Response to reviewers for "Ice-shelf ocean boundary layer dynamics from large-eddy simulations"

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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.

We have added a schematic figure (Figure 1). This text specifying the domain size was included in the original manuscript, but it would be easy to miss. With the new figure, it should be more visible

"The domain is a 64 m^3 cube"
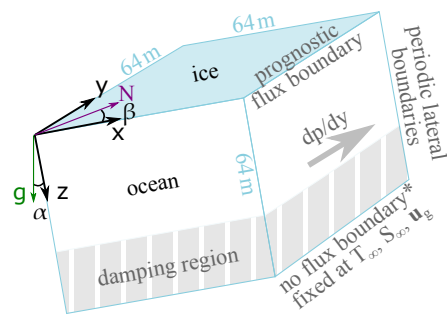
Figure 1. Schematic of the simulated ocean domain with background pressure gradient dp/dy. Purple arrow is oriented north and green arrow is aligned with gravitational acceleration. *The bottom boundary condition is Dirichlet, but there is also no flux as a result of damping.

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?).

We have added a figure to the supplemental materials (Figure S1) and the following text to the manuscript. We don't feel that too much information is needed about model set-up because we follow Abkar and Moin (2017) exactly.

"We validated our implementation against the stable atmospheric boundary layer test case published in Abkar and Moin (2017) and Stoll and Porté-Agel (2008). We follow Abkar and Moin (2017, 's)'s model set-up exactly and chose their moderate resolution with 72x72x72 grid points. Our results are in good agreement with Abkar and Moin (2017, 's) solution employing another LES model with AMD (boundary layer height differs by <10%) and other LES models with different turbulence closures (Fig. S1, their Fig. 1 and references therein)."
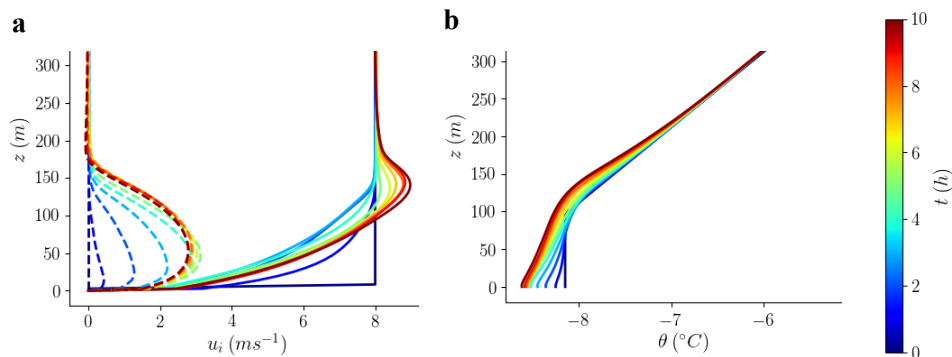
Figure S1. Results of the atmospheric stable boundary layer test case presented in Abkar and Moin (2017) using PALM with the Anisotropic Minimum Dissipation (AMD) turbulence closure. Compare with their Figure 1.

L 150: How does the resolution influence the results? Why this particular resolution was chosen? *We chose this resolution as the highest we could afford for this parameter study while achieving similar behavior to higher resolution simulations. We conducted one simulation at twice the resolution and compared the results, now described in the following text and a supplemental figure. We only ran the higher resolution simulation for one inertial period after spin-up due to the availability of computational resources.*

"We evaluated the effects of doubling both horizontal and vertical resolution in a separate simulation with an otherwise identical set-up to the base case, run for one inertial period after spin-up. While a greater portion of the vertical heat fluxes was resolved as expected, the melt rate averaged over one inertial period only increased by 7% and differences in the mean state were sufficiently small to justify the use of our standard resolution (Figure S2)."

