Second Response to Editor for Manuscript "High-Fidelity Modeling of Turbulent Mixing and Basal Melting in Seawater Intrusion Under Grounded Ice"

by Mamer, Robel, Lai, Wilson, and Washam

Dear Madeline Mamer and co-authors,

Both reviewers acknowledge the work done to improve your manuscript but still have major concerns. They both find that the salt flux at the ice–ocean interface remains poorly represented, which is a concern for the fidelity of simulated melt rates. They also both notice that this study de-facto investigates a quasi-laminar flow, which is not sufficiently acknowledged. In the end, they think that the modelling tool (ANSYS Fluent), in the way it is implemented here, is not fit for the targeted questions.

Would you be able to address the first concern by clarifying and improving the representation of the salt flux at the ice–ocean interface (S_b≠0)?

The simulations currently presented in the manuscript have a boundary salinity that is permitted to evolve in time according to both the intrusion and any simulated meltwater flux. The boundary salinity, S_b, is set to the near-ice grid cell (scale of nm's) which has an evolution equation given by B11 (now equation B12 in the revised manuscript). This evolution equation accounts for advective and diffusive transport mechanisms. The boundary salinity is used in equation 1 (the liquidus condition) to set the thermal boundary condition for the ice interfaces. The values for S_b can be clearly seen in Figure 3 (panels B, F, J) at y = 0 and range from 0 ppt to ~ 22 ppt.

In the reference simulations, S_b only evolves due to advection and diffusion from the salt wedge entering the subglacial domain and the freshwater layer exiting the subglacial space as a plume. When we run the simulations with melting, S_b is diluted as fresh, cold water enters the domain normal to the ice boundary. The dilution occurs because Fluent is a finite volume solver and so balances inflows and outflows across each control volume in the grid, while conserving overall mass balance in the domain via a pressure outlet (red arrow Figure 1).

We have made edits to pages 4-5, page 16, and page 28 to clarify this confusion.

For the other concerns, I think that the caveats could be acknowledged in the abstract, the title could be revised without claiming "high-fidelity", and the implications of these caveats could be thoroughly discussed in section 4.

We have edited the title to be:

"Modeling Mixing and Melting in Laminar Seawater Intrusions Under Grounded Ice."

and amended line 5 in the abstract:

“In this study, we investigate turbulent mixing of quasi laminar intruded seawater and glacial meltwater under grounded ice using a computational fluid dynamics solver.”

Other mentions of ANSYS Fluent being a high-fidelity solver have been removed.

Furthermore, to address concerns about the simulations being quasi-laminar, we have added to line 81:

“In the experimental set up in this study, we only consider quasi laminar flow in order to facilitate comparison to previous studies (Wilson et. al 2020, Robel et. al 2022) which find that subcriticality ($Fr < 1$) is required for intrusion development .”

Other concerns by the reviewers including buoyancy impacts on turbulence, the language on diffusion driven melting, adequacy of salt diffusive transport, and issues of interfacial shear instabilities have been addressed in the individual reviewer responses.

I expect a point-by-point response, at least to the major comments by the two referees. If needed and to save time, we can exchange by email on the way forward for some of these comments or for the manuscript in general.

Best regards

Nicolas Jourdain

Second Response to Reviewer 1 for Manuscript "High-Fidelity Modeling of Turbulent Mixing and Basal Melting in Seawater Intrusion Under Grounded Ice"

by Mamer, Robel, Lai, Wilson, and Washam

We appreciate the reviewer's suggestions and thorough comments on the manuscript. We have addressed their concerns below. The reviewer's original text is written in black, and our responses are written in red.

The authors have significantly improved their paper and I can see that how their study will contribute to the understanding of grounding zone dynamics. However, I would like to see additional improvements before it is published. My main criticism is on the boundary conditions implemented, which never really faithfully represent ice-ocean interactions. I believe that the problem being solved isn't a straightforward approximation of the ice-ocean interaction problem, because the authors have to handle the limitations of the Ansys fluent solver. I have made suggestions below to improve discussions of differences (and similarities) between the target problem and the one solved.

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General main comments
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1) One key limitation of the present work is that the turbulence closure is independent of stratification. This choice may explain why changing the equation of state has limited impact on the dynamics. It should be stated in conclusions that future work should consider more realistic equations of state as the equation of state might significantly impact double diffusive effects, which might be important at low Reynolds.

Buoyancy effects are included in turbulence production by a buoyancy source term represented by G_b in the evolution equation for turbulent kinetic energy (eq B5). Since eddy viscosity is a function of turbulent kinetic energy via the chosen turbulent closure (eq 3), inherently stratification (and any buoyancy effects) will be represented. For the sake of readability, we did not explicitly define this term in the appendix, however, we have now edited line 592 to include:

"Buoyancy effects are included in the evolution equation for turbulent kinetic energy by a source term, G_b, which can be solved for by:

$$G_b = g_i \frac{\mu_T}{\rho Pr_T}\frac{\partial \rho}{\partial x_i},$$

where g_i is the component of gravity in the i^{th} direction, μ_T is the turbulent viscosity, and Pr_T is the turbulent Prandtl number (0.85 default value). The density gradient $\frac{\partial \rho}{\partial x_i}$ is taken over the i^{th} direction. Buoyancy effects are not included in the dissipation equation due to a higher degree of uncertainty (ANSYS 2022)."

2) It should be clearly stated (in abstract and anywhere else appropriate) that this study explores subglacial flows that are quasi laminar (hence in agreement with earlier studies by e.g. Wilson et al 2020?). This statement would certainly make any inspection of the turbulence closure scheme less critical than expected for environmental flows. To support this point, could the authors show a map of the time-averaged TKE/KE, with TKE the pointwise turbulent kinetic energy and KE the resolved kinetic energy? (note that I already greatly appreciated the plot of turbulent viscosity) It might be possible to find in the literature papers discussing the stability of two-layer exchange flows. In fact, the authors might want to have a look at some of the papers by Adrien Lefauve on the inclined duct experiments, which features a two-layer exchange flow (e.g. "Buoyancy-driven exchange flows in inclined ducts" JFM 2020).

In previous theoretical studies of subglacial intrusions (e.g. Wilson et. al 2020 and Robel et. al 2022), the Froude number (equivalent to $u/\sqrt{g'H}$) has to be subcritical (< 1) for an intrusion to develop. For the geometry of interest in this study, this subcriticality requirement requires subglacial discharge velocities < 0.05 m/s for an intrusion to develop. This is why all flows explored here are quasi laminar. To explore more turbulent flows in an intrusion regime (Reynolds number > 2500) the domain would have to be a magnitude greater than tested here, which would be much more computationally expensive.

We have edited the title and abstract to represent this choice of flow domain.

New title: "Modeling Mixing and Melting in Laminar Seawater Intrusions Under Grounded Ice."

