Response to comments

We wish to thank anonymous reviewer for their valuable comments, which will help 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.
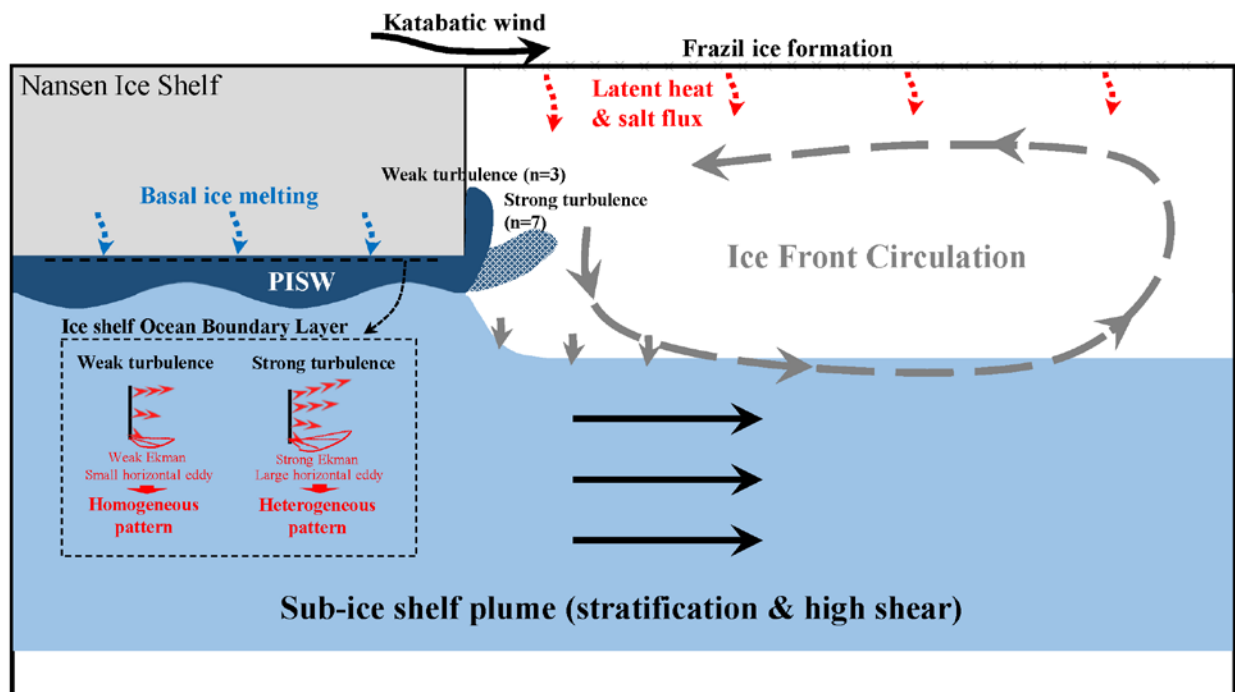
R-Figure 1. Schedamic diagram of physical processes within model domain based on in-situ CTD, LADCP, AWS (in ice breaker Araon) observations. It represents our conclusion inferred from observational and experimental evidences in revised manuscript.

