Reviewer comments on egusphere-2022-1296:
Basal melt rates and ocean circulation under the Ryder Glacier ice tongue and their response to climate warming: a high resolution modelling study
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General comments
This is a nice set of experiments on the sensitivity of an idealized Greenland ice tongue cavity to variation in ocean thermal forcing and subglacial discharge of ice sheet runoff. The results are presented in the context of other modeling studies of ice-ocean interactions in Greenland fjords and Antarctic ice shelves.

I have some comments and suggestions for revisions to the paper which fall in two main categories:

a. I think some of the results could be analyzed or explained more fully — particularly regarding the plume buoyancy (both its along-fjord evolution and its relationship to varying thermal forcing) — see starred comments.

b. References to observed/projected changes and connections to the real-world RG-SOF/GrIS systems could be expanded. This will help lend significance to the results and distinguish this paper from a more generic idealized modeling study.

One additional request: Because a large portion of the study focuses on the evolution of the plume itself, a direct comparison to a simple 1-D buoyant melt plume model simulation with the same initial T-S profiles and SGD fluxes could be quite valuable to the community in evaluating the relative benefits of running a high-resolution simulation of this nature.

I hope that these comments are useful in revising the paper and look forward to seeing this work published.

Specific comments
<u>Line 82, etc.</u> It would be great to have a map showing the RG-SOF system with the locations of the grounding line, ice tongue front, sills, and the hydrographic profiles used to initialize the model (as referenced in line 134-135), as well as maybe a smaller inset map showing the location of RG within Greenland.

<u>Line 100.</u> I initially thought this was saying that the sills were within the ice tongue cavity. Adding a map as suggested above would help to clarify this statement. However, I think it would make sense to move this information to the description of the model domain in Section 2.

<u>Lines 172/210 & Figure 1a-b.</u> In you use negative melt rates in a few places but otherwise you use positive values, which I think is more common and intuitive. This should be consistent and I would encourage you to stick to positive values = melting since you don’t talk about refreezing

at all. You can still keep the way you've plotted the melt rates in Fig 1a-b by using a reversed y-axis.

Line 177. You could add a very brief intro paragraph (2-3 sentences) to Section 2.2 referencing Table 2 and A1-A2.

Lines 179-184.

1. The title of this subsection is "Oceanic thermal forcing" but then you use the term "temperature forcing" throughout the rest of the paper. I think thermal forcing is more widely used but either way, would be good to stick to one term.
2. This had me wondering (a) what typical values are for T_b in this system and (b) how T_AW is related to T_GL (i.e. is there significant mixing that occurs along the inflow pathway). From Table 2, my impression is that T_b is roughly constant at -2.68º and that the water reaching the grounding line is effectively unmodified AW. This is something you could state explicitly, i.e. TF can be estimated as T_AW+2.68º (as you later use in Fig 4).

Lines 185-190.

1. Could you expand a little on the values of SGD volume flux used here? I understand the general reasoning for referencing percentages of winter basal melt flux for comparison, but it would be helpful to compare the resulting values to any existing estimates of SGD volume flux (e.g. see Supporting Info S03 for Slater et al. 2022 in GRL https://doi.org/10.1029/2021GL097081 — bearing in mind that those fluxes are integrated across the grounding line while you are considering a 10 m slice, and the horizontal distribution of SGD is also likely relevant to its overall impact on basal melt, as you note elsewhere)
2. What is the vertical extent of the plume as you initialize it? Is this a typical approach to implementing SGD in this type of model?

Line 221 (& Figure 1d). It's difficult to see the differences between the simulations in figure 1d. Would it be possible to e.g. add an inset in the lower left zooming in on the lower part of the pycnocline that you reference here?

Lines 237-239. Figure 1c does not show the plume velocity dropping to zero. This made me wonder about your definition of the plume vs the outflow jet — is the outflow jet part of the plume or is it distinct? (If it's the latter you might need to refine your definition of the plume in line 223.) Does it have to do with the acceleration becoming negative? The buoyancy becoming negative?

