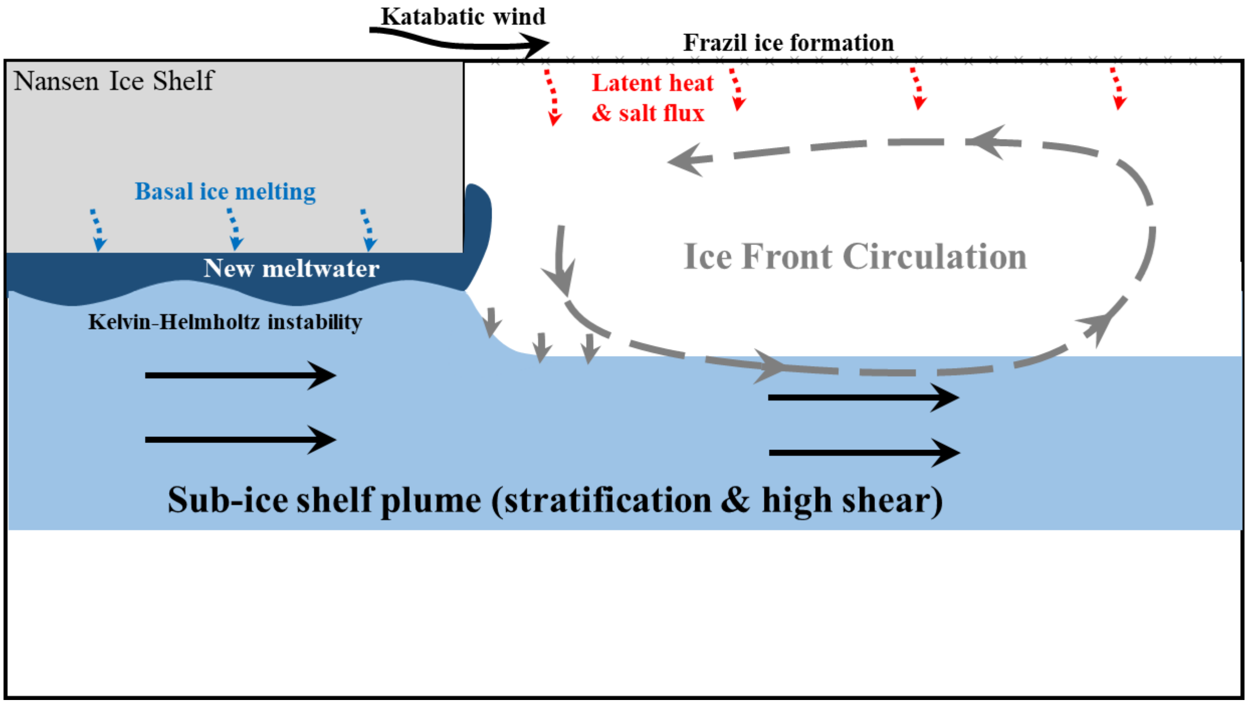
Response to comments

We wish to thank anonymous reviewer for their valuable comments, which have helped us to improve our manuscript. We addressed each of the comments in turn below. Our responses are colored by green.

* New simulation summary

We really appreciate the reviewer's valuable and helpful comments on the improvement of model validity. We performed a suite of new simulations with proper forcing based on the reviewer's comments and physical oceanography of Nansen ice shelf. Before response to each of the comments, we would like to address major changes and summary of new simulation set-up for model validation with proper forcing based on in-situ observations.



R-Figure 1. Schematic diagram of physical processes within model domain based on in-situ CTD, LADCP, AWS (in ice breaker Araon) observations. It represents our conclusion inferred from observational and experimental evidences in revised manuscript.

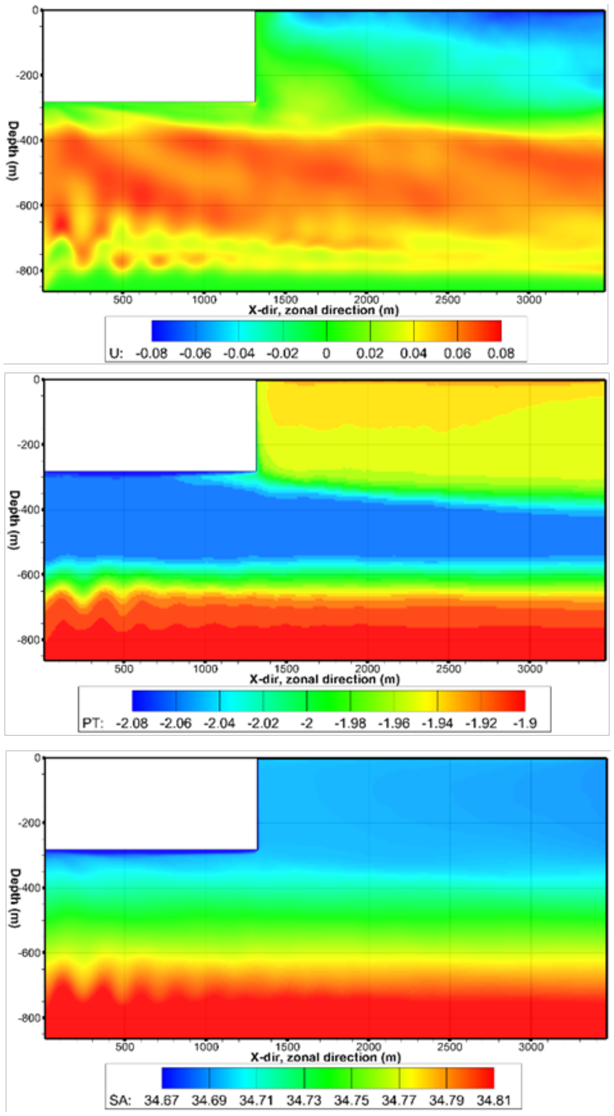
To answer the first comment of the first reviewer, we described the physical oceanographic processes and our finding in Nansen ice shelf using schematic diagram (R-Figure 1)