Edited line 5 in the abstract:

"In this study, we investigate turbulent mixing of quasi laminar intruded seawater and glacial meltwater under grounded ice using a computational fluid dynamics solver."

Furthermore, at line 81 we have added:

"In the experimental set up in this study, we only consider quasi laminar flow in order to facilitate comparison to previous studies (Wilson et. al 2020, Robel et. al 2022) which find that subcriticality (Fr < 1) is required for intrusion development."

3) More generally, it would be good to be able to disentangle quantitatively turbulent-mixing effects from laminar-geometry effects (i.e. having proper diagnostics for either types of effects). In particular, I find that the idea of a "well mixed" water column (L259) lacks support for turbulent mixing effects.

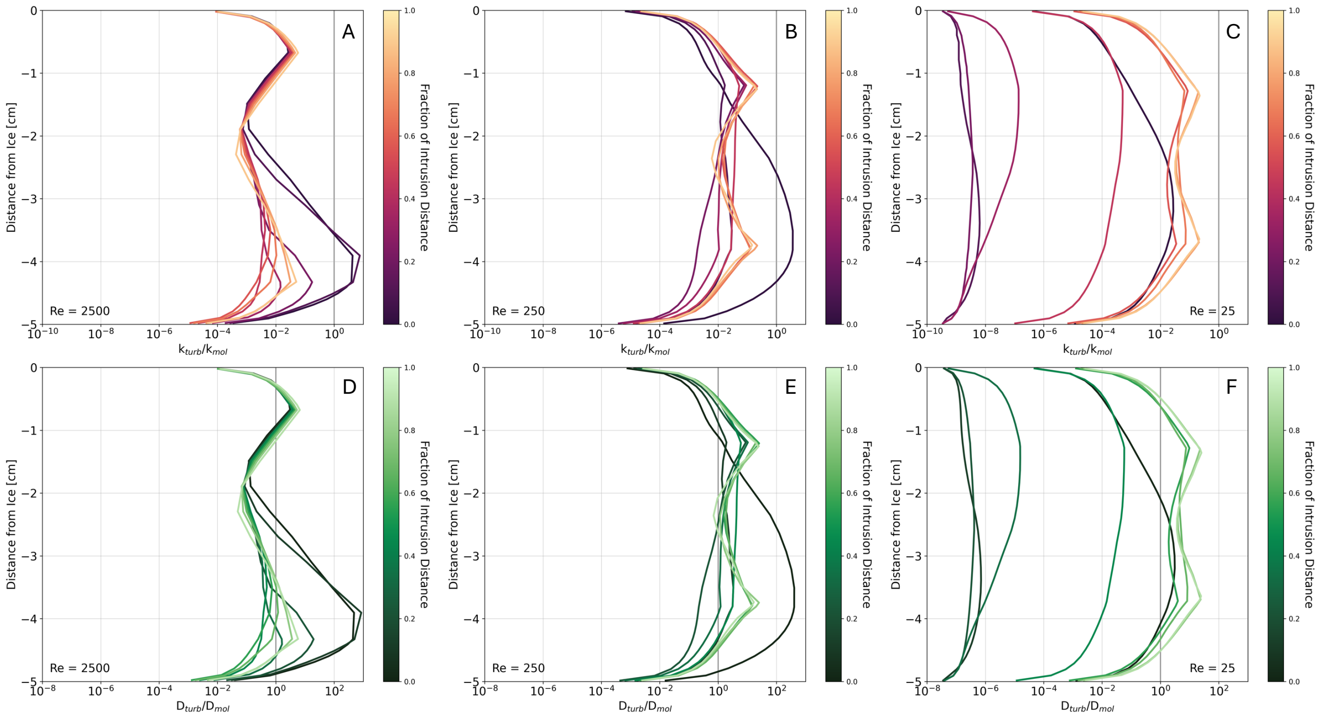

Figure R1. Profiles of turbulent diffusivities of heat (panels A-C) and salt (panels D-F) normalized by their respective molecular diffusivities from the simulation's last time step. Panels A and D are for $u_f = 5.0$ cm/s, panels B and E are for $u_f = 0.5$ cm/s, and panels C and F are for $u_f = 0.05$ cm/s. The simulations presented in this figure are for the medium turbulent mixing case where $C_\mu = 0.09$. The plotted grey line represents unity, where turbulent diffusivity and molecular diffusivity are equivalent. Darker profiles are near the grounding line and lighter profiles are upstream into the subglacial space along the intrusion distance. Values greater than one indicate turbulent mixing is stronger than diffusive mixing and vise versa.

We appreciate the reviewer bringing this to our attention. Here, we have plotted the turbulent diffusivities of heat and salt normalized by their respective molecular values. In general, turbulent mixing plays a relatively negligible role in transporting heat (Panels A-C), however, has more significance in transporting salt (Panels D-F). Notably, near the boundaries, molecular diffusion dominates as expected in a typical boundary layer. As this reviewer comment suggested, the well mixed behavior of the lowest subglacial discharge (i.e. Figure 3 panels I-L) is due to similar advective and diffusive timescales, not turbulence induced mixing.

We have edited lines 269-272 to clarify this:

"For the lowest freshwater flux, the diffusive and advective timescales are of the same order, which leads to homogeneity in the vertical distribution of heat and salt (Figure 3I-L). In estuaries, strong tides generate shear at the bed, increasing turbulence and vertical mixing

(Montanga et al. 2013). Tides may play a similar role in subglacial seawater intrusion regimes, however, that is not simulated here."

4) I would like to see the three exact equations for the liquidus, dilution and cooling rates at an ice-water boundary in the main text and a discussion of how these three equations are approximated and implemented in the RANS model (this might be the comment I care most about).

The liquidus condition is given by equation 1 at line 121. This equation is used to set the thermal boundary conditions of the ice boundaries.

We have modified line 120 to explicitly state this:

"The ice wall boundaries have a pressure and salinity-dependent thermal boundary condition represented by the liquidus condition:"

The heat flux is calculated via heat conservation at the ice boundary and is given by equation 4 at line 207. This melt rate leads to a mass flux of fresh (zero salinity), cool (at the pressure-salinity dependent freezing point) water that dilutes and chills the fluid domain.

The boundary salinity (e.g. S_b in eq. 1) is set to the boundary adjacent grid point (scale of nanometers) and is permitted to evolve via its own evolution equation (eq. B12). The values for S_b can be clearly seen in Figure 3 at y = 0 which range from 0 ppt to ~ 22 ppt. The incoming freshwater set by the melt rate (eq. 4) has zero salinity since it is representing 'melted' ice and therefore dilutes the salinity of the ice-adjacent grid cell (e.g. S_b).