To answer the first comment of reviewer, we describe the physical oceanographic processes and our finding in Nansen ice shelf in late summer using schematic diagram (R-Figure 1)
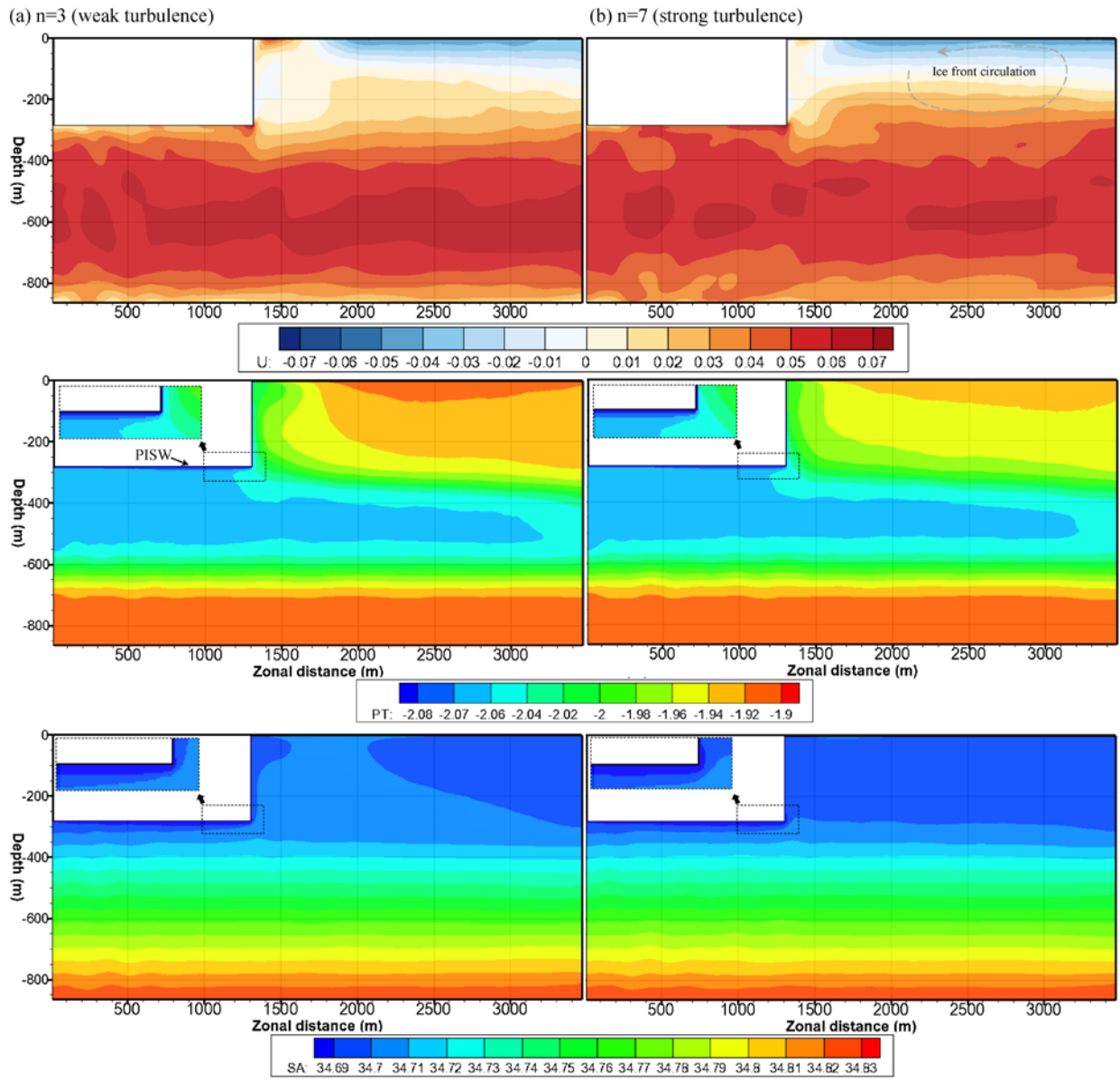
Through observing negative velocity near sea surface and positive velocity at sub-ice shelf plume in our LADCP observations, we define the "Ice Front Circulation" (In this study, we define this circulation de novo). Shear by momentum of sub-ice shelf plume and salt flux by frazil ice

formation (~1.38 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 350 m depth. Beneath the ice shelf, basal ice melting occurs 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 (Positively buoyant ice shelf water, PISW in revised manuscript) and sub-ice shelf plume, high speed current in high turbulence case occurs at this depth range (from 280 m to 400 m depth).
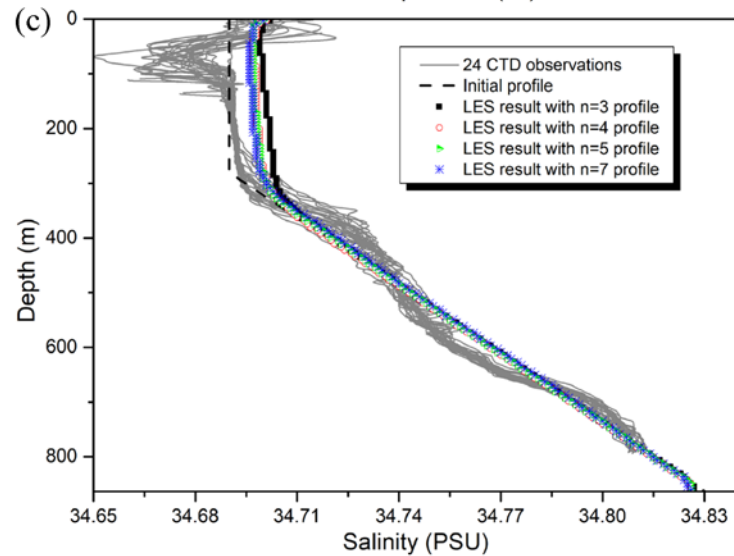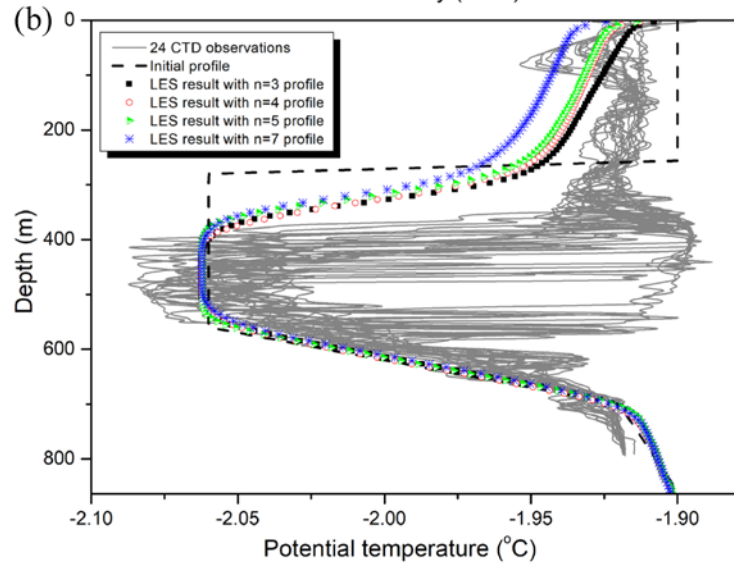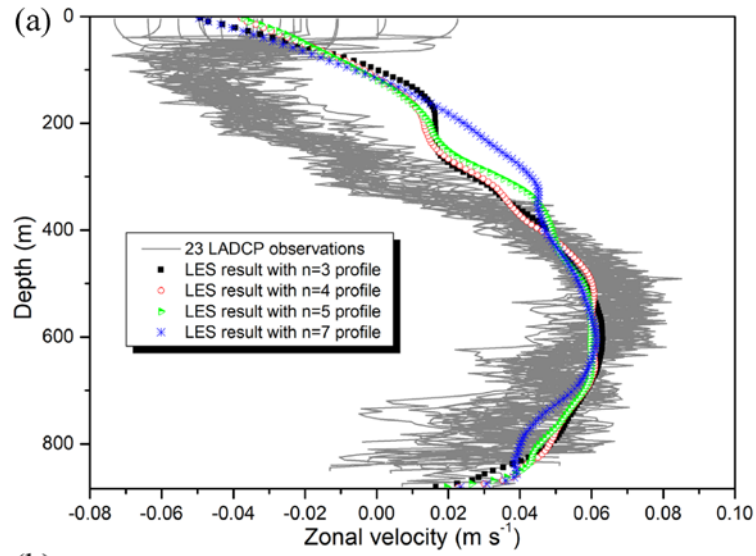
To prove this hypothesis, we set the latent heat and salt fluxes (corresponding to 1.38 cm day^{-1}) at top boundary and velocity of sub-ice shelf plume (similar with LADCP) at inlet boundary. In temperature between sub-ice shelf plume and ice shelf, –2.06 °C was set. In salinity profile, high stratification was set at 280 m. 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.