Through observing negative velocity near sea surface and positive velocity at sub-ice shelf plume in our LADCP observations, we define the “Ice Front Circulation” (In this study, we define this circulation de novo). Shear by momentum of sub-ice shelf plume and salt flux by frazil ice formation (~1 cm day^{-1}, it is determined by sensible heat (164.8W m^{-2}) based on air temperature (–7.76 °C) and wind velocity (16.23 m s^{-1}) obtained by AWS in ice breaker Araon) could trigger this circulation. The relationship between sensible heat and frazil ice production (frazil ice production = 0.1785 x Q_s – 28.048) is referred from Thompson et al. (2020). This circulation pushes the sub-ice shelf plume, making that stratification line is moved to 400 m depth. Beneath the ice shelf, basal ice melting occur because sub-ice shelf plume has a warmer temperature (–2.06 °C) than local freezing temperature (–2.115 °C) (Note that the freezing temperature was set too high as –1.92 °C in previous simulation. New simulations show not refreezing but melting and it will be described in revised manuscript.). Newly generated meltwater by basal melting is located between ice shelf bottom and stratified sub-ice shelf plume. By density difference between new meltwater and sub-ice shelf plume, shear instability (e.g. Kelvin-Helmholtz instability) occurs at this depth range (from 280 m to 300 m depth).

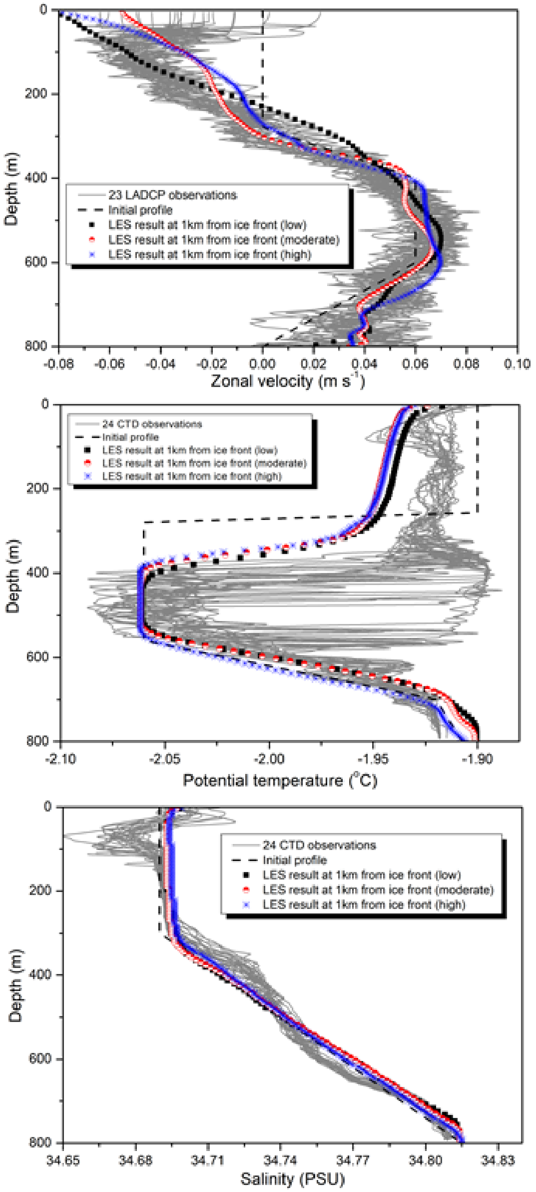
To prove this hypothesis, we set the latent heat and salt fluxes (corresponding to 1 cm day^{-1}) at top boundary and velocity of sub-ice shelf plume (similar with LADCP) at inlet boundary. In temperature between sub-ice shelf plume and ice shelf, –2.06 °C was set. In salinity profile, high stratification was set at 300 m with initial depth (20 m) of new meltwater. In upper region (from 0 to 280 m depth) at the open ocean, 0 m s^{-1} velocity and zero velocity shear at top boundary were set to prove the development of Ice Front Circulation and its trigger mechanism.

To resolve interfacial temperature and salinity by different depths, we used the equation of $\theta_b = \lambda_1 S_b + \lambda_2 + \lambda_3 P$, instead of Eq. (12) $\theta_b = -\lambda S_b$ in previous manuscript.

To investigate the effect of grid size on model performance, we tested the three kinds of grid systems (low case – 216 (16 m) x 216 (16 m) x 108 (8 m), moderate case – 288 (12 m) x 288 (12 m) x 144 (6 m), and high case – 432 (8 m) x 432 (8 m) x 216 (4 m), respectively. In this grid sensitivity study, we conclude that grid resolution in moderate case is enough for resolving this oceanic flow and IOBL.