To demonstrate how the dilution works, we can revisit that fluent is a finite volume solver, which means it balances inflows and outflows of each finite mesh element and conserves overall domain mass via a pressure outlet (red arrow in Figure 1). Dilution of the near-ice grid cells (S_b's in our model) happens because of this balancing. For example, we consider a simpler case of the salinity evolution equation which only considers advection across two dimensions (ANSYS Fluent considers more dynamics as shown in equation B12):

$$\frac{DS_b}{Dt}\rho_b = -u\nabla\rho_b S_b$$

In discretized form at steady state this would look like:

$$\dot{m}S_i + u_{b-1}S_{b-1} + u_bS_b = 0$$

Where the melt rate (~10^{-6} m/s) is given by $\dot{m}$ and enters the grid point normal to the boundary and u_{b-1} (~10^{-3} m/s) represents the inflow velocity and u_b represents an outflow velocity. The ice salinity is represented by S_i (0 ppt), the inflowing salinity is given by S_{b-1} (34 ppt), and the unknown local grid point salinity is S_b. The outflowing velocity must balance in the two inflowing ($\dot{m}$ and u_{b-1}) and is therefore slightly greater than u_{b-1}. The boundary salinity will therefore be anywhere between the 34 ppt and 0 ppt based on the relative fluxes of melting and background inflow velocity. In the model, more complicated dynamics of diffusion are also occurring which will modify the boundary salinity in addition to the advective effects demonstrated in this example.

We recognize the misunderstanding of salinity boundary condition is due to our previously poor explanation in the methods, especially the misleading line 112 in the original manuscript where we state, "Both ice faces have zero salinity and no salt diffusion across the boundary." This line's intention was to state the ice salinity is zero and we don't diffuse ice into any artificial ice block. However, its placement reads as the boundary salinity, S_b, is zero, which is not true in these simulations.

We have made edits to line 123 to clarify this confusion:

"The boundary salinity, S_b, is the salinity of the cell filled with water nearest to the ice face and is permitted to evolve during the simulation via the evolution equation B12. For the reference simulations, the boundary salinity evolves in time and space due to advection and diffusion of the subglacial discharge and seawater intrusion. In the simulations with melting, an additional source of freshwater is injected into the near-boundary grid cells, actively freshening the boundary salinity."

And added to the appendix at line 621:

"The boundary salinity, S_b, is set to the near-ice grid cell (scale of nm's) which has an evolution equation given by B12. This evolution equation accounts for advective and diffusive transport mechanisms. The boundary salinity is used in equation 1 (the liquidus condition) to set the thermal boundary condition for the ice interfaces. In the reference simulations, $S_b$only evolves due to advection and diffusion from the salt wedge entering the subglacial domain and the freshwater layer exiting the subglacial space as a plume. For the simulations with melting, S_b, is diluted as fresh, cold water enters the domain normal to the ice boundary. This dilution occurs because Fluent is a finite volume solver and so balances inflows and outflows across each control volume in the grid, while conserving overall mass balance in the domain via a pressure outlet (red arrow Figure 1). The thermal boundary condition (given by equation 1) adjusts based on this dilution. Similarly, the chilling of near-ice waters occurs due to local injection of meltwater at the freezing point."

5) The lack of salt flux (sink) at the ice-ocean interface due to melting remains the biggest model limitation. Arguably the salt flux would be small where the ice is in contact with freshwater but would become non negligible where the seawater intrudes close to the ice. The implementation of a buoyancy flux to mitigate this limitation is interesting but the switch to a free-slip boundary is unfortunately far from satisfactory.

We recognize that switching the boundary condition to one that allows for a mass flux of fresh, cool water is not the ideal approach to modeling this boundary, however the simulations including melt are merely an exploratory case to investigate what would happen under a scenario where dilution and cooling occurs. We agree this is a model limitation and have acknowledged in the paper – ultimately, we have tried to circumvent this (i.e. a no melting and a free slip case given by the dashed red line in figure 2) to disentangle the kinematic boundary condition effects and buoyancy flux effects.

6) I still find the motivation for the downward mass (do you mean salt?) flux at the ice base unclear. Am I right to think that your motivation is that your reference model, which you might want to coin "without interfacial salt flux", lacks a salinity flux at the ice base because you impose a zero salinity gradient, and that you aim to compensate it in your "with interfacial salt flux" model? If so, should we think that you turn the salt boundary condition from dz(S)=0 to dz(S) proportional to \dot{m}Sb with Sb the boundary salinity (how is this boundary salinity prescribed then?)? Do you do the same for temperature, i.e. imposing a heat flux proportional to \dot{m}Tb? Either way, please clarify all related sections, which are very important and I believe particularly confusing because you have to deal with Ansys Fluent's lack of flexibility with boundary conditions.

The motivation of the downward mass flux of cool, fresh meltwater, at a rate specified by equation 4, is to simulate the effects of an added buoyancy flux on the persistence and structure of an intrusion. The simulations that serve as the 'reference state' don't include any melting dynamics and therefore don't require a salt flux at the ice boundary. We have modified line 89 to clarify our intention with the melt-enabled simulations:

"We also simulated melting induced by seawater intrusion to investigate how this secondary source of buoyancy affects intrusion persistence and structure."

The boundaries in the 'reference state' have no-slip kinematic conditions with a thermal condition dependent on the local liquidus equation (re eq 1). The purpose of these reference simulations is to compare to previous theory (e.g. Wilson 2020 and Robel 2022) which do not consider melting effects.

We have added to line 84 to properly introduce these "reference simulations":

"The simulations in which we range over subglacial discharge velocities with medium turbulent mixing (e.g. C_μ = 0.09) are our reference simulations to which we will compare all other results. The purpose of these reference simulations is to compare with previous theory that consider quasi laminar flow and no melting effects. Further testing on potential intrusion control variables is also considered and explored in detail."

When melting is 'enabled' the ice boundary turns into a velocity inlet where fresh, cold water is input into the domain at the rate prescribed by equation 4 (heat conservation at the boundary). The boundary temperature is set by the liquidus condition (eq. 4). The boundary salinity, which is identified as the near-ice grid point's salinity, is solved for by its own evolution equation (eq B12) and transported appropriately.

7) I know this might sound subjective but I do not think that "high-fidelity" is an accurate description of the ice-ocean model considered in this work as there are clear limitations to the turbulence closure scheme being used and the representation of ice-ocean boundary conditions (To be precise I would only consider DNS and LES to be high fidelity, if they can justify the problem solved as a faithful representation of the expected dynamics). Thus I think that "high-fidelity" should be removed from the title of the paper. That is unless the authors make the point that the flows are laminar hence do not require any subgrid scale parameterizations and find a way to implement the exact boundary conditions at the ice-ocean interface (liquidus, dilution, cooling). In fact, since the Reynolds are so low, could you not solve the exact governing equations (i.e. without having to implement a closure scheme)?

We have modified the title based on suggestions from both reviewers:

"Modeling Mixing and Melting in Laminar Seawater Intrusions Under Grounded Ice."

Elsewhere in the text where we state ANSYS Fluent as a high-fidelity model, we have also removed the term.