Lines 239-241. I think by "T-S transition layer" you mean a layer of glacially-modified waters. It would be nice to see this on a T-S plot, but even without one, you could describe this more explicitly (i.e. compared to the idealized initial profiles, the outflow is colder and fresher, consistent with the signature of melt-modified ocean waters).

Lines 252-253. It took me a little while to understand what you meant by "sharpening" the pycnocline here because I was looking at the wrong part of the profile in Fig. 2b — maybe you could clarify that you're talking about "the base of the pycnocline"?

*Lines 259-260. Why is there a sudden increase in buoyancy at the regime transition? It's even more striking in the summer/SGD simulations in Fig 5d but it also happens in 3d, and this is counterintuitive to me. Is it related to the definition of buoyancy in Line 254? Since you're defining your plume using a velocity condition, is rho_p an average over the plume thickness, and rho_a is an average over the same depth range at x=21km? I'm wondering if something funky happens in that calculation as the plume reaches the pycnocline and thickens.
Another possibility is that if the isopycnals are sloping significantly (hard to tell in Fig 1a-b) comparing the plume density to such a distant reference may not be ideal.

Line 269-270. Reporting the value of T_c as a range between two experiments seems confusing to me (took me a while to connect this to Table A1 and understand where these values came from). I think you could simply write something like "…for experiments with a temperature forcing of TF_c = 3.18ºC or greater (Figure 4a)."
In the following sentence, I initially interpreted "across the whole TF range" as including TF<TF_c, which isn't the case/I don't think is what you meant, so could be rephrased to clarify. It's nice to see the reduced residuals; could you also report the R^2 and p values for the fits here? Did you do any fitting of the range of TF<TF_c?

*Lines 273-281 and 368-377. This is a nice plot (4b) and interesting to think about. I think the interpretation requires a little more careful consideration here.
You've established that melt rate increases linearly with TF above ~3º. This should correspond to roughly linear decreases in plume temperature and salinity (relative to ambient). But the change in buo-T and buo-S will also depend on the changes to the ambient stratification that you have imposed. I think this is why buo-S begins to level off (while buo-T changes linearly as the melt concentration increases).
Consider that varying T_AW while holding S_AW and PW properties constant will change the ambient stratification and dT/dS slope. This in turn will affect the relationship between plume properties and ambient properties at a given density. I am finding this difficult to explain clearly so I'm putting a little cartoon at the end of this document in case you want to think about it more.
Do you have another explanation in mind for why the relationship between buoyancy and TF changes? Whether or not I am correct about the mechanism I think it merits a bit more thorough discussion to make clear under what conditions this result may be expected to hold.

Lines 290-292. Could the simulations with the increased T_AW be omitted here, until the paragraph beginning line 304, to keep the structure more straightforward? Also, in the previous subsection, the simulation names from Table 2 aren't used in the text, so it would be nice to keep this consistent.

*Lines 302-303. Just re-upping the point that while the variation in buoyancy after x=4 km or so makes sense to me, it is unclear to me why it increases abruptly around the point of the regime transition.

Lines 305-308. I think this could be expanded to at least a full sentence or two for each of these points. For point (i), it would make sense to show the regression(s) for Fig 7a to compare to the results in 4a.

Lines 308 and 397-398. Could you make a more quantitative comparison to e.g. the x^1/3 relationship found by Slater et al. (2016)?

Lines 390-392. My understanding of the Slater and Straneo (2022) paper is that it is more about the changes under realistic forcings, so this statement could be made much stronger by comparing the experiments here to observed and projected changes (see the dataset linked above in comment on lines 185-190).

Technical corrections

Line 128. Ice tongue terminates in a 950 m deep front, or 50 m above the sea floor (not 50 m deep)

Line 143. border (not boarder)

Line 254. Check that units here match y-axis of Fig 3d/5d?

Line 319. SGD (not SDG) — spotted in Fig 7 caption as well

Line 333. Reference Table 2.

Figures

Figure 1. A legend showing dashed line = summer and solid line = winter would be helpful in 1c/d.
On my screen, the dotted line in 1d looks green (not blue as stated in the caption).
What are the small blue and orange horizontal lines in 1d?

Figure 2. In last sentence of caption, could you add "The dotted horizontal lines in (b)..."

