Response to Reviewer 1

We thank the reviewer for providing constructive and positive critique of our manuscript. The points raised are relevant and interesting. We also thank the reviewer for pointing out numerous typos. Below, we propose a response to the reviewer's comments.

Substantial comments

I wonder about the applicability of the model to systems where subglacial discharge is significant (by which I mean that the subglacial discharge flux is comparable to or greater than the basal melt flux). I feel that many (most?) marine-terminating glaciers without ice tongues are likely to fall into this category, at least in summer. This comment has two parts:

(i) On neglecting subglacial discharge in the freshwater budget (L89) – is this appropriate? Based on Table 2 the observational estimates for the basal melt flux, M, are 60 m3/s at Ryder, 300 m3/s at Petermann and 600 m3/s at 79N. Cai et al. (2017) suggest based on RACMO2.3 surface runoff that subglacial discharge at Petermann can reach over 1000 m3/s in summer, which would significantly exceed the M term that is accounted for in the freshwater budget. On the other hand, Schaffer et al. (2020) suggest that only 11% of freshwater leaving the 79N cavity is subglacial discharge, which would support neglecting subglacial discharge. I feel a few more sentences justifying this assumption are needed. Also, for glaciers without ice tongues, it is much more likely that the subglacial discharge will significantly exceed the submarine/basal melt flux, so is the model applicable to fjords with tidewater glaciers, as you say in L53, when the subglacial discharge is neglected in the freshwater budget?

(ii) On the choice of the exponents n1 and n2 (L142 and discussion shortly after). For systems with high subglacial discharge, the buoyancy of the plume can be dominated by the subglacial discharge, so that the plume volume flux (and plume velocity) becomes independent of the thermal forcing and scales only with the subglacial discharge raised to the power 1/3. Some studies that investigate this regime are Jenkins 2011 and Straneo and Cenedese (2015) ? see in particular Eqs. 7 and 8 of the latter study. This subglacial discharge-dominated case would have n2=0 and n1 would be close to 1. I think it would be great to mention this possibility when discussing values for n1 and n2. And, if the model is to be widely applicable across Greenland fjords, do the results change much if n2=0? Or is this already a sub-case of what you have presented? I appreciate that this might require a lot extra to look into properly and that is not what I am proposing – maybe just a short consideration of how n2=0 might change things.

Overall, this substantial comment is not really a criticism of the paper and doesn?t require major changes to address, but would be worth considering as I think it has a bearing on how widely applicable the model would be.

Response to to substantial comments

The reviewer is right in pointing out that our assumption that the freshwater source from subglacial discharge (D) is small compared to the freshwater source from basal melt (M) does not generally apply to tidewater glaciers in summer when surface melting is strong. Some modifications of the model are required to describe cases where the subglacial discharge is much greater than the basal melt ($M \ll D$). We will revise the manuscript to make this clear.

Substantial comment (i): How large is M/D on the large Greenlandic ice tongues?

The reviewer asks relevantly if our assumption that $M \gg D$ is valid for the large ice tongues of 79°N, Ryder, and Petermann. Moored hydrographic and velocity observations from the 79°N Glacier, reported by Schaffer et al. (2020), suggest that in the annual mean the subglacial discharge constitutes only about 10% of the freshwater exported from the glacier , i.e. $D/M \approx 0.1$. In addition, hydrographic observations taken in drill holes on Petermann Ice Tongue in August 2015 suggest that the freshwater fractions due to subglacial discharge in the glacially-modified near-ice water column is less than 30% (see Fig. 5 in Washam et al., 2019). These observations suggest that $D/M \ll 1$ can serve as a leading-order model approximation for 79°N, Ryder, and Petermann.

However, estimates of surface summer melt in the upstream catchments of these ice tongues based on regional atmospheric modelling show that this seasonal freshwater source (an upper-bound on the summer subglacial discharge) can be a few times larger than the annual-mean freshwater source due to basal melt on the ice tongues. On the other hand, the basal melt increases with the summer increase of subglacial discharge. Using data reported in the literature on annual-mean basal melt and subglacial discharge (JJA surface melt values dived by 4) (Wilson et al., 2017; Schaffer et al., 2020; Stranne et al., 2021; Slater and Straneo, 2022), we obtain annual-mean D/M values of about 0.8, 0.3, and 0.7, respectively, for Ryder, 79°N, and Petermann. The results of Cai et al. (2017), who modeled basal melt on Petermann Ice Tongue, suggest that in summer (JJA) $D/M \sim 1$ and in the annual-mean $D/M \sim 0.7$.