We have tested each subglacial discharge scenario without a closure scheme (e.g. a laminar case) as shown by the green transects in Figure 2. The goal of this project is to test the effects of turbulent mixing on the ability for seawater to intrude into a buoyant subglacial discharge system, hence why we employed a turbulence closure.

8) The momentum equation (B3) in appendix doesn't have any buoyancy term. Could the authors make sure that buoyancy is considered in their model?

Thank you for pointing out this typo, it is included as a body force with the full variable density considered based on the equation of state used (eq. 2). We have amended equation B3 to:

$$\frac{\partial}{\partial t}(\rho u_i) + \frac{\partial}{\partial x_i}(\rho u_i u_j) = -\frac{\partial p}{\partial x_i} + \frac{\partial}{\partial x_j}\left[\mu\left(\frac{\partial u_i}{\partial x_j} + \frac{\partial u_j}{\partial x_i} - \frac{2}{3}\delta_{ij}\frac{u_l}{x_l}\right)\right] + \frac{\partial}{\partial x_i}\left(-\rho\overline{u_i' u_j'}\right) + \rho g_i$$

The buoyancy term is represented by ρg_i.

9) Some wording choices are very confusing and should be changed, such as “heat limited” or “melt activated”. Should you maybe say for the latter “with interfacial salt flux”?

This melt flux does more than dilute the near ice water, it also acts to chill it, so the suggested change in terminology would not be entirely accurate. The use of the term “melt activated” is to distinguish against the reference simulations which have no melting. We have modified this term to be “with melting” throughout the text.

We chose the term “heat limited” to demonstrate that the melting we simulate is only driven by thermal forcings and not dissolution. We have modified this term to be “thermally driven” to describe melt driven mainly by thermal forcings and removed discussion of diffusion-forced melting.

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Specific comments (chronological order)
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Abstract
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L9: I don’t understand “Basal melting from seawater intrusion produces buoyant

meltwater which may act as an important negative feedback by reducing near-ice thermohaline gradients.” What is the feedback mentioned?

L12: In particular, how it connects to “We conclude that, in times or places when subglacial discharge is slow, seawater intrusion can be an important mechanism of ocean-forced basal melting of marine ice sheets.

We understand this specific comment to be addressing a supposed inconsistency between stating intrusion-induced melting might create a negative feedback that suppresses further melting hinting at an intrusion’s potential unimportance in ocean-forced basal melting of marine ice sheets. Our intentions with these sentences are to point out that intrusion can cause melting, which given a flat geometric configuration can lead to stable stratification and blanketing of the ice by a layer of cold fresh water. However, that is unlikely in the real world. In addition, there are other drivers and modulators of intrusion, that are likely to impact an intrusion’s ability to cause melting. We have modified these sentences (text in red) to communicate our intention better:

L9: “Basal melting from seawater intrusion produces buoyant meltwater which may create a negative feedback by chilling and freshening near-ice water therefore reducing further melting, however this remains unquantified.”

L12: “We conclude that, in times or places when subglacial discharge is slow, seawater intrusion can be an important mechanism of ocean-forced basal melting of marine ice sheets when considering added geometric complexities and ocean conditions.”

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Introduction

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L39: Could you clarify “may lead to a run-away positive feedback if melting outpaces ice advection (Bradley and Hewitt, 2024).”?

In this paper, the authors found that if melting of ice happens ‘faster’ than ice is advected in to replace the melted ice then the subglacial conduit can grow unabated increasing the intrusion distance, which would lead to more melting, more subglacial conduit growth more intrusion etc.

We have modified this line to be:

“More recent work suggests that shear-driven melting beneath grounded ice can enlarge the subglacial cavity, enhancing seawater intrusion and potentially triggering a runaway positive feedback (Bradley and Hewitt, 2024). This feedback arises when melting exceeds ice advection, preventing upstream ice from replenishing the ablated region and allowing the conduit to grow unchecked. Bradley and Hewitt (2024) identified a regime in

which seawater intrusions could become unbounded, effectively “intruding infinitely.”

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Methods

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L80-81: Could you indicate the thermal driving associated with the initial open ocean and subglacial water conditions?

The ocean inlet temperature is T_o = 0.5 deg C.

The subglacial discharge temperature is set by equation 1 at z = 1000m (T_f = -0.6778 deg C).

Therefore the thermal forcing is T_{tf} = 0.5 – (–0.6778) = 1.1778 deg C.

We have added this to line 92:

"The thermal driving associated with the initial conditions is approximately 1.2 deg C and the reduced gravity is 0.23 m/s^2."

L112: I don't understand "Both ice faces have zero salinity and no salt diffusion across the boundary. For the warmer fluid regime prescribed in these experiments, a non-diffusive boundary is appropriate since thermally-driven ice loss will dominate."

We have identified the first line is this comment to be the main source of confusion for the boundary salinity issue. The intention of this line was to say the ice salinity is zero and we don't account for any ice mass loss by salt diffusion.

We have made edits to line 123 to clarify this confusion:

"The boundary salinity, S_b, is the salinity of the cell filled with water nearest to the ice face and is permitted to evolve during the simulation via the evolution equation B12. For the reference simulations, the boundary salinity evolves in time and space due to advection and diffusion of the subglacial discharge and seawater intrusion. In the simulations with melting, an additional source of freshwater is injected into the near-boundary grid cells, actively freshening the boundary salinity."

And added to the appendix at line 621:

"The boundary salinity, S_b, is set to the near-ice grid cell (scale of nm's) which has an evolution equation given by B12. This evolution equation accounts for advective and diffusive transport mechanisms. The boundary salinity is used in equation 1 (the liquidus condition) to set the thermal boundary condition for the ice interfaces. In the reference simulations, $S_b$only evolves due to advection and diffusion from the salt wedge entering the subglacial domain and the freshwater layer exiting the subglacial space as a plume. For the simulations with melting, S_b, is diluted as fresh, cold water enters the domain normal to the ice boundary. This dilution occurs because Fluent is a finite volume solver and so balances inflows and outflows across each finite element in the grid, while conserving overall mass balance in the domain via a pressure outlet (red arrow Figure 1). The thermal boundary condition (given by equation 1) adjusts based on this dilution. Similarly, the chilling of near-ice waters occurs due to local injection of meltwater at the freezing point."

For the second sentence identified here, if the fluid is supercooled or lacks sufficient thermal forcing, melting can still occur through dissolution. In this process, salt from the surrounding fluid enters the ice interface, lowering the local freezing point. This effectively destabilizes the ice structure and leads to its gradual erosion, even in the absence of significant heat input.

We have removed this from the edited manuscript to ensure simplicity and inhibit further confusion.

L138: Isn't "salt diffusion" out of place here? I don't understand "As salt diffuses in the medium, it also transfers heat due to its unique thermal properties, and therefore must also be included." This sounds like a generic sentence not relevant to the chosen configuration. The energy equation should only include time variations, advection and conduction. Also, viscous heating is a highly unusual term for environmental flows.