(a) n=3 (weak turbulence)

(b) n=7 (strong turbulence)

Ice front circulation

PISW

U: -0.07 -0.06 -0.05 -0.04 -0.03 -0.02 -0.01 0 0.01 0.02 0.03 0.04 0.05 0.06 0.07

PT: -2.08 -2.07 -2.06 -2.04 -2.02 -2 -1.98 -1.96 -1.94 -1.92 -1.9

SA: 34.69 34.7 34.71 34.72 34.73 34.74 34.75 34.76 34.77 34.78 34.79 34.8 34.81 34.82 34.83

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

(a)

(b)

(c)

Anonymous Referee #1

- General comments

There are important gaps in our knowledge of sub-ice shelf ocean dynamics, particularly in the

freezing regime. Na et al. have set their sights high in this study, but the manuscript as submitted does not provide sufficient evidence of model validity. In fact, there are several theoretical reasons to believe that the dynamics are not adequately represented in the model. Their argument for validity chiefly rests on the match between simulated temperature and salinity profiles and observations. Since the model has observations as initial conditions and an inflow boundary condition, it is unclear how far from observations the simulations could evolve. Furthermore, their presentation of the results and discussion of the turbulent dynamics must be more thorough to substantiate many of their scientific conclusions. This study could be publishable after an expanded discussion of model limitations and scaled-back claims to model realism, more elaboration of simulation results, and an expanded discussion of existing literature on frazil ice dynamics.

- As the reviewer mentioned, this study is new approach about sub-ice shelf ocean dynamics with sub-ice shelf plume which was observed in 2016/17 in-situ shipboard using 3D large eddy simulation with thermal and salty fluxes based on Monin-Obukhov similarity. To reply to the reviewer's comment, we conducted new simulations of the IOBL and ocean dynamics under other conditions (profiles) to prove the validity of this model. In response to comments, we made some efforts to explain the validation issue and several limitations.

- **Specific comments**

The connection to existing literature is inadequate. The readers need to know the physical oceanographic context for the region to assess the strengths and shortcomings of the model and the model setup. What is driving ocean circulation in reality and in the model? We also need to know what observational estimates exist of melting and freezing rates of the Nansen Ice Shelf to determine whether the simulated freezing rates are realistic (Is Mode 3 melting present at Nansen Ice Shelf?). Regarding the result that refreezing rates are high at the ice shelf front, is there any observational basis for this pattern or are you proposing it de novo?

– As the reviewer mentioned, Mode 3 (Antarctic Surface Water, AASW) was observed in Nansen ice shelf (red circle in R-Figure 4). However, the Modified Surface Water (MSW) and sub-ice shelf plume was mainly observed in our 24 CTD observations in late summer season. Therefore, driving forces of ocean circulation in our simulation are sub-ice shelf plume (generated by Mode 1) and salt flux at top boundary in the summer season. We will add and re-organize the literature survey for physical oceanography near Nansen ice shelf in revised manuscript.

