Comment on tc-2021-242
Anonymous Referee #2

Referee comment on "Ice-shelf ocean boundary layer dynamics from large-eddy simulations" by Carolyn Branecky Begeman et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-242-RC2, 2021

This manuscript addresses boundary layer dynamics at the ice-ocean interface motivated by obtaining novel parameterizations of ice-shelf melting. This is a highly important topic: increasing complexity of ice and ocean models will not improve the estimates of melt rates with inappropriate representation of the processes in the boundary layer. Although the method chosen in the paper (LES using relatively low resolution) gives the results of limited applicability, which is admitted by the authors, it provides some important insights. The manuscript can be published if the authors address the following points.

Major points:

- Section 2.3: Please add the figure illustrating simulation setup for your base case. Add more detail in this figure: currently even the sizes of the domain are not written anywhere.
- L 100: please add more detail about validation in the appendix. It would be useful to have an idea how the chosen model works compared to other model (which one? Was it DNS or measurements or another LES model?).
- L 150: How does the resolution influence the results? Why this particular resolution was chosen?
- L 157-158: It would be helpful to write the boundary conditions explicitly, not just Dirichlet/von Neumann. If there is a flow developing along the ice face due to buoyancy, how justified is the use of the periodic boundary conditions?
- The whole Sec. 2.2, called 'Turbulence closure', is about boundary conditions at the ice-ocean interface.
- Section Results should be split into smaller subsections with separate titles to make it more reader-friendly.

Minor points:

Please check the equations. Several equations have misprints:
Eq (1), rhs: 1st term: should be $x_j$ and not $x_i$; 3rd term: what does index $g$ in $u_g$ mean?

Eq (3),(4): 2nd terms in the rhs: should be $u_j$ not $u_i$;

Eq (11): not clear, does this mean that gamma is the sum of the molecular and turbulent coefficients? Why the form with power `-1` is chosen if that power can be removed from this equation. In addition, I have not found this or similar expression in McPhee et al (1987), please clarify.

Eq (14) $c_p$ should be removed, it makes the expression dimensionally inconsistent.

- L 146: ‘It is noted by the PALM developers that this error was found to be small’ – please provide a reference here.
- L 168: far field thermal driving is 0.15C, but in the figures elsewhere, there is no such case, only 0.1C. I believe one of those is a misprint.
- L 147: ‘This error would be small if the first ∼10 cm were largely a constant flux layer, as hypothesized by McPhee (1983).’ – I do not understand this sentence. Clearly, if 10 cm were a constant flux layer, then the mentioned fluxes would be equal. What was hypothesized by McPhee?
- L 205: ‘thermal driving increases from 0.5 to 0.6°C’: there is no 0.5C case in the figure.
- Fig. 1: the caption does not match the figure: (a) is not TKE but friction velocity etc.
- L 347: critical gradient Richardson number for development of hydrodynamic instabilities, and further transition to turbulence, $\text{Ri}_g=0.25$, has been first obtained by Miles and Howard in 1961.