We have modified this line to:

"Since salt is tracked as an active tracer it transports enthalpy associated with its specific heat and concentration gradients. This must be accounted for in the energy equation Fluent solves."

The viscous heating term is not accounted for in our model setup (i.e. Fluent does not solve for it), however we included it in the appendix since that is how the equation appears in the solver documentation. We have removed this term ($\tau_{eff}\vec{u}$) from equation B11 and the associated text describing the term in the lines shortly after.

L152: This sentence "While the fluid density is different, the salinity and temperature of the fluid remain unchanged since they have their respective transport equations." Is somewhat misleading. My understanding is that you're saying "changing the equation of state" doesn't change the dynamics, which is possible, but likely model dependent. To see a change you would need to have processes (resolved or parameterized) that depend on density, or stratification. One key question then is: do mixing rates (or turbulent diffusivity) depend on stratification in your model? Your description of the turbulent closure scheme used suggests that there is not stratification effects considered, hence the lack of impact of the EOS on the dynamics.

The kinetic energy evolution equation does account for buoyancy effects via G_b which impacts the eddy viscosity via the turbulence closure which feeds back into the momentum equations. In this regime with this model setup, using a higher order equation of state did not noticeably change the dynamics. We have modified the text as shown in major comment 1.

L180: I find your discussion of basal melting very confusing, i.e. for instance, "In some simulations, we also simulate the added buoyancy flux resulting from the heat-limited melting scenario. Here, we neglect melting driven by dissolution, instead focusing on melting driven by thermal equilibrium at the ice boundary. Since the thermohaline conditions of the fluid domain are non-sub-freezing, the neglect of dissolution-induced melting is justified." I find the distinction between melting and dissolution confusing and unjustified for environmental flows such as here (because they are turbulent). My understanding is that fast ice losses due to high temperature flows and slow salt diffusivity resulting in fresh meltwater layers are coined ice melting, while slow ice losses, allowing for salt diffusion into the meltwater, correspond to ice dissolving. Here the ice is probably always melting, i.e. ice losses are controlled by heat fluxes. You seem to say so but what do you mean by "Heat limited"?

The purpose of these sentences is to provide a justification for why we are not modeling dissolution driven melting. In dissolution, salt from the surrounding fluid enters the ice interface, lowering the local freezing point, destabilizing the ice structure, effectively 'eroding' the ice. To the reviewers last point, in the thermal regime set by the seawater, ice loss is dominated by heat fluxes, which is our intention with the phrase 'heat-limited'.

We have edited the manuscript by removing these sentences for simplicity and to inhibit further confusion.

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Results
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Fig. 2: I am surprised that the laminar simulation isn't closer to the RANS model for the low subglacial discharge velocity. Could you comment?

The lowest subglacial discharge case shows the highest sensitivity to turbulence modeling choices, as seen in the contrast between the high, medium, and low turbulent mixing cases as well as the laminar simulation. At this weak forcing level, the flow itself generates little inherent turbulence, so the behavior of the intrusion is governed largely by how turbulence is modeled. In the RANS simulations, turbulence is generated primarily by boundary shear (ice and rock walls) and interfacial shear between the buoyant subglacial discharge and the underlying seawater. This mixing promotes entrainment, allowing the intrusion to extend farther into the domain. The resulting tail of relatively fresh water is visible in Figure 2 over the last 5 meters of the intrusion. In contrast, the laminar case cannot capture this turbulence or entrainment, and as a result, the intrusion stalls sooner.

We have added to line 240:

"For the slowest freshwater case, the intrusion is reduced in the absence of turbulent mixing due to the lack of entrainment between the seawater intrusion and the buoyant subglacial discharge. When modeled, this entrainment extends the intrusion by generating a tail of relatively low-density water."

L255: I am confused. I don't see how having a well-mixed column for low subglacial discharge (presumably low shear) is analogous to tidally forced estuaries subject to high shear? Isn't it that for low velocity the freshwater discharge is unable to fill or "occupy" the subglacial conduit, which becomes largely filled with open ocean water? Or is it really linked to turbulence (is there a diagnostic you can show to support this?)?

The decrease in stratification as the subglacial discharge is decreased is due to the convergence of the advective and diffusive timescales and not the imposed turbulence by boundary shear (as shown in Figure R1).

In line with our response to major comment 3, we have modified lines 269-272 to state:

"For the lowest freshwater flux, the diffusive and advective timescales are of the same order which leads to homogeneity in the vertical distribution of heat and salt (Figure 3I-L). In estuaries, strong tides generate shear at the bed increasing turbulence and vertical mixing (Montanga et al. 2013). Tides may play a similar role in subglacial seawater intrusion regimes, however, that is not simulated here."

Fig. 4 and related text: isn't the high Cd simply an indication that the flow is laminar? Could you comment?

The drag coefficient found in these simulations matches what would be expected given the subglacial discharge and geometry of the subglacial space per engineering literature. For laminar flow the drag coefficient can be estimated by $C_d = 16/Re$ where Re is the Reynolds number. For the middle subglacial discharge case tested here (blue lines in Figure 4) the Reynolds number is ~250. This would give C_d=0.064. The simulated drag coefficient (~0.05) at locations away from the intruded seawater (at flat part of the curves shown in figure 4) agrees well with this estimate (=0.064).

The interesting part of Figure 4 is the transition to a higher drag coefficient over the length of intrusion (everything downstream of the scatter points in Figure 4) which likely is resulting from transition to a turbulent regime driven by shear at the interface of the intrusion wedge and buoyant outflow. This value of drag coefficient has less straightforward ways of estimating due to the complexities of flow within the intrusion regime.

We have edited line 299 to help clarify this:

"The high drag coefficients simulated here upstream of the intrusion regime are in line with the expected values for laminar flows with these Reynolds numbers. However, over the regime of intrusion the drag coefficient increases by nearly an order of magnitude in all cases tested. The increased drag coefficients over the intrusion are more difficult to estimate and likely result from enhanced turbulence in the interfacial shear layer between the intrusion and the buoyant subglacial outflow.

L323: It's a bit odd to mention that some ocean models use temperature 10 km away from the boundary to estimate melt rates. Isn't few meters to few tens of meters?

Grid cells of this size may be used if one is not directly modeling ice shelf cavities and therefore use ocean forcings from 10's km from the grounding line/underneath ice shelves to estimate a melt rate. In simulations of ice shelf cavities the vertical grid cell size can be on the order of 100 meters and the horizontal grid cell size might be order 10 km. In this case, there arises the question of what to do in the limit your vertical grid cell size approaches 0, which is essentially what happens at the grounding zone, which is the region of interest in this study.

L343: Taking h has "the thickness of the viscous sublayer (Holland and Jenkins, 1999)." is somewhat surprising to me (I would assume somewhere in the log layer) and might be worth double checking.

