Response to Reviewer #1 for Manuscript "Layered seawater intrusion and melt under grounded ice" by Robel, Wilson, and Seroussi

Robel et al. present a simplified model of a subglacial salt wedge and apply a simple parameterization of associated subglacial melting in ice sheet models to estimate the effects of this process on sea-level projections. They offer a valuable extension of the work recently presented by Wilson et al. (2020) from intrusion in subglacial channels to intrusion in a subglacial water film. Overall, I found the manuscript clearly written, with some additional suggestions for improving clarity provided below. The primary concern I'd like to see the authors address, elaborated below, pertains to the feasibility of the simulated melt rates.

Our gratitude to this reviewer for their confidence in our concept and their thoughtful comments. We have addressed them each below.

Major comments:

When you discuss the conditions for maintaining stratification between layers, showing that the Reynolds number indicates a laminar regime would be helpful. (Roughly around L240)

Added a parenthetical note indicating that the Reynolds number in such a case would be approx 50, which is well below the laminar-turbulent transition.

A table of some of the cases you discuss in Sections 3.1 and 3.2, i.e., the parameter combinations and distance estimates, would be a useful reference.

We have added such a table (Table 1 in new version)

It seems that you consider the bed slope but not the ice slope in Equations 3,4. However, I think you also make the assumption that ice base slope is the same as bed slope such that the subglacial film thickness is constant. I think that's fine since you treat the velocity of the upper layer as a free parameter, but I'd like to see Equations 3,4 presented in as general a form as possible.

Slope in the ice base that is different in slope of the bed would produce variations in total layer thickness (H) that would be captured through the barotropic pressure gradient already in equations 3 and 4. Ultimately, such changes affect both layers equally and so do not have an impact on the intrusion distance (as we explain with respect to the barotropic pressure gradient). We have added some discussion to this effect specifically referencing the possibility that the ice base and bed slopes are different.

Can you provide an argument that such intrusion-induced melt rates can exist in a steady state? Given such thin films, I would think it's possible that heat exchange (in this laminar regime) from the open ocean upstream through the subglacial layer might be too slow to maintain these melt rates.

This is a nice suggestion. We have done this calculation and added a new paragraph to this effect at the beginning of section 4. Generally, we find that for melt rates of order 10's of m/yr over intrusion

distances of hundreds to thousand of meters (and the range of layer thicknesses explored in section 3), an ocean current of cm/s would be needed to maintain a steady-state in the two-layer model. This is within the range of ocean current speeds measured near the grounding lines of ice streams in West Antarctica, therefore we find this to be plausible. However, this does raise a good point, that such steady-state considerations do place a limit on the melt rates and intrusion distances that can be maintained in a steady-state with seawater intrusion. We have discussed all of these issues in the new paragraph. We have also added a new suite of MISMIP+ simulations with lower baseline basal melt rates in response to a suggestion of reviewer #2 which should also help address this comment.

L540: This line reads like a recommendation. However, with such a wide range of intrusion distances and the strong sensitivity to the bed type, this recommendation seems too general to be useful to ice sheet modelers. Can you settle instead on something like, "uncertainty in intrusion-induced melt should be incorporated into uncertainty of sea-level rise projections using ice sheet models" or "sensitivity of sea-level rise projections to intrusion-induced melt should be tested in ice sheet models." Or something even more specific like "given large uncertainties, we recommend applying melt to partially-grounded cells"
Good point. We have changed this to: "Additionally, the sensitivity of sea level projections to uncertainties in intrusion-induced melt should be tested in a deliberate fashion (rather than as a numerical artifact)."