– To describe the sub-ice shelf plume, we set prescribed-profiles for velocity, temperature and salinity as the inflow, because sub-ice shelf plume is generated at outside of simulation domain. Also, non-cyclic boundary condition (Dirichlet & extrapolation boundary conditions) was used for inflow and outflow boundary owing to spatial heterogeneous due to the presence of the ice shelf. These set-up of our model has the advantage of being able to resolve the target phenomena (e.g. IOBL flow, sub-ice shelf plume dynamics and ice front circulation) under this environment. We will explain this in methodology section in revised manuscript.

– Limitations and shortcomings are absence of the slope of ice shelf and temporal variability of sub-ice shelf plume because we only observe time-averaged features without the slope of ice shelf.
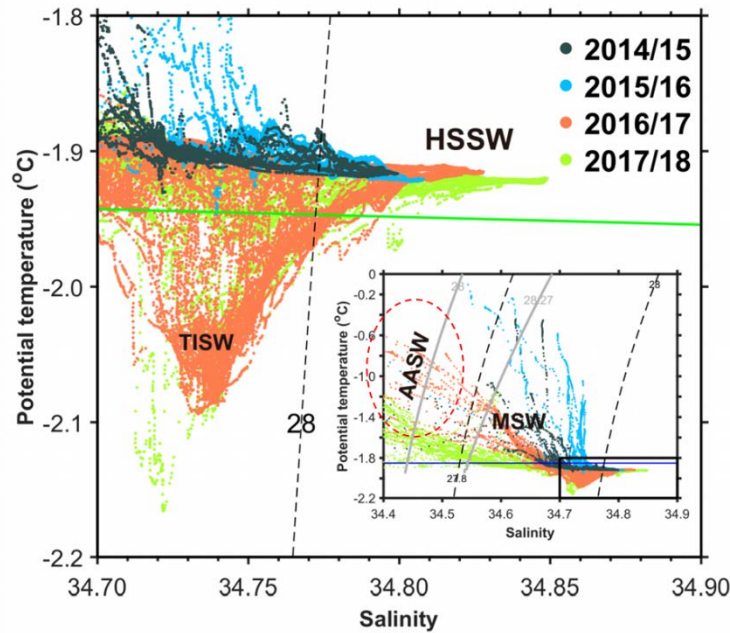
R-Figure 4. T-S diagram (Yoon et al. (2020))

Please provide more context for the observations that are used to initialize the model. There should be a brief presentation of the water masses that are present in the water column and their flow orientation (only zonal velocities are presented). The location of the observations should be shown on the study area figure, and text should indicate their distance from the ice shelf front and the time span over which these observations were collected. It is unclear whether these observations were presented in Yoon et al. (2020) or whether they are published here for the first time. Furthermore, the apparent bimodal temperature distribution at ～500 m depth also needs to be explained so that it's clear why you try to match the low temperature cases.

– As the reviewer mentioned, the explanation of observation was insufficient. With schematic diagram of R-Figure 1, we will explain the observation information in detail in methodology section. In new simulation, we used the average value of temperature (–2.06 °C) of apparent bimodal temperature distribution to examine the averaged feature of sub-ice shelf plume. Since this temperature determines the thermal driving at ice surface, we could consider various temperatures of sub-ice shelf plume in future study.


I have philosophical concerns about the manner in which the authors validate the model. Authors argue that the model is valid because the LES results match the observations, but the model is initialized to observations and has inflow that roughly matches the observations. Therefore, the argument seems to be that the model does not drift too far from observations, which may be too

weak an argument for model validity. It is also unclear to me where in the LES domain you are evaluating the agreement with observations. There should be a more thorough discussion of model limitations and a discussion of their possible impact on simulation results. There should be an explicit argument addressing whether your LES model can capture freezing dynamics in the absence of frazil ice dynamics. Frazil ice dynamics significantly influence IOBL evolution as documented in previous literature, which also should be cited (including the work of Galton-Fenzi). The lack of ice shelf slope should also be discussed. Furthermore, the parameterization of heat, salt and momentum fluxes at the ice base were developed for the ice melting case by McPhee et al. 1987. The applicability of this parameterization as well as the gamma_T, gamma_S exchange coefficients to the freezing case needs to be discussed.

– To prove the validity of this model, we made some efforts in set-up of initial and boundary conditions. In new simulation, model was validated under complex environments (new meltwater, sub-ice shelf plume and frazil ice formation at the sea surface), in terms of driving ocean circulation, its scale and quantities & trend of variables (velocity, PT and Sa). Differ to the previous simulation, we considered ice melting dynamics beneath ice shelf in new simulation. In latent heat and salt fluxes at sea surface, we considered only total heat content and salt quantity by frazil ice formation (1 cm day^{-1}) without the frazil ice dynamics. Also, we imposed gamma_T, gamma_S based on high-resolution LES study of Vreugdenhil and Taylor (2019) and sea ice study of Heorton et al. (2017). These parameters and absence of frazil ice dynamics will be discussed in discussion section in revised manuscript.

How do you know that the strong refreezing anomaly at the ice front is not a numerical artifact related to the front geometry?