Thank you for pointing this out, this error has been fixed and is in line with the phrasing used in Holland and Jenkins 1999.

Edited this line to be:

"[…] the thickness of the boundary layer (Holland and Jenkins, 1999)."

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Appendices
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I assume that equation (B3) is incorrect, and the buoyancy/gravity term is missing? Please double check.

Thank you for pointing this out, we have added the buoyancy term ρg_i to the right-hand side of the equation.

Why do you use different notations between equations (B3) and (B4)?

We have modified equation B4 to have lowercase velocities similar to B3 with an overbar to denote average quantities:

Equation (B8) seems to have a typo.

We have fixed the parenthesis exponent error in equation B8.

Could you confirm that species diffusion is set to 0 in (B10) and does not impact the heat equation?

The species diffusion term is not zero, however it does not impact the overall heat equation significantly since its magnitude is orders smaller than other terms.

The energy transport by species diffusion is set by $h_{salt} J_{salt}$ where h_{salt} is the enthalpy of salt and J_{salt} is the diffusive flux. The enthalpy can be found by:

$$h_{salt} = \int_{T_{ref}}^{T} c_{p,salt} dT = c_{p,salt}\left(T - T_{ref}\right)$$

Here, the heat capacity of salt ($c_{p,salt}$) is set to ~ 10^3 J/kg K and the reference temperature (T_{ref}) is a default value used by the solver set to 298.15 K. Given the temperature of our simulation is approximately 273 K, the enthalpy of salt is about $2.5\ x\ 10^4$ J/kg. The diffusive flux can be solved for by:

$$J_{salt} = \rho_W D_s \nabla S$$

The fluid density (ρ_W) is approximately 10^3 kg/m^3, diffusivity of salt is approximately 10^{-9} m^2/s, and the salt gradient (∇S) can be approximated for the middle subglacial discharge flux case to be 0.034 / 5 m. This would result in the energy transport by species diffusion to be about 10^{-4} W/m^2.

Comparably, the other term in equation B11 that accounts for energy transport by thermal diffusion ($k_{eff} \nabla T$) can be estimated given an effective thermal diffusivity ~ $6\ x\ 10^{-1}$ W/mK (includes both molecular diffusivity and turbulent diffusivity) and thermal gradient (∇T) of ~ 1 deg C / 5 m. This results in energy transport by thermal diffusion of order 10^{-1} W/m^2.

Therefore, even though species diffusion is contributing to overall energy transport in the system, it is much smaller than other contributions and does not impact the results significantly.

We have added to line 612:

“As salt diffuses in the medium, it also transfers heat due to its unique thermal properties and therefore must also be included. The contribution to heat transport from salt has negligible impacts since it is not a leading order term in equation B10 and therefore tracking species contribution to energy transport does not significantly change the results.

L686: Could you discuss to what extent stratification effects are considered in your turbulence closure scheme (to support your explanation “Turbulent viscosity is greatly reduced over the length of the intrusion, likely due to enhanced stratification suppressing mixing.”)?

There is a buoyancy term G_b in the turbulent kinetic energy evolution equation which allows for stratification effects to impact turbulent viscosity (aka eddy viscosity). We have added a few sentences to the appendix to describe this impact:

“Buoyancy effects are included in the evolution equation for turbulent kinetic energy by a source term, G_b, which can be solved for by:

$$G_b = g_i \frac{\mu_T}{\rho Pr_T} \frac{\partial \rho}{\partial x_i}$$

Where g_i is the component of gravity in the i^{th} direction, μ_T is the turbulent viscosity, and Pr_T is turbulent Prandtl number (0.85 default value). The density gradient $\frac{\partial \rho}{\partial x_i}$ is taken over the i^{th} direction. It is not included in the dissipation equation due to a higher degree of uncertainty (ANSYS 2022).”

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Second Response to Reviewer 2 for Manuscript "High-Fidelity Modeling of Turbulent Mixing and Basal Melting in Seawater Intrusion Under Grounded Ice"

by Mamer, Robel, Lai, Wilson, and Washam

We appreciate the reviewer's constructive comments and aim to address their concerns below. Reviewer's responses original text is written in black, and our responses are written in red.

Review #2 Mamer et al "High-Fidelity Modeling of Turbulent Mixing and Basal Melting in Seawater Intrusion Under Grounded Ice"

My sincere apologies to the Authors and Editor for taking so long to complete this review.

I appreciate the significant changes that the authors have made, including adding new simulations, in response to the first round of reviews.

Unfortunately, the new information provided suggests to me that the problem formulation and the model are not fit to address the stated aims of the paper, for the following reasons:

A – I don't believe that the RANS model can simulate this particular flow
Firstly, it appears that the simulations are essentially laminar within the intrusion region, based on the similarity between the so-called "turbulent" and "laminar" simulations and Figure D1 which shows that the eddy viscosity is similar to molecular viscosity within the intrusion region (As an aside - I find the non-monotonic relationship between C_mu and turbulent viscosity disturbing!).

It is not entirely clear why the reviewer does not think that the Reynolds-Averaged Navier Stokes Equation are not able to simulate laminar regimes. These equations still apply even in the laminar regime (perhaps especially here, since the closure terms become less important). Mathematically, the Reynolds stress terms appearing in the momentum equations will vanish in the limit of laminar flows. One primary aim of this study is to compare these more complex simulations to prior theory (Wilson et. al 2020 and Robel et. al 2022) in which all cases considered are subcritical ($Fr<1$), thus limiting the subglacial discharge velocity to essentially laminar conditions. Based on the comments from both reviewers, we have modified the title to be:

"Modeling Mixing and Melting in Laminar Seawater Intrusions Under Grounded Ice."

and amended line 5 in the abstract:

"In this study, we investigate turbulent mixing of quasi laminar intruded seawater and glacial meltwater under grounded ice using a computational fluid dynamics solver."

Furthermore, at line 81 we have added:

"In the experimental set up in this study, we only consider quasi laminar flow in order to facilitate comparison to previous studies (Wilson et. al 2020, Robel et. al 2022) which find that subcriticality ($Fr < 1$) is required for intrusion development."

The non-monotonic behavior between C_μ and the turbulent viscosity is consistent with our expectations for non-linear coupling of turbulence models. Turbulent viscosity is linear with respect to C_μ (e.g. eq 3) given constant turbulent kinetic energy, dissipation, and density. However, this is not the case, since the fields of kinetic energy and dissipation adapt to the closure and then feed back into the turbulence model. Hence the non-monotonic behavior. More simply, turbulence viscosity exists within the turbulent kinetic energy and dissipation evolution equations (eqs. B5 and B6), so they are coupled, which results in non-monotonicity when changing the co-factor in the turbulence closure model.