Suggested response: In the beginning of section 2, we briefly state that observational and modelling results suggest that $D/M \ll 1$ can serve as a leading-order approximation when examining how hydraulic constraints affect the melt dynamics on large ice tongues such as 79°N, Ryder, and Petermann.

Substantial comment (i): validity of the conceptual model

It is relatively straightforward to include subglacial discharge D in the conservation relations for mass, salt, and heat that are derived in section 2.1 in the paper. The novel feature is that Eq. (10) in the paper – the relation between the difference in salinity (ΔS) and the temperature (ΔT) between the two layers –

depends on the subglacial discharge:

$$\frac{\Delta S}{S_A} = \frac{\Delta T}{S_G}\Gamma, \tag{1}$$

where we have introduced

$$\Gamma \stackrel{\text{def}}{=} \left(\frac{M}{M+D} + \frac{T_A - T_f}{T_G}\right)^{-1}. \tag{2}$$

By noting that $(T_A - T_f)/T_G \sim 0.05$, it follows that $\Gamma \geq 1$; and we can identify three limiting cases depending on the value of D/M:

1. When $D/M \ll 1$, $\Gamma \approx 1$. This is the limit considered in the paper.
2. When $D/M \sim 1$, $\Gamma \approx 1 + D/M$.
3. When $D/M \gg 1$, $\Gamma \approx T_G/(T_A - T_f)$. This implies that $\Delta S/S_A \approx \Delta T/(T_A - T_f)$, which is the relationship between salinity and temperature changes when freshwater at the freezing temperature is mixed with Atlantic Water. This limit is approached when $D/M \sim 50$ or greater, and should be appropriate for tidewater glaciers with high subglacial discharge.

As long the subglacial discharge is small compared to the exchange flow in a fjord (Q), the inclusion of the factor Γ in Eq. (1) is the only modification of the model needed for treating cases where the subglacial discharge is not small compared to the melt. The primary effect of increasing Γ is to strengthen the layer density difference. Essentially, this causes the transition into the hydraulically-controlled regime to occur for somewhat greater sills heights than in the limit where $D/M \ll 1$. These considerations indicate that the model presented in the paper is qualitatively correct also when $M \sim D$.

Suggested response: In the revision, we will include an appendix that derives Eq. (1) and discusses briefly how strong subglacial discharge qualitatively modify the model results. To replace Eq. (10) in the paper with Eq. (1) above, however, gives an algebraically more complex model. Accordingly, we will stick to the conservations relations given in 2.1, which are formally valid when $D/M \ll 1$.

Substantial comment (ii): the choice of the exponents n_1 and n_2

The reviewer suggests that the case of melt parametrisation with $n_1 = 1$ and $n_2 = 0$ is relevant for systems with high subglacial discharge, and that this case would be interesting to discuss. In fact, we consider this case (see Fig. 10), without mentioning that it may be relevant for systems with high subglacial discharge.

Suggested response: In the revision, we will discuss the results in Fig. 10 (c,d) in relation to systems with high subglacial discharge. We will mention that the model results are formally valid only D/M, with reference to the proposed new appendix.

Minor comments

L14 – the use of "marine ice" – I worry that this terminology could be a bit confusing. I'd suggest rephrasing using "marine-terminating glaciers".

We will revise as suggested.

L26 – Slater et al., 2022 recently argued that for some regions, the impact of increasing subglacial discharge on submarine melt has been as important as AW temperature – could be worth acknowledging here.

We propose the reformulate in the following way: *In Greenland, basal melt is sensitive to the AW temperature (Straneo and Heimbach, 2013), and increases in AW temperature and subglacial discharge have been the major drivers of the retreat of outlet glaciers in deep Greenlandic fjords since the mid 1990s (Wood et al., 2021; Slater and Straneo, 2022).*

L32 – "can stabilise marine glaciers" – I feel this statement is too certain for this point in the paper. Perhaps "has the potential to stabilise marine glaciers"?

We will revise as suggested.

L34 – either here or somewhere else appropriate, I think it would be worth acknowledging that processes other than hydraulic control can also modify AW between the shelf and the glacier – for example vertical mixing due to velocity shear even in the absence of a sill, or icebergs.