– In new simulation, similar trend of melt rate was observed near this region because upwelling of new meltwater and entrainment of outer ocean, as shown in R-Figure 5. We conclude that there is no numerical artifact because we have checked that momentum flux layer of Monin-Obukhov similarity is established well at node points near the edge of ice front.
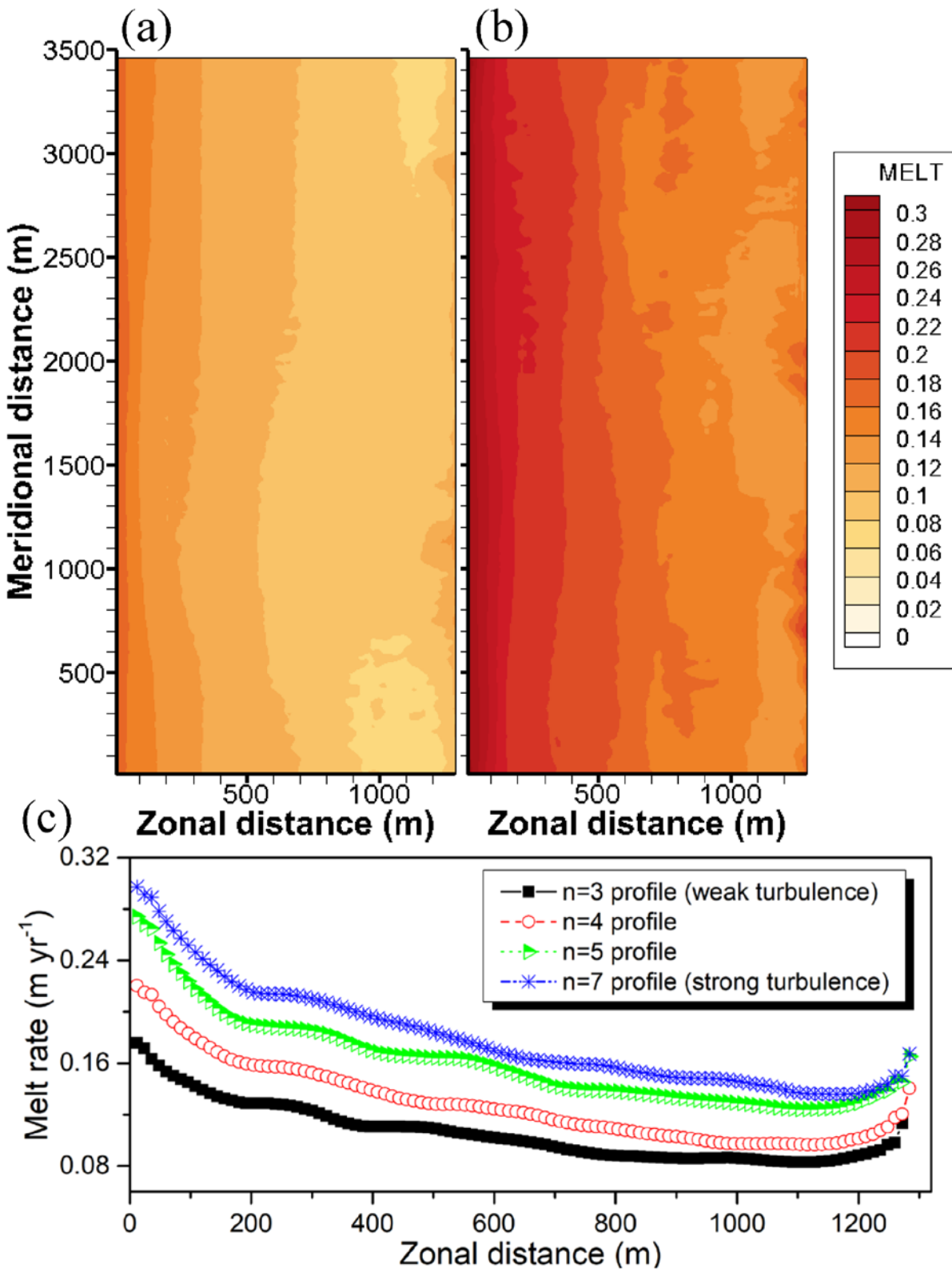
R-Figure 5. Melt rate (m yr-1) distribution of new simulation

What physically determines the location of the transition from the inner to the outer region in terms of refreezing rate? How do you know that this transition isn't just characterized by the development of turbulence along-flow from a less-turbulent inflow? Is there any observational evidence for these trends in refreezing rates, either at this ice shelf or any ice shelf?

– Because we imposed theoretical inflow of sub-ice shelf plume (based on in-situ LADCP observation profile), the region of turbulent development is relatively small (~ 300 m). In result analysis of revised manuscript, we will exclude the region of turbulence development only to observe the fully developed features.

Line 13: "In particular, it is evident that, when the refreezing effect is considered, the IOBL flow can be more realistically resolved, especially upward advection from the sub-ice shelf plume and the ice front eddy." You don't show that the entrainment and eddying in the freezing case are more realistic, if by realistic you mean closer to observations.

