Response to Reviewer #2 for Manuscript "Layered seawater intrusion and melt under grounded ice" by Robel, Wilson, and Seroussi

This study tackles a very critical concept in the world of ice sheet-ocean interactions which to date has not received a great deal of attention. Using simplified mathematical descriptions of the near-terminus subglacial environment, it builds on previous work to describe intrusions of warm salty water into the subglacial water layer under marine ice sheets, and establishes that there could potentially be warm water underlying subglacial environments kilometers from the grounding line. It then considers, through a simple parameterisation, how the presence of this water, if able to effect high melt rates compared that that observed in ice-shelf cavities, might impact marine ice sheet stability. In most respects the study is comprehensive, well thought out and well explained, and feel it deserves publication. I have two main comments for the authors and editor to consider, followed by a number of minor ones.
Thank you to this reviewer for their thorough and helpful suggestions, which are each addressed below.

Main Comments:
1) While the hard-bed treatment of the subglacial layer is very detailed and well explained, and one can clearly see how physical considerations lead to mathematical results, I feel the soft-bed case just presents results without intuition. I appreciate that soft-bed transport is probably not the mode by which subglacial transport occurs, given the volumes involved – but it would be nice to understand, if only qualitatively, where 12-14 come from without needing to read the Strack paper referenced.
We have added an explanation of the key assumptions that go into these equations and the general approach to the derivation (without a complete derivation, which is already done in other papers that we reference). In particular we discuss the Dupuit-Forcheimer and Ghyben-Herzberg approximations which form the basis for much of saltwater intrusion theory in hydrology, how they are combined at the outset, and eventually leading to the equations for intrusion distance through flat or sloped aquifers.

2) The parameterisation for under-ice melt in (17) and illustrated in Fig 5 (and was actually also used by Parizek et al 2013 – and in fact was required by that study to show instability of Thwaites) is acknowledged to be just a parameterisation with little oceanographic basis – but I still feel it is a bit of a cop-out to say that we don't have the understanding to rigorously model melt so we will just use an approach that is linear in space, as an exploratory tool. This method still leads to extremely high melt rates under grounded ice, which is known (from sources cited in the manuscript) to have a much larger impact of grounded ice than melt of similar magnitudes under ice shelves. I am certainly not an expert in boundary-layer oceanography but I know that the melt rates suggested by e.g. the equations of Holland and Jenkins 1999 under ice shelves exposed to CDW require quite developed boundary layers and high levels of turbulent mixing, and Im unsure if such mixing rates and layer thicknesses are allowed by the theory. The examples cited (Kimura and Begeman) invoke double diffusion – but I believe that in both of these works, the under-ice ocean conditions below the ice would lead to much higher melt rates than observed considering under-shelf plume type flow. I bring this up not because I understand

the physics of flow in these subglacial environments – but simply because the analogues cited to justify these high melt rates actually show very very low melt rates under such stratification envisioned in this study, and it is difficult to imagine what could make them higher. It would be quite a lot to ask the authors to come up with a better parameterisation, or to redo their experiments. But i feel, and if the editor agrees, that the use of such a parameterisation should be far more heavily caveated than it is, and in more places in the text than just its introduction.

The reviewer followed up with the following comment:
I am unable to amend my comment, so i hope that the authors and editor read this as well!

I realised that my General Comment #2 could be interpreted as saying, if under-ice melt rates were of similar magnitude to those seen in Kimura et al and Begeman et al they would be unimportant. This is my intention in any way. Under-ice sheet melting of a few m/a near the grounding line would be on the order of thinning rates in some of the fastest-thinning ice streams in Antarctica. However, this is a bit overshadowed by considering melt rates of 30-40 meters per year, which for reasons given in my original comment are difficult to imagine on physical grounds.
We respond to both the reviewer's original comment and the addendum (inserted above for clarity) here.

The point raised by the reviewer is an excellent one. Ultimately, we do not address the physics of heat fluxes in this manuscript since it would require a more sophisticated treatment of the ice-water boundary layer than what we are able to sketch out with a relatively idealized mathematical model. In order to get at the basal melt rates with some accuracy, ultimately a more sophisticated fluid dynamical model which simulates turbulent mixing and heat fluxes would be required. Such simulations are actually currently being undertaken by a student, but are well outside the scope of this manuscript. We have added additional caveats in section 4 and in the discussion to this end. We have also added a reference to Parizek, which does indeed explore the sensitivity of simulations to melt just upstream of the grounding line.