R-Figure 2. xz plane contours of time averaged variables of velocity (upper), potential temperature (middle) and salinity (lower) in new simulation (moderate case).



R-Figure 3. Vertical profiles of velocity, potential temperature and salinity in new simulation.

Also, we summarized important parameters and constants for basal ice melting at ice shelf bottom and frazil ice formation at sea surface (R-Table 1).

R-Table 1. List of model parameters and constants

λ_1	Freezing temperature salinity coefficient	–0.0573	°C kg g^{-1}
λ_2	Freezing temperature constant	0.0832	°C
λ_3	Freezing temperature depth coefficient	-7.53×10^4	°C m^{-1}
Γ_S	Salt turbulent exchange coefficient (sea surface, ice shelf bottom)	2×10^{-4} [a], 2.6×10^{-4} [b]	-

Γ_θ	Heat turbulent exchange coefficient (sea surface, ice shelf bottom)	5.8 x 10^{-3} [a], 8 x 10^{-3} [b]	-
c_w	Specific heat capacity of pure water	3974	J kg^{-1}°C^{-1}
L_i	Latent heat of fusion	3.35 x 10^{5}	J kg^{-1}
ρ_w	Density of water	1028	kg m^{-3}
ρ_i	Density of ice	917	kg m^{-3}
z_0	Surface roughness (sea surface, ice shelf bottom)	0.001, 0.07 [c]	m
-	Ice shelf thickness	280 [d]	m
θ_f	Local freezing temperature (sea surface, ice shelf bottom)	–1.9, –2.115	°C
θ_a	Ambient temperature (sea surface, ice shelf bottom)	–1.9, –2.06	°C
θ_b	Interfacial temperature (sea surface, ice shelf bottom)	–1.885, –2.092	°C
Sa_a	Ambient salinity (sea surface & ice shelf bottom)	34.69	psu
Sa_b	Interfacial salinity (sea surface, ice shelf bottom)	34.864, 34.286	psu
k_θ	molecular diffusivities of heat	1.3 x 10^{-7}	m^2 s^{-1}
k_S	molecular diffusivities of salt	7.2 x 10^{-10}	m^2 s^{-1}

a - Heorton et al. (2017)

b - based on friction velocity (0.145 m s^{-1}) and thermal driving in new simulation (refer to Vreugdenhil and Taylor (2019))

c - high drag coefficient ($C_d = 0.01$) of cold water cavity in Gwyther et al. (2015)

d - Stevens et al. (2017)

Anonymous Referee #2

- General comments

This paper has an interesting premise and some potentially significant results, but substantial analysis and revision is required before it can be published. There have been very few LES studies conducted on the ice shelf-ocean boundary layer (IOBL) and I have yet to see one with refreezing included, which makes this work of potentially great interest. However, I am concerned that the set up, validation and analysis of the LES needs considerably more work. I have outlined my major concerns below in the specific comments section, and I would like to see these comments thoroughly addressed.

- Specific comments

1. I have concerns about the set up of the LES. The authors describe that the inlet plume boundary condition beneath the ice shelf (and the initial conditions across the whole domain) are set from in situ observations. The in situ observations are measured at the front of the ice shelf, but the inlet plume boundary condition is much further beneath the ice shelf (1000m or so horizontally). According to the authors and the premise of the paper, the IOBL undergoes significant brine rejection, latent heat release and mixing due to refreezing at the ice edge. The resulting profiles of temperature, salinity and velocity in the open ocean are then compared against the same in situ

measurements used to force the simulation, to conclude that the refreezing effect is significant. It does not seem appropriate to force the model with the in situ field measurements (with the inlet forcing at a different region from where the field measurements were taken) and then validate the model against the same in situ measurements (but this time using model results in front of the ice shelf, similar to where the field measurements would have been taken). Perhaps I am missing something in the set up here?

