate has a linear response to temperature forcing whereas at lower temperature forcing. Similar results have been obtained for vertical ice faces at tidewater glaciers and at the ice shelf at Petermann Glacier, but I think the authors use their simulations to good effect to explain how this response appears from the simulations and how it links to previous studies of colder Antarctic ice shelves. I learnt a lot thinking about plume detachment that I am not so familiar with and this seems to be the crux of why there is a transition between the melt rate response. I might be missing something but I would appreciate more of an explanation of Figure 4b.

The results on subglacial discharge ties in with other studies in the literature. The layout and the story of the paper is very well written. This is my first review, so I would be interested to see what the other reviewers think, but I think this is a nice paper.

- Thank you for your review of our manuscript and your comments—they were very helpful in clarifying several aspects of the results and improving the manuscript in general. We address the comments and detail the alterations (in boldface) in the manuscript in line with your suggestions below.

2 Specific comments

1. Line 57. *Re floating ice tongues providing buttressing – not that important maybe, but should this explicitly say laterally constrained ice shelves (i.e with fjords/valleys etc). I think the point of the Gudmundsson 2013 paper was to say that if you have a laterally unconfined ice shelf it has no effect on buttressing.*

   - Thank you for pointing this out. Our intention with citing [Gudmundsson, 2013] in the Introduction was to give a general reference for the buttressing effects of the ice shelves without delving into details as we do not resolve nor analyze these effects in detail in our study. [Gudmundsson, 2013] argues that the ice-shelf buttressing is a complex process and that the result about a laterally unconfined ice shelf having no effect on buttressing is specific to 1HD reduced version of the full Stokes system; for full Stokes system this statement is not longer valid, or at least not under all circumstances. We will clarify the citation by changing this sentence to the following:

"**Under certain conditions,** floating ice tongues **can** stabilize these glaciers by **changing the stress balance and** reducing the ice discharge across their grounding lines, an effect known as buttressing (Gudmundsson, 2013)."

2. Line 65. *Is it worth maybe mentioning the phrase 'ice-pump' around here?*

   - The sentence in line 65 is meant to introduce and describe the melt driven estuarine circulation in Greenland's glacial fjords (the previous two paragraphs and following paragraphs in the Introduction focus on GrIS). We think that adding the detail about the ice-pump, i.e. refreezing, is not necessary given that there is no evidence of refreezing in our setting, it is a more common feature at colder Antarctic ice shelves. For clarity, as this sentence mentions Atlantic Water (AW) and by this refers to Greenland's fjords, we suggest instead to add a reference to Straneo and Cenedese (2015):

   "The basal melt beneath the glacier ice tongue acts as a buoyancy source, driving a rising buoyant plume that forms an outflow of glacially-modified water at its neutral density level. The entrainment into the plume drives an inflow of AW towards the ice base, establishing an estuarine circulation **(Straneo & Cenedese 2015)**".

3. Line 74. *Should this contain the Holland et al., 2008b reference, since this is one of the main papers about melt rate scaling quadratically with temperature forcing?*

   - Yes, that paper is a good addition here. We will add it.

4. Line 75. *I think these models were of vertical ice faces found at tide water glaciers. Is it worth mentioning that here because you've already that ice shelves are quite different to tidewater glaciers on line 55?*

   - Correct, the cited papers that reported on a linear dependency considered the Greenland's tide-water glaciers with vertical ice fronts, which also means that they investigated warmer ocean temperatures (i.e., a stronger thermal forcing) compared to the Antarctic studies which tend to show a super-linear dependency. This is in line with our results about a linear dependence of melt rates for warmer ocean temperatures (higher thermal forcing, see Fig. 4). We will add this detail here, changing the sentence to:

   "**The** modelling studies **considered with melt rates at the Greenland's tidewater glaciers with vertical ice fronts and exposed to relatively high oceanic forcing due to warm AW**, however, simulate a dependency that is not significantly different from a linear one (Xu et al., 2012; Sciascia et al., 2013)."

5. Line 88-90. *Would it help to have another general reference for water properties in the Arctic? Maybe also for the permanent sea ice cover outside Ryder Glacier.*

   - We will add in this line a comment that the stratification found in SOF by [Jakobsson et al., 2020] is typical for the Greenlandic fjords and support it by a reference to [Straneo et al., 2012]) (which we already cited in line 43 where the Atlantic Water in the Arctic and the two-layer stratification structure is introduced):

   "The hydrographic profiles show a two-layer ~~like~~ stratification **typical of Greenlandic fjords (Straneo et al 2012)**, with a cold (about -1.5^o C) and relatively fresh (salinity below 34 g kg^{-1}) surface layer (typical of Polar Surface Water, PSW) and a warm (0.2^{o}C) and salty (34.7 g kg^{-1}) layer of AW below 350 m."

   Regarding the sea-ice cover, the reference for this information is [Jakobsson et al., 2020] (Page 3): "Available moderate resolution imaging spectroradiometer (MODIS) satellite images dating back to 2001 reveal that large icebergs, calved from the ice tongue of Ryder Glacier, remain trapped inside the fjord because the prevailing sea ice at the mouth prevents their exit.” . We will add the reference to the sentence about the sea-ice cover:

”SOF is narrow ($\sim$ 10 km) rendering effects of the Earth’s rotation negligible on the circulation, and a permanent sea ice cover outside of SOF inhibits wind-driven water exchange between the fjord and the open ocean [**Jakobsson et al., 2020**]”.

6. Line 101. *The first time I read about the sills I hadn’t realised these were outside the domain. (i.e outside the ice shelf). Reading it again (and looking at the pictures Jakobsson et al., 2020) it makes sense but maybe this could be more explicit. Maybe you could also add a similar sentence from your conclusion suggesting that you don’t know about sills beneath the ice (if that is the case?) that might also have an effect on warm water reaching the grounding zone?*

   - Jakobsson et al (2020) conducted a bathymetric mapping of the fjord which showed two sills located outside the cavity, there are no sills beneath the ice. We will reformulate sentences 100-101 to clarify that the study focuses on the circulation in the ice cavity excluding the sills situated outside the ice cavity, i.e. outside our model domain:

   This study presents **results from a series of** high-resolution ocean-circulation model simulations of basal melt and ocean **circulation in a cavity below an ice tongue** ~~flow in a fjord with an ice tongue~~. The model geometry is idealised, but its qualitative features are selected to be representative for RG and SOF. ~~Note that SOF has two sills, which are not represented here. This is because the present focus is on flow and melt beneath the ice tongue, which are only indirectly affected by the sills: they primarily control the features of the AW reaching the ice tongue.~~ **Note that SOF has two sills outside the ice cavity, so they are not considered in model simulations presented here. The impact of the sills that control properties of AW reaching the ice cavity is a subject of a follow-up study.**

7. Line 109. *I think MITgcm is a finite-volume discretisation.*

   - This is true, we will correct it.

8. Line 125. *Did you try varying the resolution of your model set up? In particular I imagine your vertical resolution might affect the melt rates obtained based on papers like Gwyther et al 2020. It seems like other papers that this work builds on (Xu et al 2013, Cai et al 2017) also didn’t vary resolution but I think there would be more confidence in the results if you ran another a set of simulations that were 2x or 10x coarser in the horizontal/vertical and keep the parameters the same (provided the model still runs robustly) and see if it has a qualitative effect on the results.*

   - Thank you for this very important comment. First of all, the results about the high sensitivity to vertical resolution from [Gwyther et al., 2020] are not directly transferable nor relevant to our study because they considered other ocean model families implementing different types of vertical grids and (terrain following ROMS, z-coordinate model COCO without partial cells, and MPAS-O with the terrain-following top coordinate) and much lower horizontal resolution than our MITgcm (2 km vs 10 m). Their results about sensitivity is caused by their different implementations of the ice-ocean boundary layer (”tracer sampling distance” and ”flux mixing thickness”, varying between these models) while the MITgcm uses the grid cell closest to the ice-ocean interface to calculate the melt rate. The high sensitivity to vertical resolution for COCO (z-coordinate) model can partly stem from its (low) horizontal resolution (see Figs. 3b and 7b). The sensitivity to diffusivity is not considered in [Gwyther et al., 2020]. Moreover, the models also followed the ISOMIP+ protocol which implies that their simulations are 3D (with spatial structure of melt rates and thermal driving in X-Y direction, Figs. 1-2). Further, horizontal pressure gradient errors specific to terrain-following models could influence the results. This is therefore difficult to separate the vertical resolution-dependent response from other factors present in these different models. So there is no obvious reason to expect that a z-coordinate, partial-cell MITgcm in our 2D configuration will show a similar sensitivity of the melt rates to the vertical resolution as in [Gwyther et al., 2020].

   On the other hand, the sensitivity to the model resolution and viscosity/diffusivity was considered in the two MITgcm studies of similar resolution to ours, Scascia et al (2013) and in particular, by Xu et al (2012) who varied the (horizontal in their vertical plume case) model resolution by a factor of 10. Both studies concluded that while the plume got better resolved and the average melt rates increased for higher resolution, the general circulation pattern and qualitative results about e.g., dependency of melt rates on TF and SGD were consistent between the different simulations. On the other hand, the magnitude of the average melt rates was shown to depend on other factors e.g., the friction coefficient [Dansereau et al., 2013], which was used by [Cai et al., 2017] to tune the model to the observed melt rates, rather then the model resolution or diffusivity.