Unfortunately, based on your simulations alone, you cannot say whether this laminar state is a real feature of this (unusual) system or not. The extreme stratification and small height (and therefore Reynolds number) of the subglacial environment means that turbulence may not develop. However, instability or scouring of the interface, as was seen under certain conditions in the laboratory experiments of Wilson et al 2020, is certainly possible and would influence the flow significantly. However, simulating such features in a stratified flow is not within the capabilities of a RANS model, and is instead the domain of Direct Numerical Simulations (DNS).

We recognize that shear instabilities at the interface of the two layers (Holmboe or Helmholtz instabilities as examples) are possibilities (given the right advective forcings) that would act to increase mixing and are not directly resolved here. However, a recent DNS study investigating stratified flow found that non-laminar interfacial shear layers would occur at higher Reynolds numbers than simulated here (Zhu et. al 2023). Regardless, RANS should be capturing the averaged effect of what these instabilities would lead to in the flow field.

We have added to line 64:

"Interfacial shear instabilities that might be expected of highly stratified flows such as the ones simulated in this study are not explicitly resolved in RANS models, however the averaged mixing effect of such instabilities should be captured. Furthermore, a recent study using direct numerical simulations has identified that such instabilities do not arise at the lower Reynolds numbers we are simulating here (Zhu et. al 2023)."

Since the current literature on seawater intrusion are highly idealized one-dimensional models of two-layered shallow water equations (e.g. no mixing dynamics or vertical resolution), we believe the simulations presented here are helpful in identifying key next questions about the dynamics of this regime. We agree that DNS may be a logical next step, but many robust conclusions are still possible in this modeling configuration.

B – The melt model is inaccurate

I appreciate that, in response to the previous reviews, the authors added the salinity-dependent interface temperature. However, in neglecting to solve the salt conservation equation at the ice-ocean interface, you have two equations (Equations (1) and (4)) and three unknowns (m, Tb, Sb). This is a fundamental flaw in the approach and means that the melt rates will not be accurate.

The boundary salinity is accounted for in the model and permitted to evolve as the model runs. The boundary salinity, S_b, is taken to be the near-ice salinity (scale of nm's) and has its own evolution equation (eq. B11 in original manuscript). The values of S_b can be clearly seen in Figure 3 (panels B, F, J) where y = 0. Clearly in this figure, S_b is permitted to evolve to non-zero values. The melt rate is set by the conservation of heat at the boundary and given by equation 4. The liquidus equation that sets the thermal boundary condition (and dependent on S_b) is given by equation 1. When melting is turned on and an additional source of fresh, cold water enters the domain normal to the ice boundary, dilution of S_b occurs because Fluent is a finite volume solver and must balance all inflowing and outflowing fluxes for each given grid point. The thermal boundary condition of the ice responds accordingly to this dilution based on equation 1.

We have identified the main source of boundary salinity confusion to be from line 112 in the original manuscript where we state:

"Both ice faces have zero salinity and no salt diffusion across the boundary."

Here, we had intended to say that the ice salinity is zero but recognize the location of this sentence and lack of clarity is misleading.

We have made edits to line 123 to clarify this confusion:

"The boundary salinity, S_b, is the salinity of the cell filled with water nearest to the ice face and is permitted to evolve during the simulation via the evolution equation B12. For the reference simulations, the boundary salinity evolves in time and space due to advection and diffusion of the subglacial discharge and seawater intrusion. In the simulations with melting, an additional source of freshwater is injected into the near-boundary grid cells, actively freshening the boundary salinity."

And added to the appendix at line 621:

"The boundary salinity, S_b, is set to the near-ice grid cell (scale of nm's) which follows the evolution equation given by B12. This evolution equation accounts for advective and diffusive transport mechanisms. The boundary salinity is used in the liquidus condition (equation 1) to set the thermal boundary condition for the ice interfaces. In the reference simulations, S_b only evolves due to advection and diffusion from the salt wedge entering the subglacial domain and the freshwater layer exiting the subglacial space as a plume. For the simulations with melting, S_b, is diluted as fresh, cold water enters the domain normal to the ice boundary. This dilution occurs because Fluent is a finite volume solver and so balances inflows and outflows across each control volume in the grid, while conserving overall mass balance in the domain via a pressure outlet (red arrow Figure 1). The thermal boundary condition (given by equation 1) adjusts based on this dilution. Similarly, the chilling of near-ice waters occurs due to local injection of meltwater at the freezing point."

We recognize there are limitations in how we are modeling melting as described in the discussion. Based on earlier reviews, we attempted to overcome some limitations (e.g. providing a free-slip kinematic boundary condition case to compare the melting cases to). The simulations with melting are merely exploratory cases to look at what would happen to stratification and transport within the intrusion regime given freshening and cooling from local melting.

There appears to be significant confusion from the authors about the role of salt in melting, e.g. Line 182:
"Since the thermohaline conditions of the fluid domain are non-sub-freezing, the neglect of dissolution-induced melting is justified."
The only case in which the salt balance equation can be neglected is if the interface salinity Sb is zero. This only occurs in freshwater, or theoretically if the ice is melting so quickly that that salt transport towards the ice cannot "keep up" with the meltwater flux which would lead the region closest to the ice to become fresh (see figure 1b in Malyarenko et al 2020). So, there is a clear inconsistency between including the interface salinity in your freezing point temperature, but not also including a salt balance equation in the melting calculation.

As noted above, we are not neglecting the salt balance. The intention with line 182 was to justify why we are not modeling melting driven by dissolution. Dissolution occurs when thermal forcing is near zero and so salt can diffuse into the ice crystal, suppressing the local freezing point, effectively "eroding it". Due to comments by both reviewers, we have removed this line from the main text.

C – Salt diffusion does not appear to be correctly represented

Molecular diffusion of salt is of the utmost importance to ice-ocean interactions since this is the way salt is transported across the viscous sublayer adjacent to the ice-ocean interface. In addition, in the absence of turbulence, it is the only mixing process occurring and will govern the evolution of salinity stratification along the salt wedge. Although it is not spelled out in the salinity equation (B11), based on the profiles in Fig. 3 which show that salinity gradients are no steeper than temperature gradients, it appears the salt diffusion is not occurring at the expected slow molecular "rate" based on molecular diffusivity of ~7e-9 m^2/s.

In equation B11 (now B12 in the revised manuscript), the diffusion flux is represented by $-\nabla\vec{J} = -\rho_w D_S \nabla S_i$, where D_S is the diffusion coefficient for salt, here set to 1.5e-9 m^2/s. We have updated equation B11 to include the full diffusive term and edited line 620 :

"Where u is the velocity vector, D_S is the diffusion coefficient for salt (set to $1.5x10^{-9}$ m^2/s), and R is the production rate from reactions."