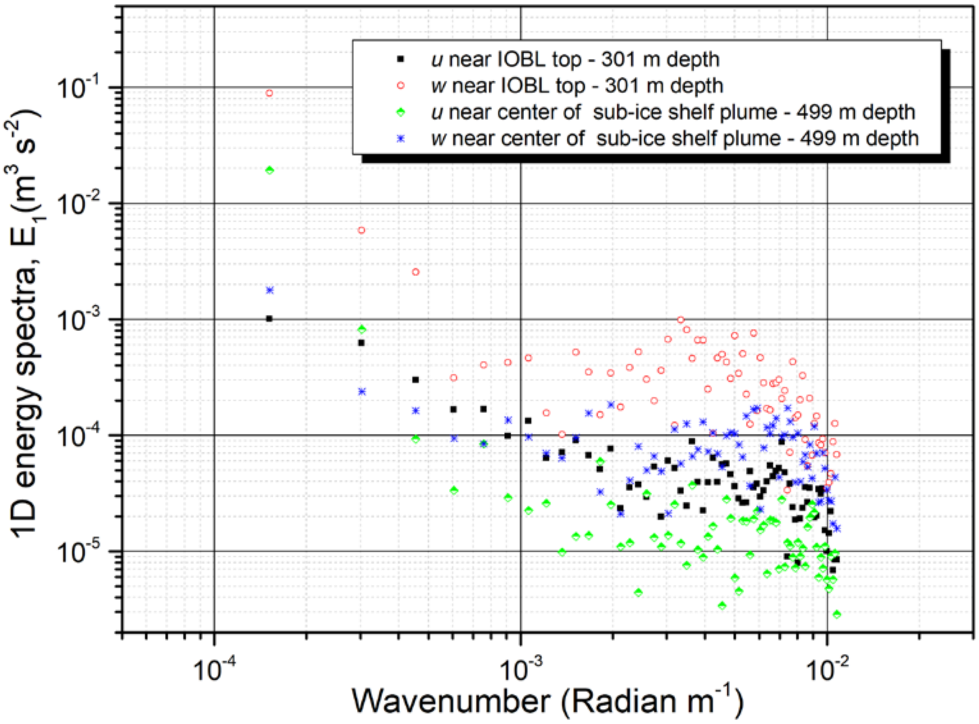
The primary difference between the refreezing and no-refreezing LES depth profiles in front of the ice shelf is then found to be the increase in temperature in the top 400m (Figure 6). I agree that the refreezing case looks closer to the field measurements than the no-refreezing case. But I also question that the initial conditions, and perhaps more importantly the inlet conditions on the plume, have a temperature profile that has values smaller than the average measurement profiles (in particular at 400-550m depth on Figure 6). The authors need to further justify why they have chosen these initial profiles (dashed lines on Figure 6), and any differences that have been made from the field measurements. It is also not clear to me that the solution they have found is truly unique, in that the inlet temperature profile could potentially be tuned to find that the no-refreezing case also gives a good match with the field measurements. I would appreciate a more transparent explanation of the LES set up, including a thorough discussion on the scientific premise going into the set up.

- As the reviewer mentioned, we concluded that the validation and LES setup with forcing & boundary conditions had some issues. Therefore, we composed new simulation set-up with different boundary conditions based on our hypothesis (R-Figure 1), using only the properties of sub-ice shelf plume. In new simulation, initial and boundary conditions were composed by our hypothesis, not from the in-situ observation.

---

2. I would like to see further validation of the LES in terms of whether it is resolving the dynamics. The authors currently have only one grid resolution here, so further justification to show why they would expect convergence with a higher grid resolution would be appreciated. The LES resolution validation in the paper was solely focused on the energy spectrum. While it is reassuring to see the -5/3 spectrum, there are other variables that really should be considered, in particular the stress, heat and salt flux. Figure 10 already shows the momentum and heat fluxes with the resolved and SGS terms. The SGS terms appear quite small throughout the whole depth, which is a good sign for LES convergence, but I would like to see this compared against the total values for each depth, along with some discussion. Does the salt flux also look resolved throughout most of the depth (especially near the ice)?

- As the reviewer mentioned, we have tried to validate our grid resolution is proper in this oceanic flow through energy spectrum of *u* velocity and resolved & SGS flux of heat and momentum. As shown in R-Figure 4, we will plot the *u* and *w* velocity which are related with shear and buoyancy effects. Also, we will plot the momentum, heat and salt fluxes. As the reviewer mentioned, SGS fluxes near the ice have relatively large values. It can be solved using stretched-vertical grid, but this has a problem for employing in this study, because simulation domain in this study have an ice shelf geometry. We will discuss the impact of relatively large SGS fluxes near the ice surface in discussion section in revised manuscript.



R-Figure 4. 1D energy spectra of *u* and *w* velocity in new simulation results.

---

3. There are some really interesting dynamics in the LES output but I am struggling to understand them and put them into context of past work. For example, the eddy at the ice front is very intriguing but there is not much information on its dynamics. I did not know whether the authors were referring to a vertical overturning type eddy, or a horizontal eddy. If the latter, is it a baroclinic eddy? What is setting the size of this eddy (e.g. the domain size or a Rossby length scale)? Is the potential vorticity in the under-ice region important in constraining where the eddy forms?

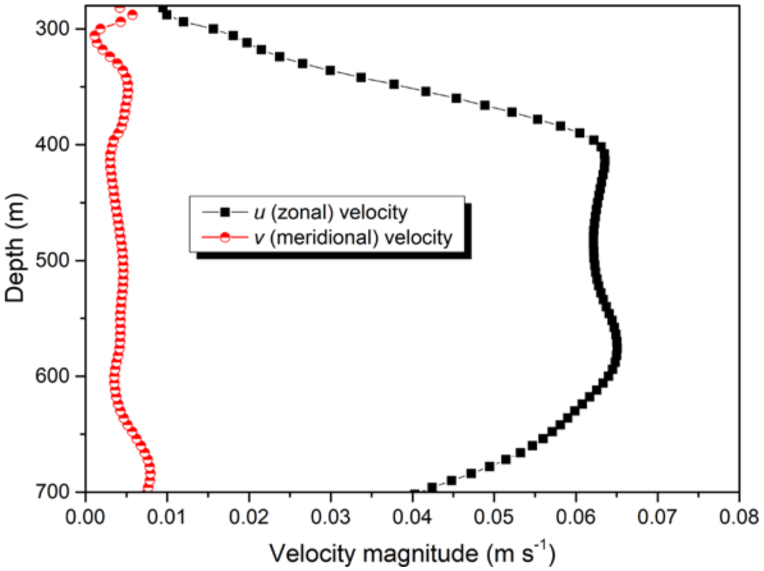
- Instead of ice front eddy, we propose the ice front circulation in open ocean based on new simulation results. This circulation is vertical overturning type and has baroclinic eddy characteristics. We will evaluate a budget and balance through comparison between buoyancy effect and shear effect by sub-ice shelf plume in revised manuscript.