However, we do agree that the question about the sensitivity to the resolution is very important. Note that our configuration exhibits a relatively very high vertical (3,33 m) and horizontal (10 m) resolution compared to other ice sheet-ocean modelling studies and the values of viscosities/diffusivities are similar to Sciascia et al (2013) and at the same time as low as possible without generating numerical noise or model instability, as is often the practice [Kantha and Clayson, 2013, Cowton et al., 2015]. The values of the vertical diffusivities are moreover on the lower range of the observed in the ocean. So our study could serve as a benchmark to exploring the sensitivity to lower resolution/higher viscosity/diffusivity values, but because of the complexity of the problem we deferred it to a separate study. One important aspect is that the MITgcm is a z-coordinate model that employs partial cells while the ocean temperature and velocity values entering the parameterization are taken from the cell closest to the ice-ocean boundary. This implies that not only the vertical but also the horizontal resolution (aspect ratio of the each grid cell) will impact the melt rates, and there is an interplay between the vertical/horizontal resolution and vertical/horizontal diffusivity, which you also refer to in your later comment 11. Second, the salinity tendencies due to the melt rates are applied vertically at the immediate grid cell at the ice-ocean interface, making the response even more specific to its representation in MITgcm (compared to studies that use other definitions of the ice ocean boundary layer). Last but not least, the vertical resolution is very important in representing the ocean stratification, i.e. resolving the pycnocline between the AW and PSW layers in a Greenland fjord setting. Changing the vertical resolution and diffusivity will influence the distribution of heat and salt fluxes in the ice cavity in general as well as mixing between the plume and the ambient waters. Because all these several different aspects need to be considered and separating the different responses (sensitivities) is not trivial, a sensitivity study to the solution requires running a large number of simulations and analyses, so it deserves a separate study. We are currently planning an outline for such a study linked to another paper we are writing with colleagues from the Math Department developing a Finite Element Model for our setting, aiming towards a paper similar to [Gwyther et al., 2020] but regarding MITgcm and FEM in a Greenland fjord benchmark setting.

To sum up, the current paper is to focus on the dynamics of the response of ice shelves to oceanic thermal forcing, while the numerical aspects will be a topic of the next study mentioned above.

We will mention the planned sensitivity study in the *Future outlook* part of the Conclusions (line 402): "**There are several important aspects considering the model representation of these processes. One is the sensitivity to the model resolution and viscosity/diffusivity. Previous studies using MITgcm in similar applications and resolutions, Scascia et al (2013) and in particular, by Xu et al (2012), found that while the plume got better resolved and the average melt rates increased for higher resolution, the general circulation pattern and results about the dependency on oceanic forcing and SGD were consistent between the different simulations. On the other hand, the melt rate magnitude depends also on other factors e.g., the friction coefficient [Dansereau et al., 2013], which was used by [Cai et al., 2017] to tune the model to the observed melt rates, rather then the model resolution. In our simulation with sloping ice shelf, both vertical and horizontal resolution (and viscosity/diffusivity) needs to be taken into consideration in a dedicated sensitivity study, and not only the effects on basal melt but also on the representation of the stratification and the mixing between the two water masses, AW and PSW in the domain will influence the ocean heat transport to the ice-ocean interface.** Future work will **also** include the influence of sill bathymetry ..."

9. Line 130. *Could you clarify a bit more about how you got the subglacial hydrology input to work, please? Is this a standard practice in MITgcm? I was a bit surprised by the 50m grounding zone*

wall since this is about 15 grid cells. I thought maybe 5 would be enough... Is this similar to the methodology of Cai et al. 2017? I understand that adding sponge regions is going to make the numerics difficult. For reproducibility it might also be good to know what advection schemes you were using, i.e. types of flux limiter and general time stepping schemes.

Also I wasn't sure what the Burchard et al.,2022 reference was adding – was this an example of an enlarged grounding zone wall? Apologies if I missed that. With their paper I got the impression there were more interested in the general boundary characteristics whereas for this study, you are more interested in the dynamics themselves and how the plume starts is probably important for the resulting flow. I understand that there are lots of unknowns re the geometry and I think what you have done is fine. But I think maybe there could be a slightly more detailed explanation and tie in with the short section on sponge regions in lines 180 – 185, as well as changing the timestep in the tables. Presumably this is all part of the same problem of getting the model to run robustly? If so I think it will make it easier for people to work on this later if the details are in one section together.

- There are many points here. We structure the answer chronically:

Subglacial Discharge input:

Thank you for catching this - we actually had a paragraph about the SGD implementation in an earlier version of the manuscript but deleted it accidentally before the submission. We implement SGD using the RBCS package as in Sciascia et al (2013) after having discussed it with Roberta Scascia, and it is a standard method in MITgcm. As we do not have observations about the subglacial channel geometry at RG, we set the height of the subglacial channel to 20 m (same value was used by Scascia et al 2013 and Cai et al 2017). We will add these details at line 187-189 (sect. 2.2 where it fits better because it is there the sensitivity experiments to SGD are introduced)~~)~~:

"**In lieu of lacking information about the RG's subglacial channel geometry, we assume that the subglacial flux is dispensed evenly across the grounding line in a series of ice cavities 10 m (domain across-fjord width dy) in width × 20 m in height, analogous as in 2D setups of Sciascia et al (2013) and Cai et al (2017). The subglacial flux is implemented as a source term in tracer and momentum conservation equations using MITgcm source and relaxation package RBCS (`https://mitgcm.readthedocs.io/en/latest/phys_pkgs/rbcs.html`). The subglacial discharge velocity is calculated as ratio of the SGD volume flux to the area of the model cells where the subglacial discharge is applied. Note that the subglacial discharge velocity in MITgcm is applied in horizontal direction. The SGD fluxes for various experiments are presented in Table A2. These are rescaled from the $dy = 10$ m wide model domain to the estimated RG grounding line width of 10 km.**" Please also note that the Table A2 was updated with additional SGD experiments and and the values of total SGD fluxes (instead of percentages of the melt flux) as requested by reviewer 2.

For clarity, we will also move the sentence that is currently at line 135-136: "We set up a winter control simulation ($control_win$) without any subglacial discharge and a summer control simulation with subglacial discharge ($control_sum$ **, see sect.2.2**)." to the end of this section (current line 147, before sect. 2.1 "Basal melt parameterization"), so that it is clear that the details about SGD are presented later in the manuscript in sect. 2.2.

Advection Schemes and general time stepping:

He asks also for time stepping. Jonathan, please check if you are using 2.14 for momentum or 2.15 and if the variables are staggered in time, see "OR":

MITgcm uses an advective operator for momentum that is second order accurate in space, variables co-located in time and Adams-Bashforth time-stepping. In our high resolution simulation featuring stratification and strong tracer gradients, it is essential to use a flux limiting scheme (we have evaluated several advection schemes in the master thesis of Jin (2020) who used a similar SOF fjord configuration with observed T/S profiles finding that simulated flow is prone to overshoots and unrealistically low salinities when using non-conserving schemes). Here, we use the highly accurate third order direct space-time with flux limiting due to Sweby. It is explained in detail in the MITgcm documentation, including a comparison between the different tracer schemes (Section 2.17

in `https://mitgcm.readthedocs.io/en/latest/index.html`). We add this information after the end of the sentence in line 142:

"**The MITgm applies the semi-implicit pressure method for nonhydrostatic equations with a rigid-lid, variables co-located in time and with Adams-Bashforth time-stepping. The advective operator for momentum is second order accurate in space. We apply a third order direct space-time tracer advection scheme with flux limiter due to Sweby (`https://mitgcm.readthedocs.io/en/latest/index.html`, sect. 2.17).**"

Grounding zone wall and Burchard 2022:

We have communicated personally with H. Burchard at the EGU Assembly last year on that issue but the reference (Burchard et al., 2022) is notadequate indeed. We take away the reference here and instead explain in more detail the choice of geometry we make:

"~~The grounding line is set to 50 m above the ocean floor to avoid instability issues at the corner and leave a space for the plume to develop (Burchard et al., 2022).~~ **In the absence of detailed data about the ice and sea floor topography at the grounding line we chose to keep a vertical wall below the lowest point of the ice shelf of 50 m including a 20 m vertical subglacial discharge region (970 m - 950m; see sect. 2.2) to leave place for inflowing AW and to to avoid generation of strong property gradients at the corner of the domain.**

10. Line 136. *I think using a linear eos probably is fine, but I did want to bring this up for later since there is quite a large range of temperature values that you use (at least compared to Antarctic ice shelves which I am more familiar with).*

    - Thank you for the comment, this is a good point. For lower temperature range, the difference between linear and nonlinear EOS is insignificant. At higher TF, the effect of temperature on the buoyancy of the plume would increase for a fully non-linear EOS (because the dependency of density on temperature is quadratic), which would strengthen the qualitative result about a stagnation of the buoyancy with increasing TF in figure 4b. Consistently, Scascia et al (2013) used a nonlinear EOS and also found the linear dependence of melt on TF for the (higher) temperature range they applied. We will make a note on this in the discussion (line 376):

    "**Note that using a fully nonlinear EOS (with a quadratic temperature dependence) instead of the linear approximation (equation 1) is unlikely to change our results about the dependency of melt on TF. At the lower ocean temperature range, the difference between a linear and nonlinear EOS is insignificant. At the AW temperatures $> 0^o$ C, the effect of ambient ocean temperature on the plume buoyancy described above is expected to be further enhanced with a nonlinear EOS. A previous study of Sciascia et al (2013) for example, did use a nonlinear EOS and found a linear dependence of melt on TF for the AW temperatures they considered $(0 - 8^o$ C), consistent with our result for this range.**"

11. Line 139. *I think along with my point about grid resolution at line 125 – did you try varying the diffusivity and viscosity values? In particular it is the vertical diffusivity, that is going to control the vertical stratification / thickness of the plume) is. 2e-5m^2/s is probably fairly typical for an ocean model but I imagine this means you are relying on the advection scheme to add a bit of diffusion for stability. i.e if you change the grid resolution the total amount of mixing will go down because the spurious numerical mixing will be reduced.*

    - Again, thanks for the suggestion. Please refer to our answer to comment 8 about a sensitivity study to model resolution and diffusivity.