Minor comments:
L7. Since you haven't yet discussed what you mean by "hard bed" I recommend instead calling it an impermeable bed.
Fixed

L11. "10-50% higher or 100% higher" This is a strange way of expressing it without indicating what makes the difference between the two cases.
Split into two sentences and clarified

L12: "whether the conditions are met for extensive seawater intrusion" or "whether extensive seawater intrusion occurs"
Fixed

L50: "distance" >> "extent"
Fixed

L89: "the bulk porosity of the sheet" Wouldn't it be better to define ϕ_1 and ϕ_2 for each layer in Equa-

tions 3,4 so that the equations are more general? And similarly for c_d to acknowledge that the ice and bed could have different roughnesses?
We have changed these to different porosities, drag coefficients and obstacle diameters in equations 3 and 4, but then explicitly stated what assumptions we are making about drag coefficients and differences between layers after that. This should make equations 3 and 4 very general for those who might want to start from those later for more complicated scenarios.

L91: This is the final term in Equation 3 but not Equation 4
Fixed

L106: "vertical interface" To me, this is unclear and would be better stated just as the horizontal extent of the saline layer.
Fixed

L107: "without considering their compositional differences" Are you referring to the inclusion of the buoyancy (reduced gravity) term? Or the representation as a two-layer system?
Both, added a parenthetical clarification

L113: "after H_2 is eliminated" This would make more sense if $h = H_1/H$ had already been introduced.
Moved the non-dimensionalization up before this, and explained the process of reaching this final equation in more detail.

L116: I think it would be helpful to describe what γ represents qualitatively (i.e., porosity).
Description added

L118: "$Fr = Fr_0 h^{-2/3}$" should be explained
Explanation added

Figure 1. I think it would be helpful to include a second panel that is the schematic of the soft bed case.
Second panel added with soft bed case indicated by hatching.

Figure 2. Define h in the caption.
Added definition (also fixed y label)

L191: "single hydraulic potential" implies that Equation 12 will contain the hydraulic potential rather

than U_{in}.
Removed this phrase to clarify the statement further.

L195: Can you direct readers to where in the literature α is defined?
Added

L222: "which" is ambiguous between g' and the density difference.
Fixed

L246: I think you mean "it is still possible to maintain two layers in water sheets 10cm thick"?
Yes, fixed

L250: Provide a reference for maximum packing density.
Realized in the process of adding this that we want the maximum density in 3D (which is equivalent to porosity), not 2D. This lower the maximum expected gamma to 2. Added a reference to the "Kepler conjecture" which establishes the maximum packing density of spheres in 3D.

L252: "That could be supported in such a thin water sheet" by virtue of what? Do you mean supporting two layers within the film? How did you determine the c_d value of 0.01? If it is just the upper bound in the literature, then it would be clearer to delete the clause I reference here.
Clause deleted and clarified

L265-272: This paragraph seems to be a distraction. As far as I can tell, the key point is that the layer in the ice sheet interior is on the order of mm and thus too thin for the 2-layer model to apply. That could be stated in the previous paragraph with the Engelhardt and Kamb (1997) reference.
Good point. We have removed the paragraph and added a sentence to the previous paragraph to this effect.

L294: delete "equation"
Fixed

Figure 3: It is hard to view inset box in panels a,c. It might be worth stating in the caption that the blue region is above the critical slope.
Added

Figure 3 and 4: I wonder if you might find a better colormap for these plots. It's pretty hard to see the

difference between values just above and below 10m. Another option would be a contour at 100m since that is the lowest value you implement in the ice sheet model.
Contour added indicating 100 m intrusion distance.

L341: Add units to K
Added

L346: "perhaps in under" >> "perhaps under"
Fixed

Figure 6: Add a legend to panel b for the two points located at x=0
Legend added

L478: The local bed type isn't easy to accomplish in a model if we still don't have a continent-wide map of hard/soft bed.
Modified this sentence to reflect this uncertainty in bed type

L491: "act to prevent strong curvature" This isn't clear offhand. Can you provide a reference?
This was perhaps more speculative. The sentence has been deleted for clarity.

L531: "rights" >> "right"
Fixed

L533: "under the right circumstances
repetitive with previous sentence
Deleted