Other dynamics that come to mind are the brine rejection from refreezing. Is the brine rejection enough to generate convective plumes and a convective region? Is this the cause of the mixing in the refreezing zone? I cannot see any brine rejection influence on the salinity field (Figure 3) but this might be hidden by the colormap scale. Is there a density inversion here? Also are there any double-diffusive effects expected in the water column?

- In new simulation, we can observe the density inversion near the sea surface (R-Figure 2, 3). But, double-diffusive effects was not observed.

What are the effects of the Coriolis parameter in these simulations? Is the horizontal movement of the plume affected by geostrophic balance (is there a strong velocity in the y-direction)? Would we expect an Ekman layer to form near the ice base e.g. Jenkins 2016 (“A Simple Model of the Ice Shelf–Ocean Boundary Layer and Current”)?

- Within depth range (from 280 m to 400 m depth where *u* velocity gradient is large), weak *v* velocity was observed and structures of Ekman spiral layer was unclear.



R-Figure 5. Vertical profiles of velocities beneath the ice shelf.

In one of the few previous studies on the exit of meltwater plumes from under ice shelves, Naveira Garabato et al. 2017 (“Vigorous lateral export of the meltwater outflow from beneath an Antarctic ice shelf”) concluded that centrifugal overturning instability played an important role in setting the mixing of the meltwater plume. Are the present LES similar to these at all?

- In new simulation results, we had come to a similar conclusion of study of Naveira Garabato et al. 2017. We focused on vertical distribution, whereas they focused on kinetic energy extraction from lateral shear. We will discuss this in revised manuscript.

The paper would be really strengthened by discussing the dynamics in the context of past work. In the introduction in general, I would recommend discussing some more field observations (e.g. Larsen C by Davis and Nicholls 2019, Ronne ice shelf by Jenkins et al. 2010, Fimbul ice shelf by Hattermann et al. 2012, Ross ice shelf by Arzeno et al. 2014) to really put the Nansen Ice Shelf observations into context. Similarly, I think it would be worthwhile citing some more process-based studies here also, especially ones involving a meltwater plume (e.g. McConnochie and Kerr 2018; Mondal et al. 2019).

- As the reviewer mentioned, it needs to be more strengthened by discussing the other cold water cavity such as Larsen C, Ronne ice shelf, Ross ice shelf. We will re-organize the literature survey in introduction section, adding those studies in literature survey.

---

4. The ice-ocean boundary condition for refreezing is the commonly used "three-equation" model, but how appropriate is this model for the refreezing case (in particular the frazil ice case)? The turbulent exchange velocity of heat is set to be 10ˆ-4 m/s (please provide a citation for this value) but this could be influenced in some way by the refreezing dynamics, including the brine rejection or latent heat release. This three equation model is parameterising the heat and salt fluxes mixed towards the base of the ice, so I think more discussion should be included to justify the choice of turbulent exchange coefficients. This is a more minor comment, but could the authors define the friction velocity where it first appears (Eq. 14) and also explain how it is calculated (e.g. is a drag coefficient set?).

- When we performed the new simulation, we considered heat and salt exchange coefficients based on Vreugdenhil and Taylor (2019). Although it has a different friction velocity range, we can interpolate these values using physical relationship. We will discuss this in discussion section in revised manuscript. And, we will also explain the definition of friction velocity.

Li 26: please provide a citation for the line ". . .the ice shelf–ocean interaction in cavities is the dominant driving force for ice thinning"

- This sentence will be change to "the ice shelf-ocean interaction in cavities is one of the driving force for ice shelf instability" with reference.

Li 87: I would also appreciate more discussion of the type of LES used here, and why this type of LES SGS parameterisation would work well in this IOBL system. Has PALM has been used in previous studies of ice-ocean flow?

- In this study, 1.5 order Deardorff SGS model was employed for LES SGS parametrization. In this model, averaged features of IOBL can be resolved, because SGS stress is proportional to local gradient of mean quantity. It can have a huge difference in simulation results as we simulate the flow with high temporal-variability of local gradient, having a various mixing length. We think that this is the first study for ice-ocean flow using PALM. We will discuss this in discussion section in revised manuscript.

Li 90: Please describe the turbulence closure scheme used here in more detail. Eqs. (1-9): Please ensure that all variables are defined when first introduced (e.g. u_g, Rd, Cp not defined here). Also try to include the values of any constants (e.g. what value of Coriolis parameter f is used?).

- Description of some parameters were missing. We will add the explanation of turbulence closure and parameters. Coriolis parameter is -1.41 x10^{-4} which is corresponding to 75 S.

Eq. (6) and Li 113: are K_m and nu_T the same (they are defined similarly)?

- Those parameters are same. We will remove the nu_T in equations.

Li 113: citation for C_m= 0.1 and more explanation needed for the definition of l (where does 1.8z come from for instance?). Is Eq. (13) just the rearrangement of Eqs. (10-12)? If not, where does it come from?