– In new simulation, we observed the development of ice front circulation under environments with meltwater production and salt flux based on in-situ observation. Therefore, we conclude that this simulated oceanic flow is realistic, representing the physical oceanographic processes in Nansen ice shelf.

Lines 30-45: The modes of ice shelf melting and the classification of warm and cold water cavities should be presented in the context of your study. This paragraph feels too general and unfocussed.

- As the reviewer mentioned, we will include the modes of ice melting, the classification of warm and cold water cavities and Nansen ice shelf characteristics in introduction section in revised manuscript.

Line 41: This sentence should state that the basal melt rate of ice shelves is determined by the rate of BOTH heat and salt exchange, as salt exchange plays a key role in the dissolving regime characteristic of most ice shelf settings.

- We will amend this sentence, including the importance of dissolving regime and salt exchange.

Line 76: "we were able to account for refreezing patterns, detailed flow structures including turbulent characteristics, fluxes and the relationship between refreezing and entrainment of supercooled water from sub-ice shelf plume within the IOBL." This makes it sound as if all of those properties were observed, when in fact you don't present observations that can be linked with those characteristics.

- As the reviewer mentioned, this sentence was not clear. We will amend this sentence, emphasizing that those characteristics were obtained from validated simulation results.

Line 134: I might have missed it but I can't find the value of z_0.

- In R-Table 1, we list-up z_0 values and its reference.

Line 136: It is unclear how the no-refreezing case is configured. Are both heat and salt fluxes set to zero at the top boundary?

- In revised manuscript, we will exclude the no-refreezing case (case without heat and salt fluxes).

Line 145: Argue for the appropriateness of setting a CTD profile that was observed in the open ocean as inflow conditions under the ice shelf.

- Because sub-ice shelf plume is generated from ice melting near grounding line, we cannot resolve this in our simulation domain. Therefore, we have to use the prescribed profiles of temperature and salinity based on observations. For velocity profile in new simulation, we set theoretical profile of turbulent flow with different turbulence. In additional, we set the inflow conditions using observation under the assumption that temperature, salinity of sub-ice shelf plume in the open ocean is similar with those beneath ice shelf. We will explain this assumption and appropriateness in methodology section in revised manuscript.

Line 149: Please specify how the radiation boundary condition is implemented.

- Radiation boundary condition at outlet boundary is extrapolation boundary condition (gradient of variables are zero) that outlet boundary does not affect the oceanic flow. We will explain this in methodology section in revised manuscript.

Line 150: Further explanation is needed to address how Dirichlet conditions at top boundary are appropriate even under the ice shelf and what they represent in the open ocean. Is this consistent with observed winds?

- Through the AWS observation in Araon, observed wind direction was opposite to upper current direction (negative zonal velocity). It represents that upper current was not developed by winds. Therefore, we set the Dirichlet condition ($v = 0$) at top boundary to exclude the wind effect, because wind effect is out of scope.

Line 153: With what metric is quasi-steady state evaluated?

- We plotted time series of the friction velocity near ice shelf bottom to examine the time convergence. As shown in R-figure 6, it is observed that friction velocity is converged after 14 t^*. Through this feature, we evaluate the quasi-steady state in simulation result.
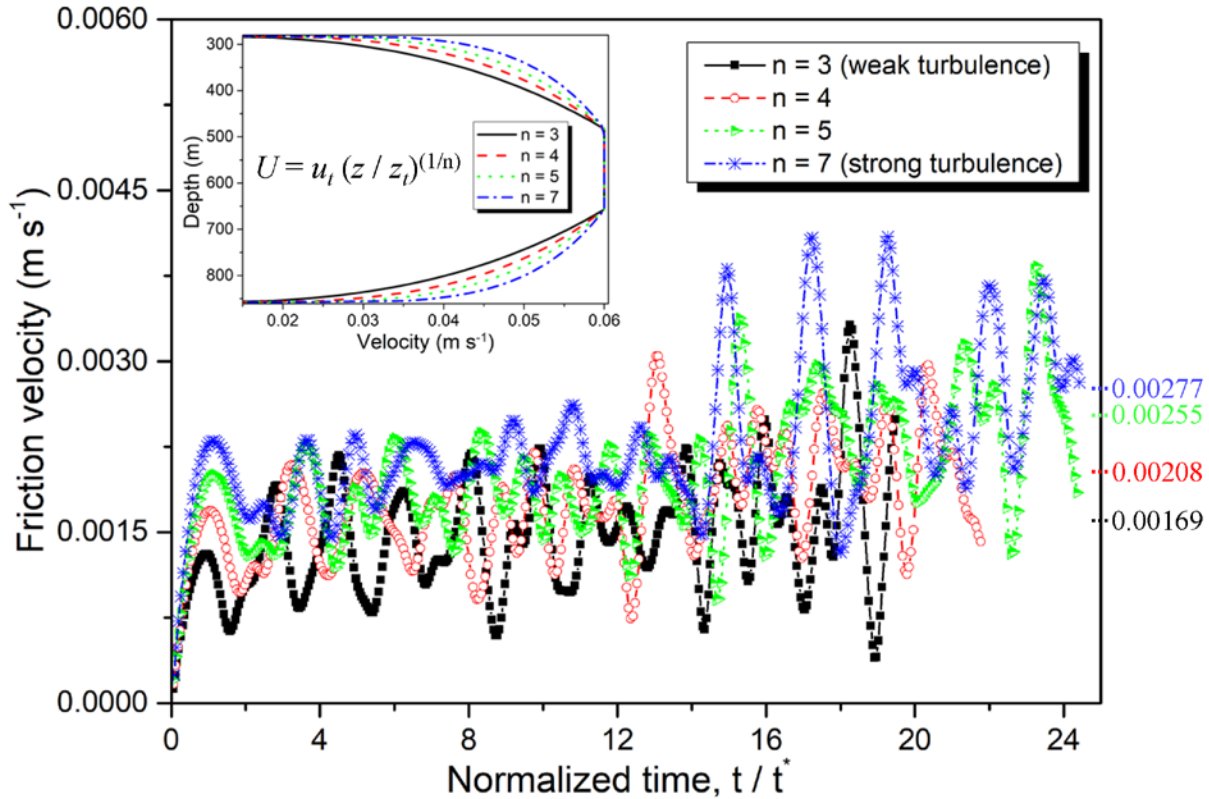
R-Figure 6. Time series of friction velocity in new simulations.