12. Line 141. *You say that because of turbulence you keep the diffusion and viscosity values the same (Prandtl number =1) but in fact you only have this for the horizontal viscosity and diffusivities. I think it makes sense to use similar/the same values from the previous experiments because these numbers are not well constrained and ultimately are going to depend on your grid resolution. One choice could have been to scale the vertical diffusivity/viscosity by the grid aspect ratio. One thing that occurred to me is that for the tidewater glaciers case (which is Sciascia et al. 2013 paper)*

mixing away from a vertical wall is going to be strongly affected by the horizontal viscosity/diffusivity (sciascia et al. 2013 – does actually explore this with some scaling analysis). Whereas in a shallowly sloping ice shelf the vertical diffusivity has much more of an effect on the temperature and salinity stratification. As above I think it would be interesting to know how sensitive your results are to vertical diffusivity.

- Thank you for the comment. We will clarify what the values of diffusivity and viscosity are in the text (line 139-141):

"Sub grid scale processes are parameterized using a Laplacian eddy diffusion of temperature, salinity, and momentum with constant coefficients as in the MITgcm fjord simulation of comparable resolution by [Sciascia et al., 2013]. ~~At the model resolution, the ocean mixing processes are dominated by turbulence, so we apply equal values of diffusion coefficients for all variables~~ **In the horizontal dimension we apply equal values of diffusion coefficients for temperature, salinity and momentum (horizontal Prandtl number of unity) while in the vertical the viscosity is higher than tracer diffusivity to ensure numerical stability (Table 1).**"

Yes, we agree that both, vertical and horizontal resolution, will play a role and need to be investigated concurrently in a sensitivity study to follow. Please refer to our answer to comment 8 about a sensitivity study to model resolution and diffusivity. And scaling the vertical diffusivity/viscosity by the grid aspect ratio is an interesting idea to explore in the future sensitivity study, thank you for the suggestion.

13. Equation 3. *I think I've normally seen the melt rate defined with a negative number so the melt 'wb' is a positive... It's also a bit odd to have the negative melt rate in Figure 1 but positive melt rate in Figure 3c but I can see how it fits in the plot better...*

    - For Equation 3, we adopt the formulation from [Losch, 2008], which is the reference underpinning the SHELFICE package of MITgcm we use. We will add here an explanation on how 'q' is defined (line 158):

    "Equations 3 and 4, that describe heat and salt balances at the interface, respectively, are used to calculate S_b **and** q**, where** q **is the upward freshwater flux (negative melt rate, in units of freshwater mass per time) and** L_i **is the latent heat of fusion. Upward heat flux implies basal melting (a downward freshwater flux), hence the minus sign (Losch, 2008).**

    While writing this comment, we noted that in this paragraph the variable T_i (ice bottom temperatures) is undefined. In fact, it should read T_b here to be consistent with equation 3 (we will correct this in line 160). We would also like to add Cai et al (2017) to the references in line 150 as they also used the SHELFICE package of Losch (2008) to simulate basal melt of the nearby Petermann Glacier.

    Regarding the Figures 1a-b vs 3c and 5c, we agree that it is a bit unfortunate to plot melt rate with different sign in the different figures. Reviewer 2 suggested to flip the y axis to show positive melt rates and keep the figure compact (Figure 1). We will adopt this suggestion (see a new version of the Figure 1 below).

14. Line 160. *I was a bit surprised of the form of the vertical ice shelf heat flux but looking at the MITgcm shelf ice docs maybe this is what you are doing. I think Holland and Jenkins 1999 (Section 2d) says this approximation for the ice shelf heat flux is uncommon because the ice shelf is so thick... They are other options in MITgcm so it might be worth double checking this is what you are doing.*

    - For this study, we decided to stick to the vertical ice shelf heat flux formulation and implementation that was used e.g., in [Cai et al., 2017] for Petermann Glacier, among other studies. We do think that it is a relevant question and we are currently working with colleagues from Grenoble on a study investigating the effect of different formulations of the vertical heat flux into the ice in MITgcm and other models. We will add the reference to Cai et al here (line 158-160):

    "**As in Cai et al (2017)**, we assume a linear temperature profile in the ice and approximating the vertical temperature gradient in the ice as the difference between the ice surface (...) divided by the local ice thickness."

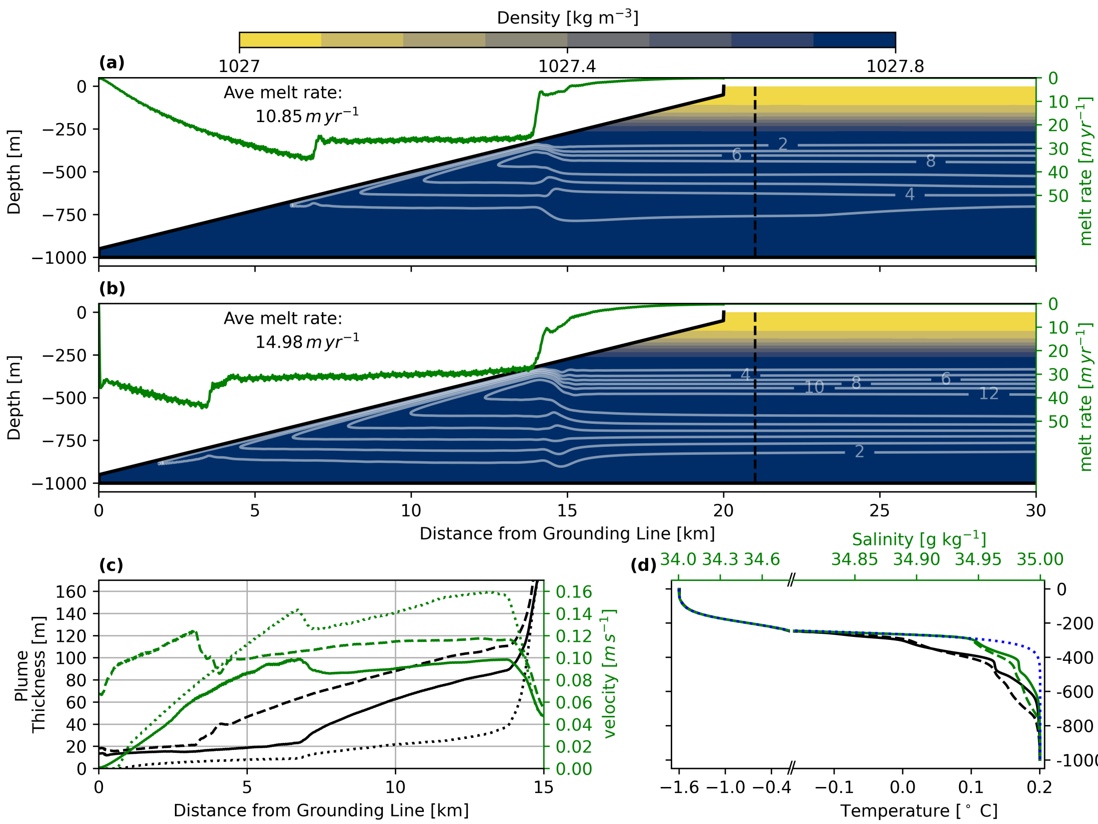

Fig. 1: a) The stream function (white contours in m^2 s^{-1}) of the steady circulation superimposed on the density (colors) and the melt rate (green line, right axis**, flipped**) along the ice ocean interface (black line) for control_win. The black dashed line indicates the location of profiles shown in figure 2 and 6; b) same as in a) but for control_sum; c) The plume thickness (black) calculated for summer (dashed) and winter (solid) control simulation; and the vertically averaged plume velocity (green) for summer (dashed) and winter (solid) control simulations. **Dotted lines show plume thickness (black) and averaged velocity (green) for an alternative plume definition based on the stratification below the ice in the control_win simulation (see text).** d) Initial and open ocean boundary condition profiles of salinity and temperature (showing as one blue dotted line for the chosen axes limits) and the steady state temperature (black) and salinity (green) profiles of the summer (dashed) and winter (solid) control simulations at x = 21 km. **Note the split in the x-axes marking two different scales.**

15. Line 176. *Just to reiterate again, I think at the end of this section is where you could add a sentence explaining that the melt parametrisation is very dependent on vertical grid resolution (and grid type) e.g see Gwyther et al. 2020.*

    - As we explained in comment 8, we do not think that Gwyther et al. 2020 is an appropriate reference in the context of our MITgcm study because their results consider different model architectures and different technical solutions for the implementation of the melt parameterization. Please see our answer to question 8 for more details.

16. Line 182. *Is Sb the salinity at the depth defined by the initial/restoring conditions? I think it is, but I think your sentence implies that it is the salinity 'after' melting which might not be the same.*

    - Thanks for pointing it out. The text was in fact unclear here. This is a very important note and would otherwise have lead to major misunderstanding. There was a confusion with the naming of the variables so it is not the *Sb* defined in eqs. 2-4 that is used to calculate TF. We will correct the names of the variables used in this paragraph and elaborate more on what they stand for (lines 180-182):

    "**To quantify the response of the system in terms of melt rate and circulation changes to changing oceanic thermal forcing (by varying T_{AW}), w**e define a**n average** temperature forcing (TF= $\mathrm{T}_{GL}(\mathrm{x}_{GL},\mathrm{z}_{GL})$ - $\mathbf{T}_f(x_{GL}, z_{GL})$) **for each experiment, based on the time averaged fields when the model is in a statistical steady state (model days 61-100).** ~~where~~ T_{GL} is the **time averaged** water temperature at the grounding line (x_{GL}, z_{GL}) and $\mathbf{T}_f$ is the freezing point temperature evaluated at the same point using the local water salinity $\mathrm{S}(x_{GL}, z_{GL})$~~and quantify the response of the system in terms of the melt rate and circulation changes to changing TF~~. **Note that the water at the grounding line is a slightly modified AW so $\mathbf{T}_{GL}$ is close to T_{AW}. Furthermore, $\mathbf{T}_f$ at the grounding line is essentially constant throughout all experiments at $\mathbf{T}_f$ = -2.68°C, hence we can approximate TF=$\mathbf{T}_{GL}$+2.68°C (See tables 2, A1 and A2).** We apply a wide range of AW temperatures to quantify the response of the melt rate and the resulting circulation to varying TF with more confidence."