- C_m is important factor for determining the eddy viscosity. We refer the Maronga et al. (2015). This mixing length is from Stable Boundary Lalyer study of Saiki et al. (2000). Eq. (13) is the solution of rearrangement equation of Eqs. (10-12). We will add this information to methodology section in revised manuscript.

Li 129: please include a citation for the turbulent exchange velocity of heat.

- We list-up these coefficients of heat and salt in R-Table 1.

Eqs (14-16): please define friction velocity u* when it first appears. Also is tau the boundary stress?

Li 141: Please justify the surface roughness value of 0.07m. Why was this chosen?

- As shown in R-Table 1, we set the surface roughness, referring the high drag coefficient, C_d=0.01 case in Gwether et al. (2015).

Li 143: How was the ice shelf modeled in the simulations? Was it immersed boundary, interface condition, etc?

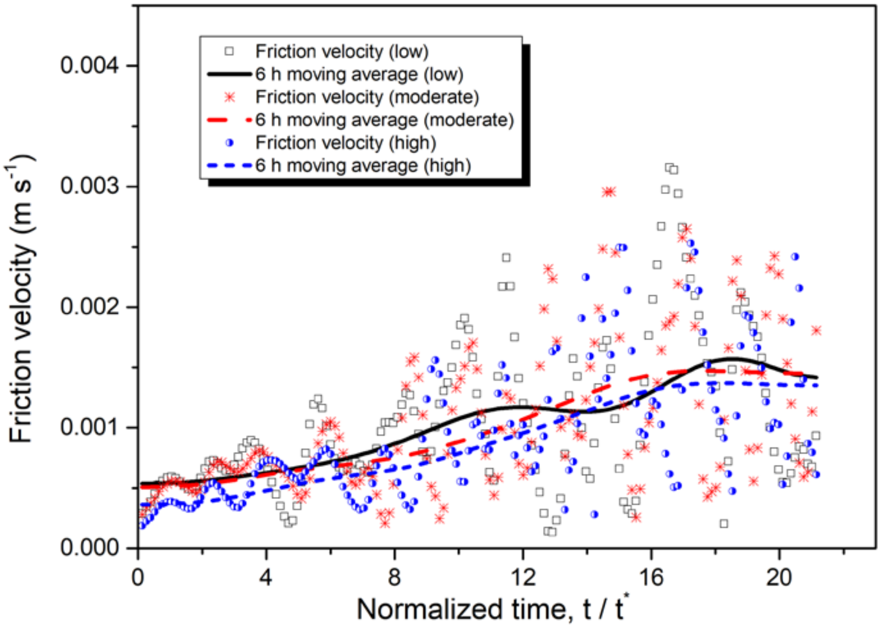
- We modeled ice shelf using mask method of Briscolini and Santangelo (1989) (fixed topography). We will explain this in methodology section in revised manuscript.

Li 150: How was the U_top value chosen? Is it based off observations? If so, please cite.

- In new simulation set-up, we set the 0 m s^{-1} at the top boundary to exclude the wind effect.

Li 174: the mean velocity of 0.0729m/s – where did this value come from? Also what is gained by putting the time in terms of the overturning time here?

- In previous simulation, we used the mean velocity and total domain scale to obtain the large eddy turnover time (t^*). Since it was not IOBL property, we used the friction velocity and initial IOBL depth to obtain the large eddy turnover time in IOBL. In new simulation, t^* is 3.83 hours which is determined by 0.145 m s^{-1} and 20 m depth of new meltwater (R-Figure 6)



R-Figure 6. Time series of friction velocity in new simulations with different grid resolutions.

Li 174: I would like more discussion of the fluctuations – it is mentioned that they are because of large-scale eddies beneath the ice shelf, but it would be nice to see what these eddies look a like a little more in terms of velocity, etc.

- In results section of new simulation, we will discuss velocity and its shear beneath the ice shelf to observe the shear effect in IOBL.

Li 177: “High momentum exchange by refreezing and its brine rejection. . . ” please explain the physical processes here a little further. Why does refreezing mean high momentum exchange?

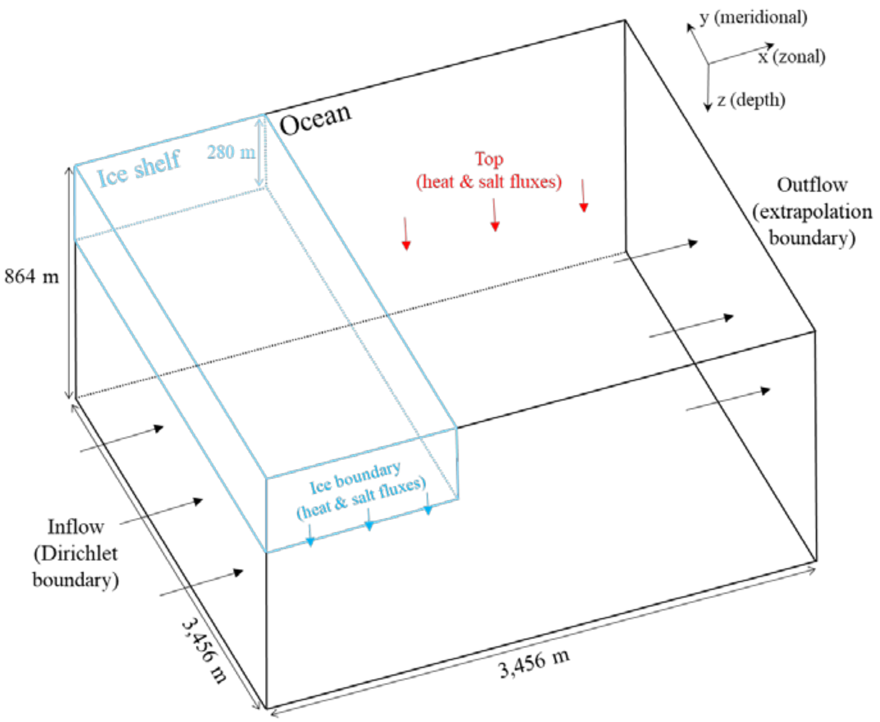
- In previous simulation, de-stratification by brine rejection induced the vertical fluxes of momentum and entrainment of momentum.