Results: Explain in the refreezing case to what degree the increase in boundary layer temperature is due to the release of latent heat and differences in entrainment.

- In new simulation, we did not consider the release of latent heat at ice shelf bottom. Instead of this, we will discuss the specific degree of changes by ice melting at ice shelf bottom and frazil ice formation at sea surface.

Line 178: "This difference is induced by high momentum exchange by refreezing and its brine rejection." This statement is unclear. Is the high momentum flux related to the destruction of stratification by brine release?

- In new simulation, we did not consider the release of latent heat at ice shelf bottom. This

statement will be removed in revised manuscript.

The horizontal velocity orientation throughout the text is unclear. Are zonal velocities aligned with the x-axis of the simulation domain? In some places in the text the zonal velocity but not the meridional velocity is presented.

- As shown in R-Figure 7, we will present flow structures of velocities with Ekman layer in revised manuscript.
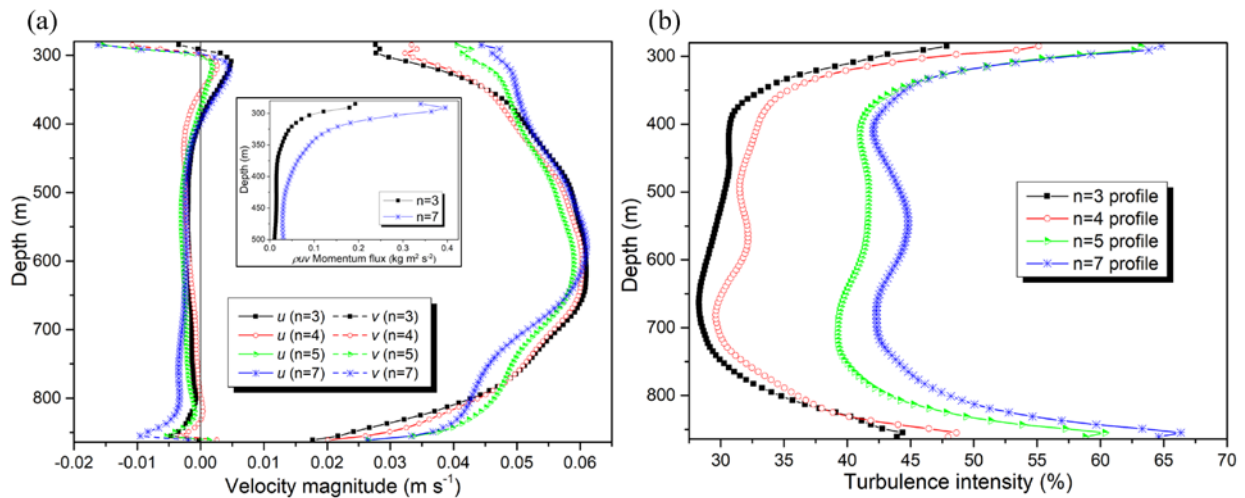
R-Figure 7. u (zonal), v (meridional) velocity profiles of new simulations.

Line 185: "negative mean velocity": the velocity vector orientation is unclear.

- We will provide information for directions in revised manuscript.

Line 215: "stream-wise zonal velocity": the velocity vector orientation is unclear.

- We will provide information for directions in revised manuscript.

Line 186: The description of the forces responsible for the ice front eddy are unclear. Can you also describe the eddy structure more clearly? Is it a singular overturning cell spanning the length of the ice front?

- In new simulation, the overturning cell near ice front was not observed. So, we will remove these contents for the ice front eddy in revised manuscript.

Line 189: Are you saying here that convection due to brine rejection inhibits entrainment? It is unclear why this would be the case.

- In new simulation, this part will be removed.

Line 194: "upward advection from sub-ice shelf plume" is unclear. Are you talking about entrainment due to turbulence or advection by the mean flow?