    See also the response to Reviewer 2, comment 5.2.

17. *Line 189a. Just to reiterate I think maybe the details from Line 130 should be here/ repeated. I think expect having zero salinity is something you have to be careful with numerically because if you get negative salinity from spurious numerical mixing then the density relationships start to go wrong. I think that is why it would be useful to at least note the types of timesteppers / advection schemes used because even though it is not the focus of the paper it will help other researchers to work on similar problems later.*

    - Yes, this is a good point. We will add the detail about using a conservative tracer advection scheme here, after adding the details about the SGD implementation you requested above (line 189):

    "We use a conservative third order direct space-time tracer advection scheme with flux limiter (sect. 2) to avoid tracer extremes and the possibility of salinity going negative during the numerical integration when implementing SGD."

18. *Another thing did you try with an open boundary at the grounding zone? I noticed in Figure 6 that you get stronger return flow at depth as the subglacial flux increases. I wonder if this would have the same pattern if the flow at the grounding zone could be balanced by the boundary condition and not recirculating flow within the cavity...*

    - This is an interesting suggestion. As explained above, we implement the subglacial discharge as a source term in tracer and momentum equations using MITgcm source and relaxation package RBCS, which is a standard method to do it. It is unclear to us which Open Boundary formulation exactly would need to be used to implement the SGD. Note also that the strength of the circulation is a few orders of magnitudes larger than the subglacial discharge flux. Therefore we do not expect it would make any discernible difference to the modelled circulation in the cavity.

19. Line 192. *I think this implies you changed the timestep in order to see how the melt is sensitive to timestep whereas it seems more that you were changing other parameters at the same time.*

    - The time step was changed to keep the model stable. We will reformulate here to make that clear:

    "All simulations were run for 100 days with a time step of 2-10 s **depending on the strength of the TF and/or SGD forcing to achieve model stability** ~~for the control runs and varying time steps for sensitivity experiments~~ (Table 2)."

20. Line 224. *I understand that you need a way of analysing the plume so taking u>0 seems like a reasonable choice. I am not too sure though how the resulting thickness compares to other more established plume model results. My impression was that order 150m e.g Fig 3a, is quite large. Would you be able to find some references please to justify the magnitudes?*

    - Thank you very much for this very important comment. First, we would like to comment regarding results we presented in the manuscript using the plume definition based on the velocity. The only published study, to our knowledge, to explicitly look at buoyant plume diagnostics for a sloping ice shelf-ocean interaction in an ocean circulation model is the study by [Holland et al., 2008]. They found averaged plume thicknesses of 40 m and the maximum plume thickness (estimated from temperature section shown in their fig 4b) reached about 200 m. The plume averaged plume thickness in our winter control simulations is around 40 m (around 20 m in the first 7 km, and then thickening henceforth) across all experiments except the coldest (where it is much smaller). For experiments with SGD the average depth varies from 62 m (for *control_sum*) to 80 m for *SGD100* experiments. Given the higher melt rates and velocities in our study compared to [Holland et al., 2008], our boundary layer thicknesses based on the velocity diagnostic are thus consistent with those of [Holland et al., 2008].

    However, your comment made us realize that there might easily emerge a confusion regarding the definition of a "plume" when comparing results from an ocean circulation model to results from the one-dimensional plume model of Jenkins (2011) in an attempt to find one-to-one correspondence between the two. Ocean circulation models exhibit much higher complexity than the 1D plume model (just to mention the nonlinear and viscous terms for example), so the diagnosed plume thickness will depend on the plume definition. Our plume, defined based on horizontal velocity, is thus not necessarily equivalent to the well-mixed plume concept from Jenkins (2011).

    To show this, in Figure 2 we compare the distribution of buoyancy and horizontal velocity in the region that of positive horizontal velocity. Note that the plume as defined from horizontal velocity is not a uniform body of water but does in fact show the largest gradient in buoyancy closer to the ice (Figure 2a). We impose an additional criterion to define the plume based on the buoyancy in the spirit of [Arneborg et al., 2007] estimated from the buoyancy, by using the 75th percentile of buoyancy within the region of positive horizontal velocity. This added condition yields a significantly narrower plume (white thick line in figure 2) than the velocity criterion (color-shaded area). We also plot the plume thickness estimated using the buoyancy criterion for the winter control experiment in the new version of Figure 1c; compare the black solid and dotted lines. This difference arises due to the vertical Prandtl number being larger than one (vertical viscosity higher than diffusivity, see Table 1), which leads to a wider region of positive horizontal velocity than of uniform buoyancy (Figure 2b). As mentioned in comment 8 above, the higher vertical viscosity is needed however to keep the model stable. Moreover, the processes that are resolved in our MITgcm model but absent in the 1D plume model, like internal waves, cause high variability (high variance that we diagnose, not shown) at the plume "interface" and make it "broader" in the velocity-criterion sense.

    It is important to note that the two regime structure of the plume velocity distribution is insensitive to the stricter definition of the plume based on velocity and criterion (see figure 2a-b and compare green solid and dotted lines in the new version of figure 1c), although the plume velocities from the buoyancy criterion are slightly larger. This holds for all experiments in this study.

    We suggest to add Figure 2 to the Appendix and to use the new version of Figure 1c (showing plume thicknesses and velocities from the original velocity criterion and the new combined velocity and buoyancy criterion for comparison) in the manuscript. We will append the text in Section 3.1,

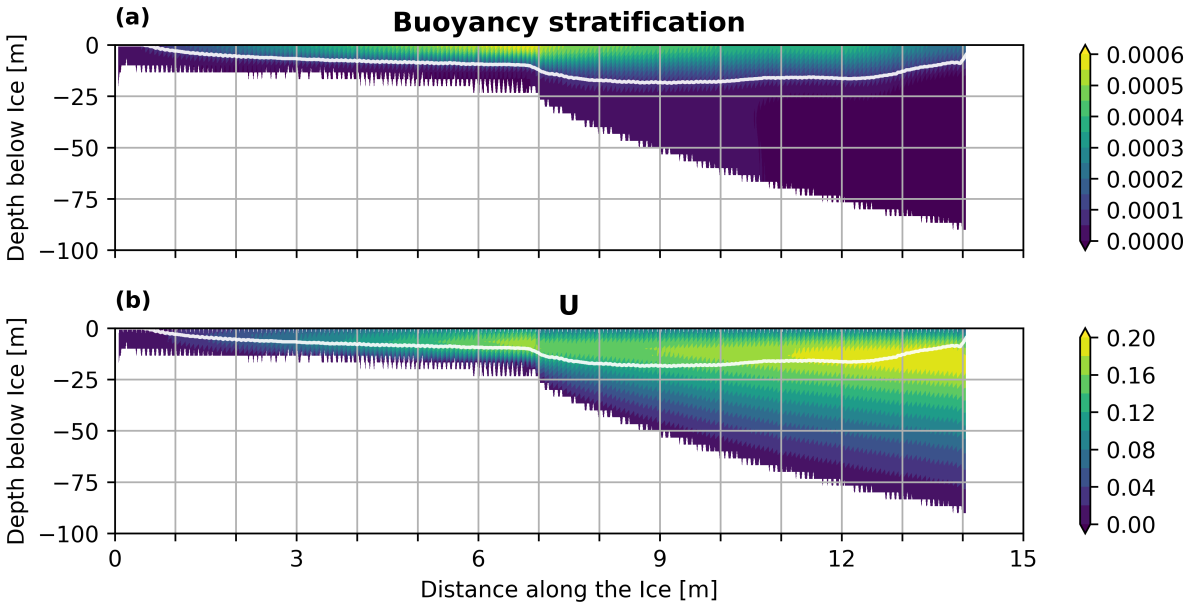

Fig. 2: Section of buoyancy (a) and along ice velocity (b) within the plume region, as defined by the horizontal velocity criterion (u>0). The white lines indicate the 75th percentile buoyancy isoline.

line 224, with the results where relevant and we will add the reference to [Holland et al., 2008] in the discussion (in line 337):

Line 224: "We tried alternative definitions of the plume based on the temperature and salinity difference compared to the ambient and prescribed stratification. These resulted in a **narrower or** wider plume over the distance between 7 and 14 km **depending on the value of temperature and salinity used. For values closer to these of ambient stratification, the resulting plume was wider.** As the difference encompasses the region of no horizontal flow outside the plume (by definition u $\leq$ 0 here), this has no impact on the further calculations of e.g. plume transport. **Using a buoyancy criterion, i.e. temperature and salinity combined, and defining a threshold (75th percentile) results in a narrower and relatively well mixed plume, i.e. in characteristics more comparable to the plume of [Jenkins, 2011]. To quantify this, we show** the plume thickness and averaged plume velocity ($u_p = \sqrt{u^2 + w^2}$) in figure 1c. Clearly distinguishable are two different plume regimes during its ascent along the ice base**, no matter the way of defining the plume**: the accelerating plume and the thickening plume. **When using the velocity criterion, i**n the accelerating plume regime ~~close to the GL,~~ the plume has a thickness of around 20 m, while the **vertically** averaged plume velocity increases steadily to a maximum of 0.1 m s^{-1} at 7 km. In the thickening regime the velocity is around 0.095 m s^{1} and the plume thickness increases from 20 m to 90 m between 7 km and 14 km. **The average plume thickness is around 40m for all experiments. Note however that the plume defined by velocity only is still stratified , so it is not fully equivalent to the "well mixed plume" in the sense of [Jenkins, 2011]'s plume model. If we define the plume by adding a buoyancy criterion (only the 75th) percentile of buoyancy values in the velocity plume), the plume is narrower with a higher average velocity, compared to the original definition based on velocity only (Figure 1c). Notably, the plume accelerates strongly in the first regime to a local maximum average velocity of 0.14 m s^{-1} and shows a significant decrease of velocity at the regime transition but subsequently starts again to accelerate in the second regime. The overall higher velocities using the buoyancy criterion in the plume definition arise because the region of low velocities further away from the ice is not considered**. ~~This two-regime structure is evident in other plume properties (e.g., temperature, salinity and density; not shown) and is corresponding to the spatial variability~~

~~in the melt rates described above.~~ The depth of the transition from accelerating to thickening plume is linked to the ambient stratification in the fjord (Figure 1d, see Section 3.2 and 3.3)."

