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Will Scott

Title: Basal melt rates and ocean circulation under the Ryder Glacier ice tongue and their response to climate warming: a high resolution modelling study
Author(s): Jonathan Wiskandt et al.
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## 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 Peterman 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.

## Specific comments

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.

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

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?

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?

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.

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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?

109. I think MITgcm is a finite-volume discretisation.

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.

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.

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).

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.

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.

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…

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.

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 paramererisation is very dependent on vertical grid resolution (and grid type) e.g see Gwyther et al. 2020.

Line 182. Is $S_b$ 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.

Line 189. 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.

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…

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.

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?

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.

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.

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?

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?

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 shelfs.

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!

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?

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.

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.

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.

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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. 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.

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,

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?

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.

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

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.

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.

Line 377. I do think the results are interesting and generic.

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.

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 somepoint in the discussion section about the dependence of the melt rate parameterisation on grid resolution.

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## Technical corrections

Line 22. ‘and complex geometries of both, ice and ocean, domains,’ I am not sure you need the commas around ‘ice and ocean’

Line 51. ‘in the southern Greenland’ -> ‘in southern Greenland’

Line 53. ‘a the terminus’ -> ‘at the terminus’

Line 55. ‘considers’ seems like the wrong word. Maybe replace with ‘a different type of ice-ocean interaction occurs for ice shelves’

Line 79. If there is only one other high res numerical modelling study of a Greenland ice shelf you should probably say this.

Line 96. I don’t think ‘rise’ fits grammatically. Maybe ‘suggests’ instead.

Line 101. ‘tide water’ vs ‘tidewater’ consistency and in other places…

Line 114. ‘nonhydrostatic’ vs ‘non-hydrostatic’ same as above, probably best to be consistent.

Line 103. Maybe you could add more references for MITgcm used in polar settings… although you do say e.g.….

Table 1. units of diffusivity and viscosity should be $m^2/s$

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)

Table 1. I think you use PSU and g/kg in different parts of the text. Probably best to be consistent.

Line 163. ‘*’ -> ‘+’ ?

Line 163, value of C_d in table?

Line 168. ‘approximated to be increase’ -> ‘approximated to increase’.

Line 172. Probably don’t need to spell out the units.

Figure 2a. not that important but maybe you could have centralised the plot around zero velocity.

Line 248-249. You use TF before defining in the next sentence.

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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’.

Line 276. ‘not’ -> ‘no’

Figure 4a. Add residual units

Line 329. Add Ryder ‘ice tongue’

Line 330-334. I found the brackets a bit confusing to understand the different values – maybe you could rephase this paragraph.

Line 347. Is this cai et al 2017 figure 2? If so might help the reader to add this in.

Line 407. I tried to access the set up files at the DOI given but the link did not work.