L 157-158: It would be helpful to write the boundary conditions explicitly, not just Dirichlet/von Neumann.
We have modified this paragraph to explicitly state the boundary conditions, and rearrange some of the text accordingly. The relevant sentences are:

"We set von Neumann boundary conditions at the top boundary corresponding to the dynamic sub-grid momentum
and scalar fluxes (Equations 8, 14, and 15, as resolved fluxes go to zero at a no-penetration boundary. ... Boundary
conditions are Dirichlet at the bottom boundary, set to the far-field temperature, salinity, and geostrophic velocity.
... The flow is periodic along the x and y dimensions."

If there is a flow developing along the ice face due to buoyancy, how justified is the use of the periodic boundary conditions?
*In the literature, periodic boundary conditions in the context of simulations of the IOBL have been employed either to represent
the domain moving with the IOBL in a Lagrangian sense (Chen et al. 2021. The Cryosphere Discussions.) or the domain
remaining fixed in space with periodic boundary conditions that provide the opportunity for the development of turbulence and
of equilibrium conditions (Vreugdenhil and Taylor, 2019; Rosevear et al., 2021). We take the latter approach, with the intent
to bring the flow to equilibrium with the forcing by the end of the simulations. As discussed in Section 4.1, equilibrium was not
achieved.*

The whole Sec. 2.2, called 'Turbulence closure', is about boundary conditions at the ice-ocean interface.
*We agree that this may not be the clearest descriptor of this section. We move the first paragraph to the previous section and
rename this section "Boundary conditions for ice-shelf melting."*

Section Results should be split into smaller subsections with separate titles to make it more reader-friendly.
*Good suggestion. We have added the following subsections: Overview of the mean simulated state, Turbulent kinetic energy
budget, Boundary layer turbulence, Melting and its relation to thermal driving and slope, Vertical structure of turbulent fluxes.*

Minor points:

Please check the equations. Several equations have misprints:
Thank you for reviewing the equations so carefully. We have made the changes you requested

Eq (1), rhs: 1st term: should be x_j and not x_i; 3rd term: what does index g in u_g mean?
We have added the following text to define u_g:

"The momentum terms on the right hand side of Equation 1 are, in order: advection, Coriolis forcing, imposed
geostrophic flow with a geostrophic velocity u_g, ..."

Eq (3),(4): 2nd terms in the rhs: should be u_j not u_i;
Change has been made.

Eq (11): not clear, does this mean that gamma is the sum of the molecular and turbulent coefficients?
The text has been modified to make this equation more clear:

"where the exchange coefficients are defined with two terms, one representing turbulent transfer within the turbulent surface layer and one representing molecular transfer within the viscous sublayer following McPhee et al. (1987) (their Equation 11):."

65   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.

The power of -1 was not necessary in these equations an has been removed. This form (without the -1 power) can be found in Equation 11 of McPhee et al. (1987) where the second term is the molecular contribution.

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

70   *Change has been made.*

L 146: 'It is noted by the PALM developers that this error was found to be small' –please provide a reference here.

This note is embedded in the code itself. We provide the link here but we do not think it is an important reference for the readers as no further information is provided in the manuscript.

https://github.com/lanl/palm_lanl/blob/7f1444073b20ae81b451d4ab469d13e66b5358b7/trunk/SOURCE/surface_layer_fluxes_

75   mod.f90#L2513-L2519

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. It was actually the legend of these figures that did not show the thermal driving with sufficient precision. We have increased the precision such that the 0.15C case is labeled appropriately in all figures.

L 147: 'This error would be small if the first ∼10 cm were largely a constant flux layer, as hypothesized by McPhee (1983).'

80   – 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?

We have edited the text to be a bit more specific about what we mean. We hoped to convey that the surface layer is thought to deviate only slightly from a constant flux layer. We also changed the citation to McPhee (1981), which contains this argument in more detail (though the 1983 paper provides the extension to scalar fluxes).

85   "This error would be small if the first ∼10 cm were nearly a constant flux layer; McPhee (1981) hypothesized that the sub-ice surface layer would be nearly but not exactly a constant flux layer."