Adding to the discussion in line 337: "Our high resolution model simulation allowed to resolve a spatial pattern of the basal melt and the melt driven circulation under the ice tongue. In the winter control simulation, the basal melt rates and the plume exhibit a two-regime structure along the ice base (high melt rates in the accelerating plume regime up to 7 km and the lower melt rates in thickening plume regime thereafter up to 14 km). **This two regime structure is insensitive to the way of defining the plume (by buoyancy or velocity). We have diagnosed various plume diagnostics using a velocity criterion, which led to e.g. average plume thicknesses of around 40 m, comparable with what was found in [Holland et al., 2008]. Care has to be taken, when comparing these diagnostics to the one dimensional plume model ([Jenkins, 2011]) , where uniform plume properties are assumed. This is not necessarily true in the plume defined using the horizontal velocity. Adding a buoyancy criterion yields a narrower and faster plume (see Section 3.1), with almost uniform distribution of buoyancy. The uneven vertical spreading of momentum and tracers (i.e. temperature and salinity) can be attributed to a vertical Prandtl number larger than unity, which leads to a stronger downward diffusion of momentum away from the ice, increasing the region of positive horizontal velocities beyond the region of uniform buoyancy. The increased viscosity is needed in order to obtain stable simulations; increased tracer diffusivity would lead to a smearing out of the thermocline.**

To our knowledge, small-scale variations in the melt rate have been barely captured by observations (Wilson et al., 2017)..."

21. Line 230. *The two regimes that you find is quite striking and I think the plots (e.g Figure 2 and 3) do a good job of explaining them. I am not that familiar with plume models or detachment of the plume from the ice-ocean boundary. Did you expect to see this patterns before? If so probably there should be some more references. But they are nice results.*

    - Thank you for this comment, we are happy that you find these results remarkable and interesting. Regarding the two regimes: We are not aware of any study addressing similar patterns in plume and melt rate properties. But after scrutinizing the references, we found that in [Holland et al., 2008], figure 4b, a similar two-regime structure seems to be present, but it was not specifically discussed there. Their study was also initialized with a two-layer ocean stratification, but several other details of their model setup are very different so we do not attempt a detailed comparison. When it comes to the detachment of the plume from the ice-ocean boundary: it is a common feature in stratified Greenland fjords that the buoyant plume detaches at the level of neutral buoyancy ([Straneo and Cenedese, 2015]). We did mention the plume detachment and cite this reference in the beginning of section 3.1 of the submitted manuscript (lines 203-205).

22. Line 239-241. *Would you be able to plot your results of temp or sal depth profile against the cruise results? If this is Figure 1d maybe it could be bigger? If that comparison is difficult maybe pointing the reader at the relevant figure in Jakobsson et al., 2020 would help.*

    - A direct comparison between the observed profiles during *Ryder 2019 Expedition* and the model profile is difficult as we idealized the observed profiles by a hyperbolic tangent (tanh) function. A pointer to the exact figure (Figure 2) in [Jakobsson et al., 2020], where the observed profiles are shown, will be added to the reference in line 241. Furthermore, the second reviewer suggested a reformulation of the text in this paragraph, see their comment 9. We will reformulate the paragraph with the added reference to the figure so:

    "The outflow forms a T-S transition layer between the AW and the PW, that was smoothed out in the idealized initial profiles (Figure 1d and 2b). **This layer is characterized by a cooling and freshening compared to the initial profile, in line with what would be expected from glacially modified water. This glacially modified layer can also be found** ~~This transition is recognizable~~ in the observations of (Jakobsson et al., 2020, **Figure 2**), lending confidence to the model results."

23. Line 257. *This is interesting! I am a bit more used to cold Antarctic ice shelves so the first question is: can AW temperature reach ∼6degC? Although you talk about their study later (and I have some more questions later), I think it is interesting that for the Holland et al 2008b study for their low temperature cases the plume detaches. But at higher temperatures (i.e more melting, fresher plume) the plume is able to overcome the stratification and reach the top. Presumably at the lowest temperatures your plume still detaches from the ice base because the PW outside the cavity is so fresh/less dense?*

    - Thank you for the question. The reported AW inflow temperatures in ship-based hydrographic profiles close to the Greenland marine terminating glaciers vary between around 0^o C in the north to 4.5^o C in the south, [Straneo et al., 2012]. However, temperatures in individual seal dives from East Greenland Shelf do reach temperatures of $8^o - 9^o$ C at the AW-depth (300-350 m), see Sutherland et al (2013; `https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012JC008354`), so the range we used is not completely unrealistic. The main point with the extended temperature range was however to generate enough data points to have higher statistical confidence in the dependency of the melt rate produced by the melt parameterization we applied on the oceanic thermal forcing, similar was done by e.g. Sciascia et al (2013). We do mention why we use an extended temperature range already in sect. 2.2 (when introducing the sensitivity study), line 183, and we do mention it in the Discussion in line 365. But we will clarify it further at this point in the Discussion, line 365:

    "Here, we applied a wide range of oceanic thermal forcing (**with T_{AW} up to over 6^o C, i.e., higher than typically observed at the Greenland's marine terminating glaciers, see e.g., Straneo & Cenedese 2015)** and a resampling technique to quantify the response of the melt rate and the resulting model circulation to varying TF ~~in more detail~~ **with a higher statistical confidence**."

    In all our simulations the plume detaches from the ice in the base of the pycnocline, where it reaches its neutral buoyancy, to form a laminar outflow at around 400 m depth (see figure 2 in the manuscript). This is a consequence of the 2-layer stratification in the fjord (PW overlaying AW).

24. Line 269. *I think Figure 4a is nice and clear – there does seem to be a linear trend as TF increases and evidently that trend doesn't continues as the temperature difference gets closer to zero. Having said that, sorry I couldn't understand why you have a cut-off value which is a range between 2.88 and 3.18 degC. Could you explain your reasoning more please?*

    - The cut-off temperature is determined by re-calculating the linear regression for a sub-set of the data pairs (TF and Average Melt) to find the best fit. The linear regression fits best (lowest mean squared error and highest coefficient of determination, R^2) when calculated for experiments with a temperature forcing higher or equal to 3.18°C. Therefore we assume, that the cut-off temperature for the linear vs non-linear dependency is somewhere between 2.88°C (the highest TF to exclude from the linear fit) and 3.18°C (the lowest TF to include). To make it more clear, we will add a figure with the values of R^2 and the mean error in the Appendix (Figure 3a). A reformulation of the text based on suggestions from reviewer 2 is given in the response to their comment 12.

25. Line 270. *Did you do a curve fit to find what the non-linear relationship is over the lower temperature values? I think this might make the paper stronger so you can directly compare with the work done on Greenland tidewater glaciers and ice shelves.*

    - Thank you for the comment. We did not, because it would require us to run several additional computationally-heavy model experiments to get a confidence on the fit constraint to the first decimal and we do not expect much new knowledge we would gain through this. There is only one similar study considering the ice-shelves to compare the fit with ([Cai et al., 2017]) and there are many differences in the model setup and experiments to make such comparison meaningful.

26. Line 278. *I think this is very interesting and does seem like it is the crux of the paper. I am not sure I completely understand it though. If it is obvious then just ignore the following ramble!*

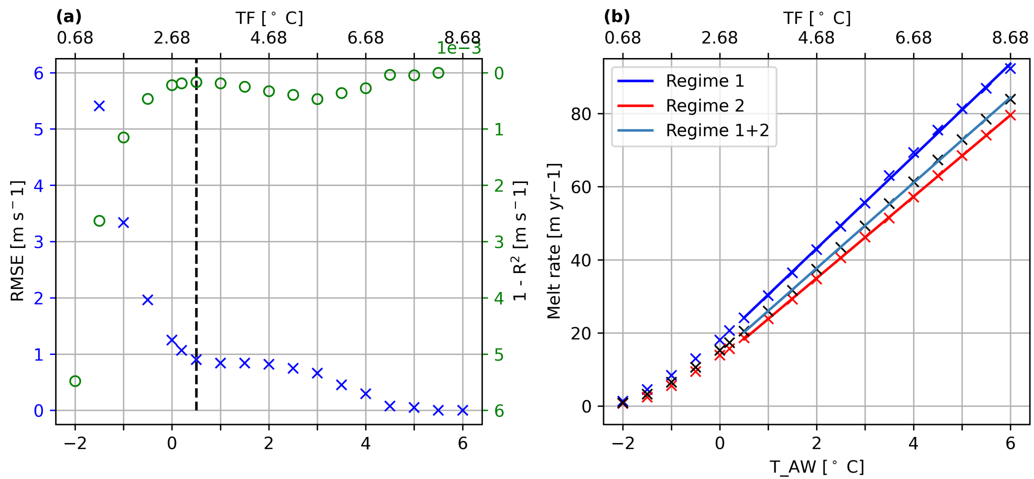

Fig. 3: Measures of goodness (root mean squared error (left y-axis, blue) and R^2, shown as 1 - R^2 (right y-axis, green)) of the linear fit for the melt dependency on temperature Forcing. Dashed black line shows the experiment with the lowest AW temperature (i.e. temperature forcing) included for the best fit.

- Thank you for the comment, if it was so obvious, there would be no need for a ramble so I am sure that we need at least some re-formulation there. I will address each paragraph separately to keep the structure.

