Comment on tc-2021-262
Anonymous Referee #2

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.

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.

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 I'm 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.

Minor comments:

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

line 92 Rominger reference: it would still be nice to see intuition for the functional form of this 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.

line 138 hard-->difficult

line 146: what is W

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?

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?

line 246: *stratification within* water sheets?

line 274: what does "exponentiated" mean in this context? does L become an exponent somewhere?

line 287 2nd "of till" redundant