L 205: 'thermal driving increases from 0.5 to 0.6C ': there is no 0.5C case in the figure.

We have corrected this typo to refer to the 0.4 °C thermal driving case.

Fig. 1: the caption does not match the figure: (a) is not TKE but friction velocity etc.

90   *We have replaced Figure 1 with the version with TKE in panels a,d, now Figure 2.*
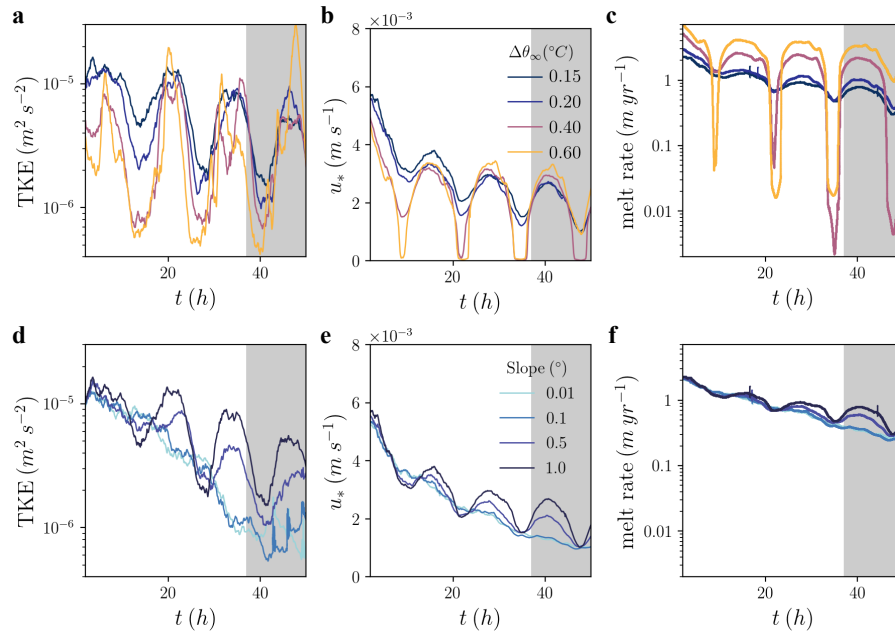
Figure 2. Time evolution of (a,d) domain-averaged, resolved turbulence kinetic energy (TKE), (b,e) melt rate and (c,f) friction velocity for (a-c) thermal driving simulations and (d-f) variable slope simulations. The black curve represents the same simulation in all panels in this and subsequent figures. The analysis window, the last inertial period, is shaded.

L 347: critical gradient Richardson number for development of hydrodynamic instabilities, and further transition to turbulence, Ri_g=0.25, has been first obtained by Miles and Howard in 1961.

Thanks for the tip. This reference has been added.

References

95 Abkar, M. and Moin, P.: Large-Eddy Simulation of Thermally Stratified Atmospheric Boundary-Layer Flow Using a Minimum Dissipation Model, Boundary-Layer Meteorology, 165, 405–419, https://doi.org/10.1007/s10546-017-0288-4, 2017.

McPhee, M. G.: An analytic similarity theory for the planetary boundary layer stabilized by surface buoyancy, Boundary-Layer Meteorology, 21, 325–339, https://doi.org/10.1007/BF00119277, 1981.

McPhee, M. G., Maykut, G. A., and Morison, J. H.: Dynamics and Thermodynamics of the Ice/Upper Ocean System in the Marginal Ice

100 Zone of the Greenland Sea, Journal of Geophysical Research, 92, 7017–7031, https://doi.org/10.1029/JC092iC07p07017, 1987.

Stoll, R. and Porté-Agel, F.: Large-Eddy Simulation of the Stable Atmospheric Boundary Layer using Dynamic Models with Different Averaging Schemes, Boundary-Layer Meteorology, 126, 1–28, https://doi.org/10.1007/s10546-007-9207-4, 2008.