Salt is diffusing at a lower rate than temperature throughout the domain. Here we provide two figures from the lowest subglacial discharge scenario (u_f = 0.05 cm/s) with no turbulence closure (i.e. laminar) to demonstrate this. Figure 1 shows a vertical slice of normalized temperature and salinity within the intrusion regime. Note in the upper portion of the intrusion (y > 2.5 cm) heat and salt diffuse at different rates into the freshwater layer that occupies this upper layer of the subglacial space. Figure 2 showcases depth average horizontal transects of normalized temperature and salinity along the intrusion. Once again, the profiles are not

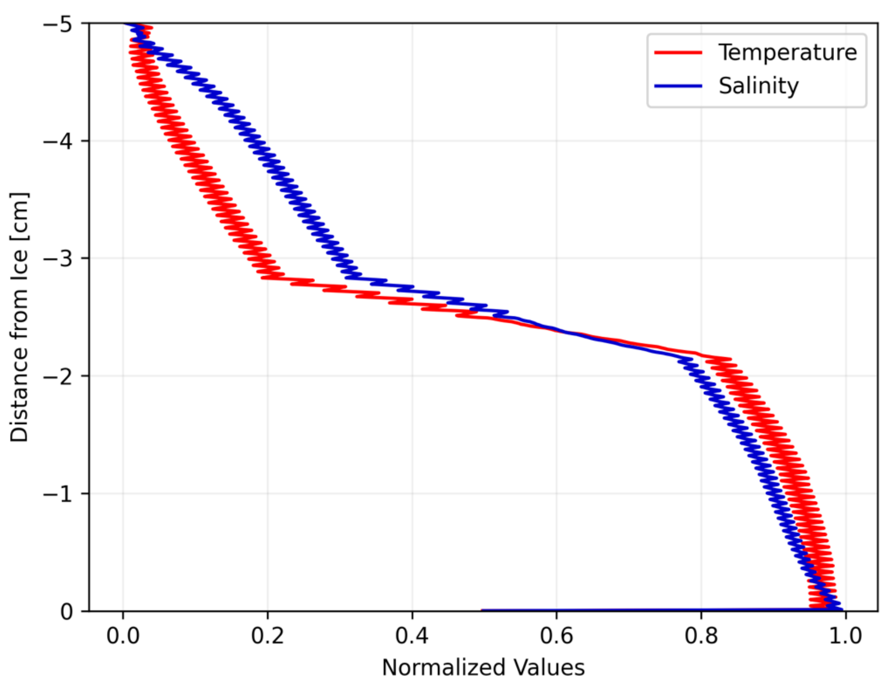

Figure 1. Vertical profiles of time-averaged temperature and salinity for the lowest subglacial discharge velocity with no turbulent mixing. Temperature and salinity are normalized via equation 1.

identical, meaning they are diffusing at their own respective rates. Furthermore, the temperature profile is broader and decays to the background value (subglacial value) quicker than salinity does, i.e. it diffuses quicker.

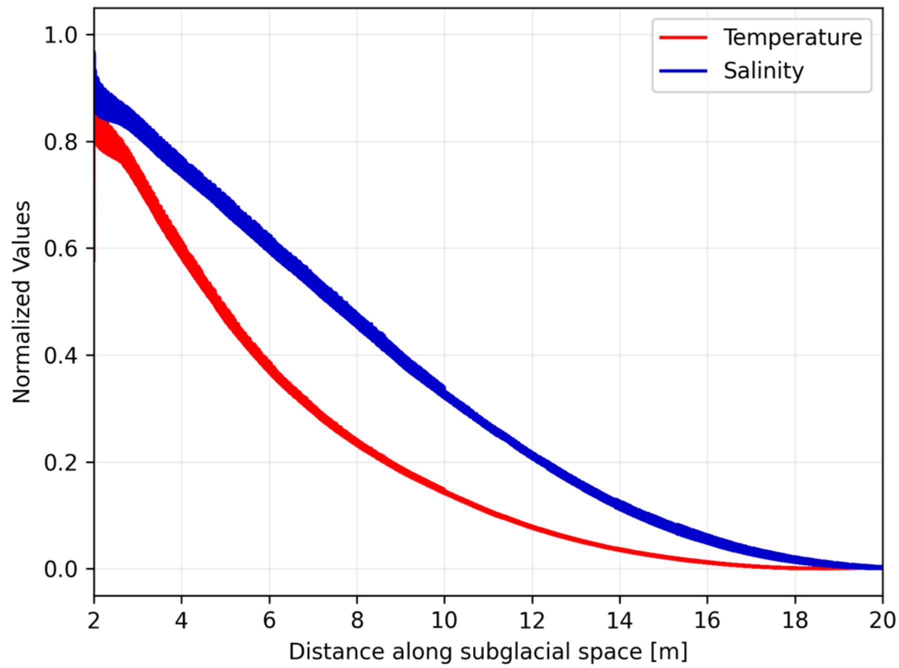

Figure 2. Depth-averaged transects of temperature and salinity through the subglacial space. Temperature and salinity are from the time-averaged domain and normalized by equation 1. The grounding point is at x = 2 m.

We normalize temperature and salinity by:

$$\Phi_{norm} = \frac{\Phi - \Phi_{\min}}{\Phi_{max} - \Phi_{min}} \qquad eq. 1$$

where Φ is a placeholder for the respective variable being normalized.

Relating these to your specific aims:

"(1) to test previously proposed controls on seawater intrusion distance,
◊ this might be ok, although is questionable given (C)

Figure 1 and Figure 2 demonstrate that salt is diffusing at a slower rate than temperature, and so we are resolving this transport mechanism correctly. We have updated the salt evolution equation to articulate the diffusive flux more thoroughly.

(2) to determine the effects of turbulent mixing on seawater intrusion, and
◊ based on (A) and (C), I don't think you can reasonably do this in your setup

The averaged effects of possible interfacial shear instabilities are captured by RANS models, and existing literature supports that such instabilities do not arise in the Reynolds regime we are testing here (e.g. Zhu et al. 2023). The current state of the field on seawater intrusion are simplified one-dimensional two-layer models and this paper serves to expand on this by providing insight into the vertical structure, stability, and controls on intrusion given the presence of turbulence induced mixing.

(3) to investigate the dynamics of intrusion-induced basal melting
◊ based on (B), this aim has not been addressed.

The boundary salinity is set to the near-ice wall salinity which is permitted to evolve in the model by its own evolution equation (eq. B11 in original manuscript). The dilution of this value happens when cold, freshwater inflows at a rate set by the melt rate (eq. 4). The intentions of the simulations with melting were to explore feedbacks between added buoyancy forcings and persistence of intrusion, not necessarily to produce a constrained value for expected melt rate in the tested regimes of intrusion.

I see two ways forward for this study. One approach would be to address (B) and (C), and focus on the laminar problem. A second approach would be to switch tools and use DNS.

Based on our responses to your three points, we believe this modeling framework is adequate to answer the questions laid out in the beginning of the paper and will provide useful information to those studying seawater intrusion and grounding zone fluid dynamics.

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