Li 178: The time averaged results of 1t* - why not use a longer time-averaging interval? Especially as the fluctuations appear to be on a slightly longer timescale (Figure 2).

- In new simulation, we used the last 3 t* for averaged results after 17 t* (R-Figure 6)

Figure 3 and onward: I have some confusion about zonal and meridional velocity here. For some reason I kept thinking that zonal was the y direction, but actually it was the x direction? Please define the velocity directions in terms of x and y, at least initially. This would help me out a lot!

- We will add the directions with boundary conditions in revised manuscript (R-Figure 7)



R-Figure 7. Domain information and boundary conditions

Figure 3: what are the undulations on the plume interface (top and bottom) on the far left hand side of the domain? Why do they form so strongly near the inlet condition? Are these the eddies referred to in Li 174?

- New simulation had also an undulations near the inlet conditions, showing the IOBL flow had the wave-like features (Kelvin-Helmholtz instability). In previous simulation, we did not consider the instability, but we will add the part for Kelvin-Helmholtz instability development which is linear stability analysis (Na et al. (2014)).

Li 181: What is the swirling strength criterion? Please include a one-line explanation.

- This method is for separating the swirling component of vorticity equation to identify the vortex structures. We will add a one-line explanation with reference in revised manuscript.

Li 182: “Due to neutral buoyancy, sub-ice shelf plume is about 100 m apart from the ice shelf and has a high velocity” This information is important and should come much sooner in the paper.

- In revised manuscript, we will add an explanation for this neutral buoyancy part to methodology section.

Li 192: “. . . where salinity stratification is not formed. . .” this is tricky to make out on Figure 3 colourmap. Perhaps think of using a different colormap here?

- We will try the different colormap, and choose proper colormap to examine those features obviously.

Li 194: “This demonstrates that the stratification is more dominant than flow shear near the ice front and play a major role in preventing flow advection from subice shelf plume.” Is this really shown here? Please explain more about what is meant by the role of “stratification” here?

In new simulation, we cannot observe the stratification is dominant than flow shear near the ice front. Instead of this, we will discuss the analysis of comparison between shear and stratification to generate the Kelvin-Helmholtz instability in IOBL.

Figure 6: Where were the vertical profiles taken in the LES?

- We obtained the profiles at 1 km apart from ice front in both previous and new simulations.

Li 226: “However, the multi-layered stratified characteristics of the salinity profile at depth of ice shelf bottom and IOBL top are observed in the case with refreezing effect.” What is “multi-layered stratified characteristics” referring to exactly?

- In previous simulation, we referred the multi-stratification layer at near IOBL top (400 m depth) in vertical profiles.

Li 229: “However, it should be noted that, since this is an idealized model, some differences can be expected between the simulated results and observations, in terms of ice shelf bathymetry and surface roughness, the temporal variability of the sub-ice water plume, the drag effect of frazil ice, etc.” Please discuss these further. E.g. what effects would each of these processes potentially have on the LES?

- Ice shelf bathymetry (e.g. local slope of ice shelf) and surface roughness can affect the structure of mean velocity and turbulence intensity. Temporal variability can be expected the change in turbulence properties such as intensity, shear production. We will discuss this in discussion section in revised manuscript.

Li 245: “Additionally, meridional direction-stretched structures are observed at the interface between the inner and outer regions.” Are these referring to the domain-sized undulations on Figure 7?

- In previous simulation, we referred the stretched eddy at interface between inner and outer regions.

Figure 8: Is this the vertical heat flux? Similarly in Li 251, “high negative heat flux” is this referring to the vertical or horizontal heat flux?

- Yes. It was the vertical heat flux. But, it is not related with melting (or refreezing) pattern. In revised manuscript, we will consider the integral of vertical flux (or entrainment quantity) to observe the relationship between melting pattern and fluxes.

Around Li 260: So is the supercooled water mixed up from the plume below, or horizontally (by eddy) from waters outside the ice shelf?

- In this part, we referred the super-cooled water was mixed up and advected to upper region.

Li 263: “Figure 10 shows the vertical profiles of momentum and heat fluxes within the IOBL. As shown in figure, the depth of the IOBL top (438 m) is determined to be the depth where the magnitude of the heat flux is 1% of the maximum heat flux induced by the sub-ice shelf plume.” Is this for the inner or outer region?