I can see that intuitively from Figure 4b that as you are increasing the temperature difference cooling starts to have as big an effect and balances the freshening due to melting. But it is not obvious to me that this should be the case. It's also a bit curious (and I only saw it after staring at it for a while) that it looks like the integrated plume buoyancy in Figure 4b might be starting to turn down at TF 8.68degC. Implying the cold water is having more of an effect. Did you run another simulation to check this?

- We explain this effect (alongside with the effect of freshening due to increased melting) later on in the manuscript in the Discussion section in line 370. We will reformulate to make it clearer:

"We went further in trying to elucidate this regime shift in the melt rate response to oceanic thermal forcing by examining the buoyancy forcing of the melt driven plume. For cold ambient temperatures the plume buoyancy is dominated by the salinity difference between the plume and the ambient water ~~, and this salinity difference increases slowly with TF due to the increased melt water flux to the fjord. The increasing ambient water temperature however, leads to increasing temperature difference between the plume and the ambient water, leading to a negative effect on the plumes buoyancy. For sufficiently warm ambient temperatures (i.e., high TF), the negative effect due to increasing ambient water temperature difference on the buoyancy overrides the positive effect of freshening from increased input of melt water (Figure b)~~. **For increasing temperature forcing, i.e. increasing ambient temperature, the following mechanisms are in place. First, the melt rate increases leading to higher input of fresh and cold melt water. Second, the cooling due to mixing of the ambient AW becomes more efficient because of the larger temperature gradient between the (warmer) ambient water and melt water. Because the cooling close to the ice boundary increases stronger than the freshening with increasing TF. In figure 4b this manifests in the slopes of "Buo-S" and "Buo-T" becoming approximately the same for higher TF, leading to a flattening of the slope of "Buo". Since salinity and temperature effect are of opposite sign, the net change in buoyancy in the plume with increasing TF diminishes.** As a consequence, the plume velocities do not increase further with TF (figure 4), resulting in effectively constant exchange coefficients in (Eq. 3) and a linear dependence of melt rates for higher TF. **An additional factor could be the dependence of T_b (Eqn. 2) and therefore the heat balance (Eqn. 3) on**

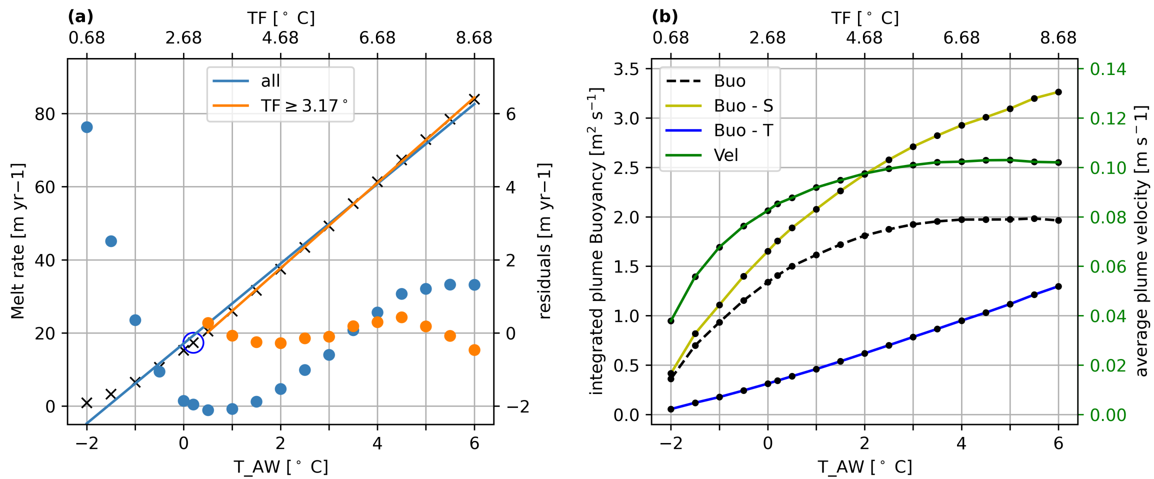

Fig. 4: as figure 4 in the manuscript. b) Green line and right y-axis shows the averaged plume velocity.

S_b. An increased melt rate due to higher TF will decrease salinity at the interface, thereby increasing T_b and decreasing the local temperature difference (T_w - T_b) along the ice. This could potentially be negative feedback on the melt rate contributing to the observed change in dependency of the melt rate on TF from quadratic to linear at higher TF. These results are generic and relevant for future development of the basal melt parameterizations for marine terminating glaciers in the climate ice sheet models."

Second, we did not run more experiments because the warmest AW temperature is already untypically high for AW and our main focus was on exploring a non-linear vs. linear relationship between the melt rate and thermal forcing. To statistically constraint the bending down of the buoyancy curve, we would need more than just one additional experiment, and extending the range further would make the experiments even less typical and we don't see this as relevant for gaining additional information for developing the melt parametrization.

Probably the first thing to say is back to my earlier point about the linear EOS, are you convinced that it is valid over these temperature ranges? Secondly, have you thought about trying to plot this using Gade lines? I wonder if you can relate the initial change in salinity directly as a function of temperature based on the gade lines. When the temp forcing is cold evidently salinity controls the density difference due to the coefficients in the eos. You might be able to plot these initial slopes on Figure 4 b to guide the eye.

- Regarding the EOS please refer to our answer to comment 10.

Regarding Gade slopes: To a first approximation, the Gade Slope is the same for all experiments. The difference between the experiments is the slope of the ambient T-S stratification (Figure 5. Since S_{AW} stays the same for experiments, the slope in T-S space becomes steeper for higher T_{AW}. The change of properties at the melt interface will however still follow (approximately) the same T-S slope. Therefore the change in temperature and salinity difference between plume and ambient water can not be equal. This is just a different way of explaining the same mechanism we describe in the answer to the first paragraph and in the reformulated text. We have plotted T-S diagrams with Gade lines but were unsure on how to plot information relevant to interpret the integrated information from figure 4b.

Looking at the Holland et al. 2008b paper I think as part of their simple scaling analysis of the melt rate they show that the thickness of the mixed layer isn't changing. This implies that the entrainment is only a function of the velocity increase (caused by the change in buoyancy due to melting) and from there they back out the direct temperature effect on melting and the velocity effect. I wonder if you could make some plots like Figure 5 in Holland et al. 2008b because then you would be able to make / refute similar arguments that they did in section 4.

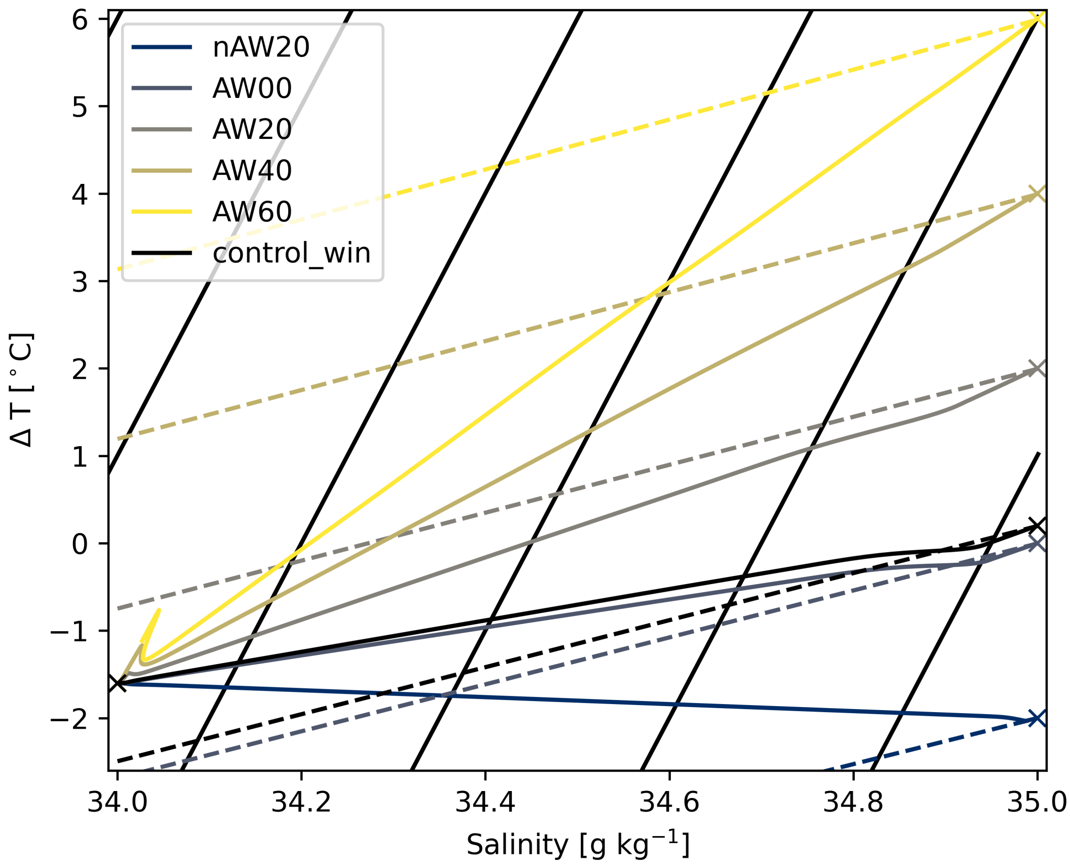

Fig. 5: T-S Diagram for selected oceanic thermal forcing experiments. Aw and PW properties are marked by crosses (black only for PW and color coded for AW). The dashed lines are the Gade line and the solid lines shows the ambient profile at x=21 km. Black solid lines mark lines of constant density.

- We added a secondary y-axis to figure 4b and plotted plume averaged velocities (see figure 4 in this response). The curve follows directly the buoyancy and hardly increases for stronger TF. Therefore the dependency of melt on the heat exchange coefficient, which is a linear function of the velocity, and the temperature forcing becomes linear, as the heat exchange coefficient does not increase with increasing linear forcing. This is the argumentation we use in the discussion (line 375) to refute similar arguments from [Holland et al., 2008].