We propose the reformulate in the following way: *Numerous observations of sill flows demonstrate that the vertical mixing increases strongly when the flow becomes hydraulically controlled (Pratt and Whitehead, 2007), and Jakobsson et al. (2020) and Schaffer et al. (2020) show that as inflowing AW passes over the sills and descends on the landward slopes, it mixes with overlaying cold glacially-modified water.*

Fig. 2, panels b and c – it would be great to have a scale bar for these panels.

We will fix this.

Fig. 3 – it would be great to have the locations of these profiles shown on Fig. 2b and 2c

We will examine if this is compatible with readability of Fig. 3; if not we will – in the figure caption – refer to locations in Fig. 2.

L78 – it would be nice to finish off the introduction with a sentence that bridges into the next section. For example, "We now describe a two-layer model to investigate .."

We will do as suggested.

L122 (and a few other places) – it would be more consistent to refer to "Eq." instead of "relation"

We will follow this suggestion.

L129 – is the value of rho0 ever actually used in the model? Or does the density difference always get normalised by rho0 (e.g. Eq. 22), in which case there would be no need to assume a value for rho0.

This is correct and we will simply write "where ρ_0 is a constant reference density ..."

L198 – I don't quite follow why the exchange flow increases with deltaT when $n_1 - n_2 > 1$. From Eq. 21, don't we require $n_2/(n_1 - n_2) > 0$? Which would give $2n_2 - n_1 > 0$, but perhaps I am mistaken.

We have assumed that $n_1 \geq 0$ and $n_2 \geq 0$, but did not state that clearly. When $n_2 \geq 0$ it is simple to see that $n_1 - n_2 > 0$ is the relevant criteria. We will state that $n_1 \geq 0$ and $n_2 \geq 0$ when the exponents are introduced.

L200 – I think somewhere in this paragraph it would be appropriate to cite Zhao et al. (2021), which similarly looked at parameterising hydraulically-controlled transport (e.g. Eq. 17 in that paper).

This paper is very relevant. We will cite the paper around L200 and also on L45.

Fig. 4 – could you say how the axes are non-dimensionalised?

The axes are non-dimensionalised such that the AW height is one when the AW thermal forcing is one; we will state this. Equation (28) shows that non-dimensionalisation can be done by selecting an arbitrary scale for the AW thermal forcing ($\mathcal{T}_A$), and define a non-dimensional h that is one when non-dimensional $\mathcal{T}_A$ is one.

L315 – suggest adding "in the case n1=2 and n2=1" at the end of the first sentence

We will follow this suggestion.

Figs. 6 and 7 – it could be better not to use the jet colorscale

We think that this is partly an aesthetic matter, and the jet scale is preferred by a color blind coauthor.

Fig. 12 caption – "see the text for details" – did this mean to look at the text for details on the refreezing, or for details on the figure more generally? I took it to mean details on refreezing, and I think I didn?t see those, so perhaps revise.

We will revise the caption to make this clear.

L492 – suggest adding "unmodified" before "AW".

Good suggestion!

L506 – on the two idealised scenarios – can you speculate which might be more realistic? Scenario 2 feels more realistic to me because there is more a gradual transition from no entrainment to some entrainment, but perhaps we are not able to say yet.

Scenario 1, which assumes no entrainment, is extreme and less likely: the observations from Ryder and 79N show that entrainment occurs. Thus, scenario 2 is more realistic. This is also supported by the paper of Bao and Moffat (2023), which has recently been published in Cryosphere Discussion. They report an ocean-modelling study of glacial melt in a silled fjord, and their results support scenario 2. We will state this around L506 and also in in the beginning of section 3.

L536 – on Ryder and 79N having "basal melt processes that are less sensitive to thermal forcing than Petermann" – surely according to your model, the basal melt processes at all of the glaciers are equally sensitive to thermal forcing (because M varies as T^{n_1})? So is the higher melt rate at Petermann likely due to factors beyond thermal forcing (i.e. gamma1 in your equations), such as subglacial discharge or grounding line depth or basal slope?

Good point! We will change to: "*basal melt processes characterised by lower thermal sensitivity coefficients γ_1/A than Petermann*".

L538 – suggest adding "at 79N" after Schaffer et al. (2020)

We will follow this suggestion.

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