We do seriously take the point that the melt rates of 10's of m/yr considered in these ISSM simulations are higher than would be expected for double-diffusive convection. The reason why these melt rates were originally used is because they are in line with the standard MISMIP+ benchmarks which we are trying to reproduce here to provide a point of comparison for others who are attempting to discern the importance of intrusion-induced melt in a "standard" configuration. Additionally, such melt rates are also in line with those observed (and typically modeled) at Thwaites. So, we have retained these original simulations, but also added new ones to address this point. Given that we argue for the potential for intrusion-induced melt through double-diffusive convection, even in the absence of strong interfacial mixing, it is worth it to determine whether the results hold up for melt rates more typical of such a stratified sub-ice layer. Thus, we have added a suite of new MISMIP+ simulations (Figure 7) where the "baseline" case has basal melt rates at the grounding line an order of magnitude lower (i.e. a few m/yr) in line with melt rates observed where there is double-diffusive convection (e.g., Kimura et al. 2015). We find in these simulations that though the absolute ice volume loss is lower (as expected for lower melt rates in the floating ice), intrusion-induced basal melt over hundreds to thousands of meters upstream of the grounding line still increases volume loss by 10-55%. While these increases are lower than in the higher baseline melt scenarios, it is still the case that intrusion-induced melt can have first-order effect on the rate of ice volume loss.

We also point out (in response to the reviewer's comment) that basal melt rates of 10's of m/yr are within the range of melt rates that have been observed near the fastest-melting grounding lines, and that since ice flow generally compensates for most basal (and surface) melting, the corresponding net thinning rates would be much lower. Indeed, in our transient simulations with high basal melt rates, the thinning rates are generally in the range of 0.1-1 m/yr. Thus, even though the highest thinning rates in Antarctica are m/yr, basal melt rates may still be considerably higher than this.

Minor comments:
Line 92: this is not the final term in eq 4
Fixed

line 92 Rominger reference: it would still be nice to see intuition for the functional form of this term
Added further explanation of this new term

Derivation of (5), and expressoin for Fr: I think some clarity is needed. do you not require eqs 1 and 2 as well (or at least eq 1)? How is $H2$ cancelled, is it via $H2 = H - H1$? The nondimensionalisatoin seems to imply H is a constant, rather than a varying field.. if this is the case, i can't find where it is made clear.
This discussion has been reorganized a bit (also in response to suggestions by reviewer 1), which should clarify how this expression is reached (by combining equations 1-4 after non-dimensionalization). Have also added explanation that H is indeed a constant (though H_1 and H_2 are not).

line 138 hard$--$ >difficult
Fixed

line 146: what is W
Removed W and added words to describe instead

Figure 2: labels (a) through (d) not shown. what are the parameters in eq 5 for these solutions other

than the ones shown in the legend?
Labels added and other parameters indicated in caption.

paragraph at line 218: awkward. shouldn't the solutions to eq 5 be *exactly* this length scale, as you are implicitly defining the length scale through this equation?
Not necessarily - as the length scale is derived through approximation of the exact intrusion distance that is the solution to equation 5. It is easy to see how this paragraph is confusing on this point, however, and we have re-written it to clarify the point that we are making (just that there are only a few parameters determining intrusion distance).

line 246: *stratification within* water sheets?
Deleted "stratified" to clarify

line 274: what does "exponentiated" mean in this context? does L become an exponent somewhere?
Deleted to clarify and simplify this sentence

line 287 2nd "of till" redundant
Fixed

References

Holland, D. M., & Jenkins, A. (1999). Modeling thermodynamic iceâocean interactions at the base of an ice shelf. Journal of Physical Oceanography, 29(8), 1787-1800.

Parizek, B. R., et al. (2013), Dynamic (in)stability of Thwaites Glacier, West Antarctica, J. Geophys. Res. Earth Surf., 118, 638â 655, doi:10.1002/jgrf.20044.