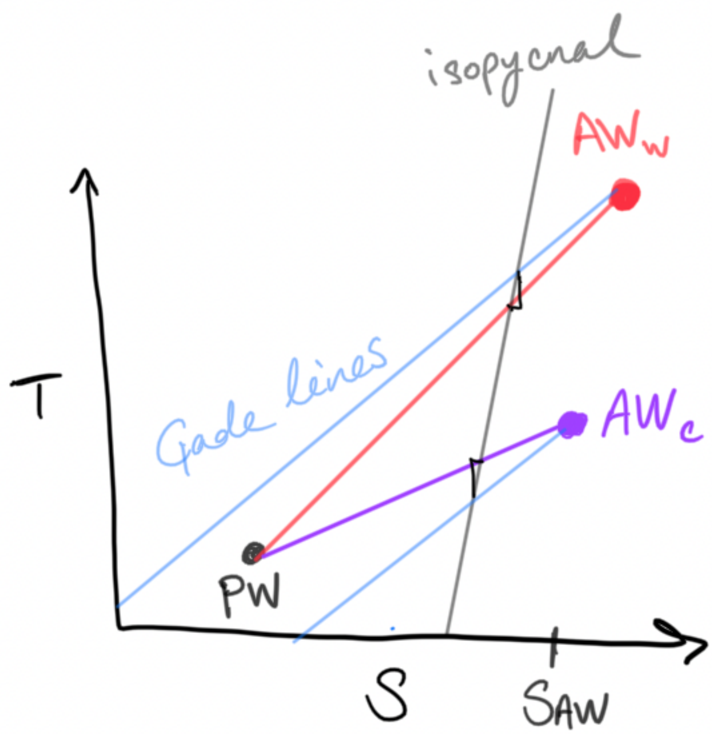
Figures 2a & 6a. It would be helpful to darken/otherwise distinguish the vertical grid line at u=0 to emphasize the change of depth of velocity reversals.

Figure 4a. Could you highlight (e.g. circle) the point corresponding to the control simulation here?

Tables

Table 2. Would be nice if you could further highlight the two control simulations here since the winter control is in the middle of the AW temp range (light grey shading of those rows?), and maybe add a dashed line between sgd100_AW02 and sgd010_AW20 to separate the two sets of summer experiments.
In caption, I'm not sure it's necessarily correct to imply that melt rate and ice retreat are equivalent (in your model, the ice base position is static, I think? And in reality, "retreat" would also depend on ice flow divergence?)

Re: ocean variability, ambient stratification, and plume buoyancy —



The slope of the Gade line can be approximated as constant. It shows that melting of ice by ocean water always creates a mixture that is colder and fresher than the water doing the melting.

Changing T_AW while holding S_AW and PW properties constant changes the slope of the mixing line between AW and PW.

In this example the cold AW (AW_c) has a shallower slope than the Gade line. The resulting mixture of AW_c and meltwater is colder and fresher than ambient water (a mixture of AW_c and PW) at the same density.

The warm AW (AW_w) has a steeper slope than the Gade line. The resulting mixture of AW_w and meltwater is warmer and saltier than ambient water at the same density — even though it is colder and fresher than the AW_w itself.

This is probably more extreme than what might be happening in the experiments here but I think the general concept might be relevant — the ambient profile is getting less stratified with a stronger temperature gradient, and the AW-PW mixing line is getting closer to the Gade slope so the salinity contrast between the plume and the ambient at a given density is getting less pronounced.

Long story short: I think it's just worth noting that the ultimate response of plume buoyancy to AW temp/TF is not straightforward and likely depends on variation in other properties as well.

Some observational context — in 79 North, AW got both warmer and saltier between 2009 and 2016, as well as over the course of the year in 2016-17, so there's reason to expect that these might covary on inter-/intra-annual timescales (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2020JC016091).
Petermann Gletscher — Washam et al. 2018 (https://journals.ametsoc.org/view/journals/phoc/48/10/jpo-d-17-0181.1.xml).
NE Greenland water mass variability — Gjelstrup et al. 2022 (https://www.nature.com/articles/s41467-022-35413-z).