I am little bit concerned about why your plume thicknesses are so large. For instance the holland et al. 2008 mixed layer is 'only' 40m but seems fairly constant over the temperature ranges they examined (which are admittedly lower than what you have here). This might be a red-herring, but if the plume thicknesses were caused by too much spurious mixing then maybe you have a situation where the balance of melting, diffusion and temperature forcing isn't quite right. I think the results are still interesting and your results seem robust as you change the temperature forcing. That's why I made the comments earlier about the grid resolution and trade off between spurious and explicitly specified turbulent mixing. It might be worth bearing this in mind. Probably the results you have won't change but maybe the magnitudes would be different, and I think it might be worth saying that in the text. Writing this I've realised that because you have a maximum velocity/melt peak the plume has to get thicker because of convergence of the flow. I still think the approach in Holland et al 2008.b might help to work out the mechanism.

- We would like to refer back to our answer to comment 20, showing that our averaged plume thicknesses are consistent with those in [Holland et al., 2008].

I think Jenkins 2011 might also help explain what is happening. I think in that case the buoyancy of the plume is controlled by the initial flux of subglacial discharge at the grounding zone. This is the convection driven example. Jenkins showed that in that case you actually do get linear dependence of melt rate on temperature forcing, because the plume is so fresh that adding more melt doesn't really change the density difference of the plume so the speeds are not controlled by the temperature and as a result the melt is only linearly related to temperature. It seems like you have a similar situation here although coming from a different effect, in that increasing the temperature doesn't seem to influence the buoyancy of the plume. As you point in Figure 3d and Figure 2b the density of the ambient stratification is obviously determining when the transition occurs. Maybe you could plot the integrated melt rates before 7km (in the accelerating phase) and maybe you would recover the nonlinear behaviour.

- Our thermal forcing experiments are all conducted without subglacial discharge. However, we also tried to explain our situation along the lines of [Jenkins, 2011] but as you point out correctly the effect has a different basis. In our case it is rather the change in the ambient density, that is preventing the buoyancy from increasing further for warmer ambient water. As we can see in figure 4b the properties of the plume keep changing with changing TF. We also tried to identify a non-linear increase by looking at the two regimes separately but the melt dependencies do not look significantly different (See figure 3b). For a reworded explanation about the two regimes, please refer back to our response to the beginning of this comment.

Also what does the melt rate look like for nAW20. The scale you are using really doesn't show it at all... Perhaps this could be in an appendix if it helps tell the story of the two regimes. Apologies for that long ramble but it is not clear to me (evidently!) why the integrated plume buoyancy plateaus.

We agree that the structure of the plume in the nAW20 experiment is not visible in the melt rate plot (Figure 3c in the manuscript). However the velocity and thickness distribution (Figure 2a-b in the manuscript) show the structure exists as well although the first regime extends to about 10 km (instead of 7 km). This actually adds to the argument about the ambient stratification determining the regime transition, because figure 2b shows a much shallower lower bound of the pycnocline. We will correct the statement in line 256:

"For the coldest experiments, i.e., weak oceanic thermal forcing, the melt rate is lower and the plume **shows the shift to the secondary regime only around 10 km (Figure 3a,b) and a depth of around 500 m (Figure 2b)** ~~does not develop the two-regime structure we see in warmer experiments~~."

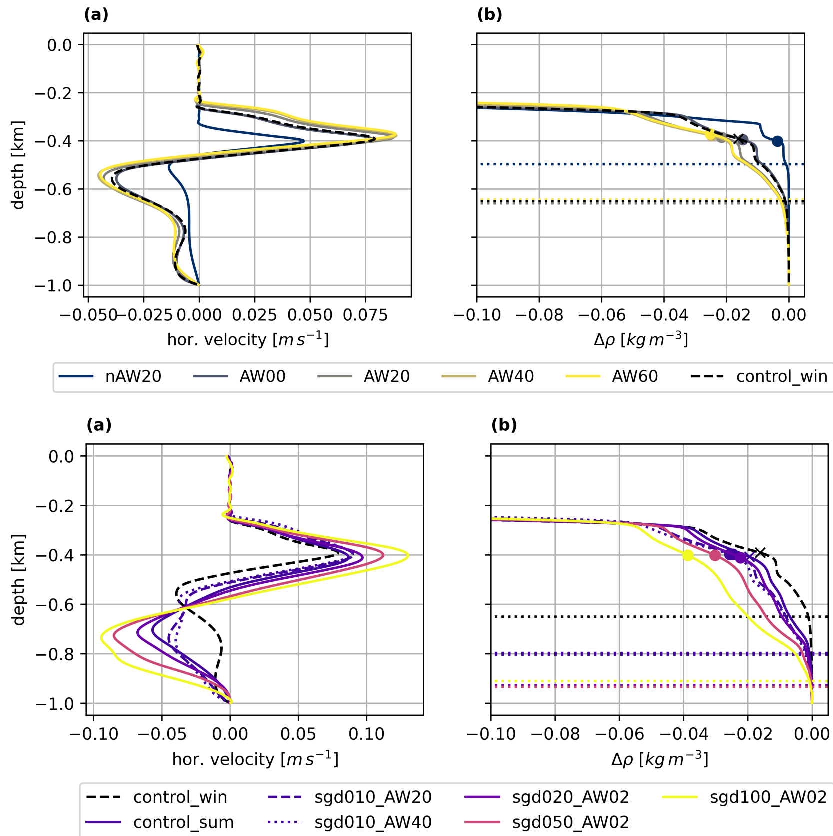

Fig. 6: Figure 2 and 6 from the manuscript

This leads to an inconsistency in defining the depth of the transition. For now we used the maximum melt rate, this is not consistent when including nAW20. Therefore we propose to change to the criterion to be the maximum decrease in vertically averaged plume velocity (Figure 3b and 5b in the manuscript). We will adapt the text and figure 2b and 6b accordingly (see figure 6.

27. Line 330-334. *I am not suggesting it is a good idea but you could suggest tuning the melt rates using the turbulent exchange coefficients / drag coefficient in the melt parameterisation as I think Cai et al 2017 did this,*

    - Existing estimates of melt rates for Ryder Glacier are few (to our knowledge only [Wilson et al., 2017]) and are derived from satellite data for a limited amount of data points for a limited period of time (2011-2015). The oceanographic data is even more scarce and the profiles we use here are from summer 2019, so we are not in a position to attempt a rigorously constraint simulation. Furthermore, our two dimensional simulations, not taking into account across fjord variability in the ice geometry, are very idealized. The purpose of this study is therefore not to reproduce the exact melt rates at RG but rather investigate the relationship between basal melt and oceanic forcing. Our main results (about melt dependency on TF and the two plume regimes) are not dependent on the magnitude of the melt, so tuning the melt slightly will not change those results.

28. Line 335. *Apologies if I missed it but I couldn't see in the text where there is a figure to back this up. Is this supposed to be a combination of 1d and 2b?*

    - Indeed the change in the T and S profile (solid and dashed, green and black) can be seen in Figure 1d and 2b - we will add a cross-reference to the figure here.

29. Line 360. *I think you should probably add that Jenkins 2010 found linear response to melt rates when subglacial discharge is dominating buoyancy of plume.*

- I assume you mean [Jenkins, 2011] and refer to the convection driven plume you mentioned above. That is in fact a good addition here.

30. Line 362. *I think you should say that these references were vertical cliff tidewater glacier studies.*

    - We agree with this suggestion:

    "On the other hand, several modelling studies of **vertical** tidewater glaciers around Greenland, where ocean temperatures are higher due to the AW inflow, have reported on a linear dependency of melt rates on TF (Xu et al., 2012; Sciascia et al., 2013, 2014)."

31. Line 365. *Just to emphasise again I think the paper conclusions / impact would be stronger if you had calculated a curve fit for a subset of the low TF simulations to compare with the literature.*

    - Thank you for the comment. Please refer back to the answer to comment 25.

32. Line 367. *I think 'unifying results from the previous studies' is a little bit strong maybe. Based on what you argued in the introduction that the tidewater glaciers with vertical ice fronts is different to ice shelf cavities I am not sure that you have linked ice shelves to tide water cavities. I think the main contribution which I think is still very interesting and relevant is that for high temperature differences the dynamics are quite different to Holland et al 2008b. Maybe there is a link to the Jenkins 2011 results, but I am not sure I understand it yet. They might be some caveats that this applies on small cavities where Coriolis doesn't play a role, but I think it is still very interesting. I think the same comment applies in the abstract.*

    - We can reformulate the phrase "unifying results from the previous studies" to "linking up and contextualizing results from the previous studies".

33. Line 377. *I do think the results are interesting and generic.* - Thanks, that is nice to hear.

34. Line 397. *I think you should cite Jenkins 2011 since that is what Cai et al 2017 and Slater et al 2016 are building on. Similarly to the nonlinear low TF, I think adding in a curve fit for the exponent would improve the paper and again make it more relevant to the existing literature.*

    - Thank you for the comment. We have extended the SGD range for warmer experiments and calculate curve fits and compare to Jenkins 2011 (see figure 7). We will add a comparison of exponents here as well. Please also refer to our response to reviewer 2, comment 16, for a formulation suggestion to be inserted instead of the paragraph starting in line 304. We will add this also to line 397:

    "**For experiments with constant TF, the melt rates increase less than linear (in a fractional manner) with the SGD, consistent with the modelling experiments of [Cai et al., 2017] for Petermann Glacier and the theoretical scaling of [Jenkins, 2011] and [Slater et al., 2016]. Our values for the exponent vary between 0.4 and 0.5 for the different experiments; they are slightly higher than what is estimated from theory (1/3) and close to those found by [Cai et al., 2017] and [Sciascia et al., 2013]**"

35. Line 400. *Maybe you could a caveat about turbulence in the conclusion. Presumably the details of the mixing / growth of the plume thickness will affect the melt rates quantitatively if not qualitatively. I think it might also be worth adding at some point in the discussion section about the dependence of the melt rate parameterisation on grid resolution.*

    - Please refer to our answer to comment 8 where we also suggest a paragraph text for the Discussion section.