- We referred IOBL top in the inner region (438 m). IOBL top of outer region is 447 m. In new simulation, there was another criterion for IOBL top using momentum flux, because it is shear-dominant flow.

Li 265: “In the vertical momentum flux in the inner region, negative flux induced by refreezing and stratification is observed, showing that the IOBL flow in the inner region is in a stable condition.” I thought that there was little to no refreezing in the inner region?

- In previous simulation, there was little refreezing in the inner region, having a stable condition of IOBL flow, as the reviewer mentioned.

Li 267: “However, positive flux with large-scale advection (IOBL scale) induced by the ice front eddy is observed in the outer region, showing that the IOBL flow in the outer region is in an unstable condition” What about the top 20m where the flow appears to have negative momentum flux?

- In previous simulation, negative momentum flux at 20 m depth are induced by large refreezing rate and ice front eddy.

The flatness factor is of some interest, but I would be more interested to see the energy budget of the simulations (e.g. turbulent kinetic energy, buoyancy production term, etc). Have the authors thought about calculating the energy budget?

- We also have a vertical profiles of energy budget at each depth. If it is proper for the explanation our hypothesis, we will use the energy budget analysis, comparing this with fluxes or other turbulent properties.

First paragraph of the discussion. This paragraph is a nice description of the overall flow – it might be helpful to have this earlier, in the introduction or the simulation set up.

- In revised manuscript, we will re-organize the paragraph location to highlight our hypothesis and findings.

Conclusions: This should be more to the point, with a succinct summary of the main findings of the paper.

- Using the schematic diagram, we will specify the conclusions for our study.

Figures: please consider using different colourmaps for each of the velocity, temperature and salinity figures. It might be easier to follow and make comparisons.

- We will re-plot the contours using another colourmap in revised manuscript.

- Technical corrections

Li 32: type "groundling" should be "grounding". Also ". . .dense and salty water melts the ice. . . "

- It will be corrected as the reviewer mentioned.

Li 34: "tidal pumping melts" – what process is this referring to?

- In this sentence, we referred to increment of melt rate by tidal shear.

Li 39: Please rephrase the sentence "Even iceberg calving . . ." it does not make sense as it stands.

- We will rephrase this sentence in revised manuscript.

Li 51: ". . . preventing the heat entrainment. . ." Please be clear what heat entrainment is being referred to here.

- "Stratification by a density gradient of meltwater near ice surface prevents the heat entrainment"

Li 64: Gayen et al. 2016 used DNS not LES.

- It will be corrected as the reviewer mentioned.

Li 105: bracket missing in the fourth term on the RHS.

- It will be corrected as the reviewer mentioned.

Li 185: ". . . with negative mean velocity. . ." mean velocity in which direction?

- Zonal direction. We will clarify velocity direction in revised manuscript.

If possible, try not to start all the paragraphs with “Figure x . . .” it is a little clunky.

- We will amend this in revised manuscript.

Reference

Thompson, L., Smith, M., Thomson, J., Stammerjohn, S., Ackley, S., & Loose, B. (2020). Frazil ice growth and production during katabatic wind events in the Ross Sea, Antarctica. The Cryosphere, 14(10), 3329-3347.

Heorton, H. D., Radia, N., & Feltham, D. L. (2017). A model of sea ice formation in leads and polynyas. Journal of Physical Oceanography, 47(7), 1701-1718.

Vreugdenhil, C. A., & Taylor, J. R. (2019). Stratification effects in the turbulent boundary layer beneath a melting ice shelf: Insights from resolved large-eddy simulations. Journal of Physical Oceanography, 49(7), 1905-1925.

Gwyther, D. E., Galton-Fenzi, B. K., Dinniman, M. S., Roberts, J. L., & Hunter, J. R. (2015). The effect of basal friction on melting and freezing in ice shelf–ocean models. Ocean Modelling, 95, 38-52.

Stevens, C., Lee, W. S., Fusco, G., Yun, S., Grant, B., Robinson, N., & Hwang, C. Y. (2017). The influence of the Drygalski Ice Tongue on the local ocean. Annals of Glaciology, 58(74), 51-59.

Yoon, S. T., Lee, W. S., Stevens, C., Jendersie, S., Nam, S., Yun, S., ... & Lee, J. (2020). Variability in high-salinity shelf water production in the Terra Nova Bay polynya, Antarctica.

Maronga, B., Gryschka, M., Heinze, R., Hoffmann, F., Kanani-Sühring, F., Keck, M., ... & Raasch, S. (2015). The Parallelized Large-Eddy Simulation Model (PALM) version 4.0 for atmospheric and oceanic flows: model formulation, recent developments, and future perspectives. Geoscientific Model Development Discussions 8 (2015), Nr. 2, S. 1539-1637.

Saiki, E. M., Moeng, C. H., & Sullivan, P. P. (2000). Large-eddy simulation of the stably stratified planetary boundary layer. Boundary-Layer Meteorology, 95(1), 1-30.

Briscolini, M., & Santangelo, P. (1989). Development of the mask method for incompressible unsteady flows. Journal of Computational Physics, 84(1), 57-75.

Na, J. S., Jin, E. K., & Lee, J. S. (2014). Investigation of Kelvin–Helmholtz instability in the stable boundary layer using large eddy simulation. Journal of Geophysical Research: Atmospheres, 119(13), 7876-7888.