3 Technical corrections

36. Line 22. *'and complex geometries of both, ice and ocean, domains,' I am not sure you need the commas around 'ice and ocean'* - Thank you, we will correct it.

37. Line 51. *'in the southern Greenland' -¿ 'in southern Greenland'* - Thank you, we will correct it.

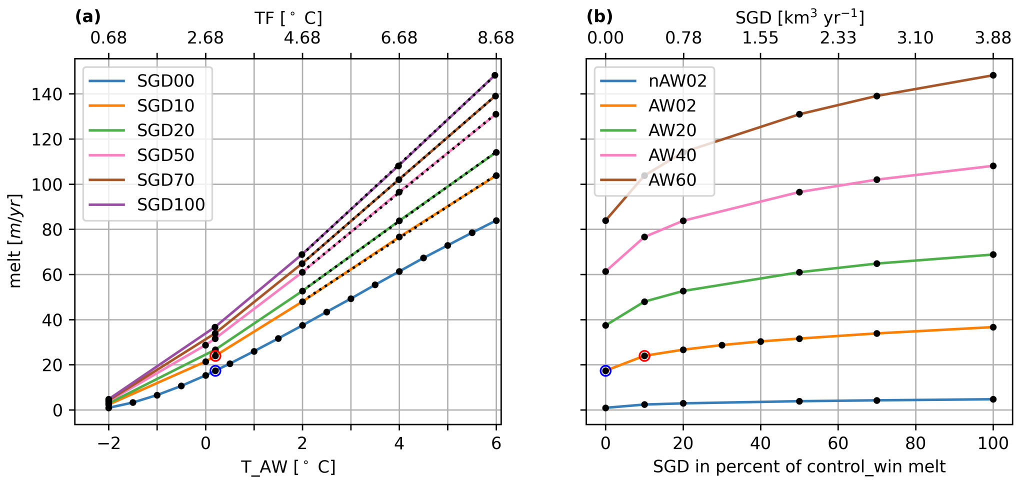

Fig. 7: Same as Figure 7 in the Manuscript but with extended SGD ranges.

38. Line 53. *'a the terminus' -¿ 'at the terminus'* - Thank you, we will correct it.

39. Line 55. *'considers' seems like the wrong word. Maybe replace with 'a different type of ice-ocean interaction occurs for ice shelves'* - Thank you, we will try to find a better formulation.

40. Line 79. *If there is only one other high res numerical modelling study of a Greenland ice shelf you should probably say this.* - Thank you, we will reformulate the sentence accordingly.

41. Line 96. *I don't think 'rise' fits grammatically. Maybe 'suggests' instead.* - Thank you, we wanted to write "raise questions about...". We will correct it.

42. Line 101. *'tide water' vs 'tidewater' consistency and in other places...* - Thank you, we will carefully go to the manuscript and change it to be consistent.

43. Line 114. *'nonhydrostatic' vs 'non-hydrostatic' same as above, probably best to be consistent.* - Thank you, we will carefully go to the manuscript and change it to be consistent.

44. Line 103. *Maybe you could add more references for MITgcm used in polar settings... although you do say e.g....* - If there are more Fjord modelling studies that we missed that use MITgcm, we will gladly add them. We are curious about what you mean by 'MITgcm used in polar settings'. We don't think that paper about the arctic or antarctic are of relevance at this point.

45. Table 1. *units of diffusivity and viscosity should be m2/s* - Thank you, we will correct it.

46. Table 1. *I think the thermal conductivity of ice also has the units of diffusivity here based on the definition in the melt param so as above. (Holland and Jenkins 1999)* - Thank you, we will correct it.

47. Table 1. *I think you use PSU and g/kg in different parts of the text. Probably best to be consistent.* - Thank you, we will carefully go to the manuscript and change it to be consistent.

48. Line 163. *'*' -¿ '+' ?* - Thank you, yes, we will correct it.

49. Line 163, *value of* C_d *in table?* - Thank you, we will add it.

50. Line 168. *'approximated to be increase' -¿ 'approximated to increase'.* - Thank you, we will correct it.

51. Line 172. *Probably don't need to spell out the units.* - We can leave it out.

52. Figure 2a. *not that important but maybe you could have centralised the plot around zero velocity.* - We see your point. Can we suggest to add a vertical line at zero velocity to make it obvious, where zero velocity is. Otherwise we will loose some detail on the positive side. In any case, we can try to make the figure easier to read.

53. Line 248-249. *You use TF before defining in the next sentence.* - Thank you, we will adjust it.

54. Line 259. *I think the sentence is a little confusing. Maybe 'corresponding to' -¿ 'as ... the plume transitions', and maybe you don't need the 'with respect to the ambient stratification'.* - Thank you, we will adopt you suggested formulation.

55. Line 276. *'not' -¿ 'no'* - Thank you, we will correct it.

56. Figure 4a. *Add residual units* - Thank you, we will add it.

57. Line 329. *Add Ryder 'ice tongue'* - Thank you, we will add it.

58. Line 330-334. *I found the brackets a bit confusing to understand the different values – maybe you could rephrase this paragraph.* - Thank you, we will clarify the sentence(s).

59. Line 347. *Is this cai et al 2017 figure 2? If so might help the reader to add this in.* - Thank you, yes, we will add it.

60. Line 407. *I tried to access the set up files at the DOI given but the link did not work.* - Thank you, I notice now that something went wrong with publishing the files. We will make sure it is publicly accessible.

References

[Arneborg et al., 2007] Arneborg, L., Fiekas, V., Umlauf, L., and Burchard, H. (2007). Gravity current dynamics and entrainment - A process study based on observations in the Arkona Basin. *Journal of Physical Oceanography*, 37(8):2094–2113.

[Cai et al., 2017] Cai, C., Rignot, E., Menemenlis, D., and Nakayama, Y. (2017). Observations and modeling of ocean-induced melt beneath Petermann Glacier Ice Shelf in northwestern Greenland. *Geophysical Research Letters*, 44(16):8396–8403.

[Cowton et al., 2015] Cowton, T., Slater, D., Sole, A., Goldberg, D., and Nienow, P. (2015). Modeling the impact of glacial runoff on fjord circulation and submarine melt rate using a new subgrid-scale parameterization for glacial plumes. *Journal of Geophysical Research: Oceans*, 120(2):796–812.

[Dansereau et al., 2013] Dansereau, V., Heimbach, P., and Losch, M. (2013). Journal of Geophysical Research : Oceans. *Journal of Geophysical Research: Oceans*, 2001(3805):1–26.

[Gudmundsson, 2013] Gudmundsson, G. H. (2013). Ice-shelf buttressing and the stability of marine ice sheets. *Cryosphere*, 7(2):647–655.

[Gwyther et al., 2020] Gwyther, D. E., Kusahara, K., Asay-Davis, X. S., Dinniman, M. S., and Galton-Fenzi, B. K. (2020). Vertical processes and resolution impact ice shelf basal melting: A multi-model study. *Ocean Modelling*, 147(February):101569.

[Holland et al., 2008] Holland, P. R., Jenkins, A., and Holland, D. M. (2008). The response of Ice shelf basal melting to variations in ocean temperature. *Journal of Climate*, 21(11):2558–2572.

[Jakobsson et al., 2020] Jakobsson, M., Mayer, L. A., Nilsson, J., Stranne, C., Calder, B., O'Regan, M., Farrell, J. W., Cronin, T. M., Brüchert, V., Chawarski, J., Eriksson, B., Fredriksson, J., Gemery, L., Glueder, A., Holmes, F. A., Jerram, K., Kirchner, N., Mix, A., Muchowski, J., Prakash, A., Reilly, B., Thornton, B., Ulfsbo, A., Weidner, E., Åkesson, H., Handl, T., Ståhl, E., Boze, L.-G., Reed, S., West, G., and Padman, J. (2020). Ryder Glacier in northwest Greenland is shielded from warm Atlantic water by a bathymetric sill. *Communications Earth & Environment*, 1(1):1–10.

[Jenkins, 2011] Jenkins, A. (2011). Convection-driven melting near the grounding lines of ice shelves and tidewater glaciers. *Journal of Physical Oceanography*, 41(12):2279–2294.

[Kantha and Clayson, 2013] Kantha, L. H. and Clayson, C. A. (2013). *Numerical Models of Oceans and Oceanic Processes*, volume 66.

[Losch, 2008] Losch, M. (2008). Modeling ice shelf cavities in a z coordinate ocean general circulation model. *Journal of Geophysical Research: Oceans*, 113(8):1–15.

[Sciascia et al., 2013] Sciascia, R., Straneo, F., Cenedese, C., and Heimbach, P. (2013). Seasonal variability of submarine melt rate and circulation in an East Greenland fjord. *Journal of Geophysical Research: Oceans*, 118(5):2492–2506.

[Slater et al., 2016] Slater, D. A., Goldberg, D. N., Nienow, P. W., and Cowton, T. R. (2016). Scalings for submarine melting at tidewater glaciers from buoyant plume theory. *Journal of Physical Oceanography*, 46(6):1839–1855.

[Straneo and Cenedese, 2015] Straneo, F. and Cenedese, C. (2015). The Dynamics of Greenland's Glacial Fjords and Their Role in Climate. *Annual Review of Marine Science*, 7(1):89–112.

[Straneo et al., 2012] Straneo, F., Sutherland, D. A., Holland, D., Gladish, C., Hamilton, G. S., Johnson, H. L., Rignot, E., Xu, Y., and Koppes, M. (2012). Characteristics of ocean waters reaching Greenland's glaciers. *Annals of Glaciology*, 53(60):202–210.

[Wilson et al., 2017] Wilson, N., Straneo, F., and Heimbach, P. (2017). Satellite-derived submarine melt rates and mass balance (2011–2015) for Greenland's largest remaining ice tongues. *The Cryosphere*, 11(6):2773–2782.