- In previous results, we talked about entrainment due to turbulence (heat advection by turbulent mixing).

Line 195: "stratification is more dominant than flow shear" You haven't made a strong case for this in Results. Perhaps you could move this statement to the discussion and expand on it there.

- In new simulation, ratio between buoyancy force and shear production will be examined via flux Richardson number flow shear in revised manuscript.

Line 200: "upward flow advection" again, is this mean flow or turbulence?

- In this part in previous simulation, we used "upward flow advection" as mean flow (upward moving of sub-ice shelf plume).

Line 200: "Since there is no downward force due to brine rejection, the upper region of the sub-ice shelf plume expanded to the upward direction immediately after it passed the ice front." It's unclear what you determine the driving mechanism to be. Is it that changes in stratification determine the sub-ice shelf plume extent or changes in the degree of mixing between water masses or something else?

- In new simulation, dynamics and physics were different with previous simulation. So, we will remove this sentence.

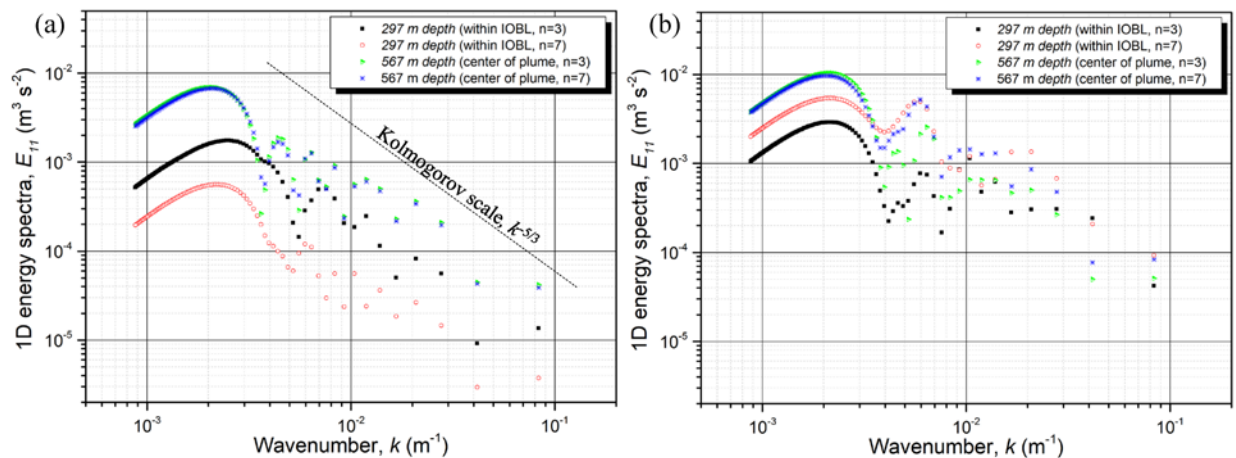Results: The relationship between refreezing and entrainment of supercooled water at the ice shelf front is unclear in the manuscript. How can we tell that the higher rates of refreezing at the ice shelf front are due to entrainment as opposed to reversed flow of cooler water from the open ocean below the ice front?

- In new simulation, it was observed that reversed flow of warm water from open ocean caused ice

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Figure 5. Show the inertial subrange in wavenumber space. You mention that the low wavenumber values don't fit the -5/3 slope but aren't these wavenumbers outside the inertial subrange? Why are these spectra so noisy? Could this indicate insufficient spatial or temporal averaging? You don't specify these details in your description of the methodology. It's also unclear why there are several curves for each case and depth. Furthermore, why are the spectra evaluated in 1-d?

- As the reviewer mentioned, energy spectra in low wavenumbers is outside the inertial subrange. When we obtain this spectra plot, we consider velocity fluctuation of spanwise direction (homogeneous by cyclic boundary condition) at specific depth and specific zonal (streamwise) location, because energy spectra calculation have to be performed under the assumption for homogeneous turbulence. in previous simulation, insufficient spatial averaging caused noisy feature. In new simulation, we will plot the energy spectra as shown in R-Figure 8.

R-Figure 8. 1D Energy spectra in new simulation. These plots are obtained at 297m (IOBL center), 567m depth (Plume center).

Figure 7, 8. There doesn't appear to be a relationship between heat flux at -281 m and refreezing rate. Can you explain why this is the case?

- As the reviewer mentioned, heat flux at -281 m was not related with refreezing pattern (melting pattern in new simulation). Because melting pattern is related with temperature, integral of local heat flux near ice shelf will be related with melting pattern. We will re-plot this figure, including relationship between the melting pattern and vertical structure of heat flux.

Lines 215-231: You address differences in velocity magnitude but not orientation.

- To orientation of oceanic flow, we will add vertical profiles of velocities (x, y directions) in revised manuscript. (R-Figure 7)

Line 230: This caveat should be explored more deeply in the discussion.

- In revised manuscript, this part will be moved to discussion section.

Line 234: "it is shown that the LES model adequately resolves the oceanic flow beneath the ice shelf with the proper refreezing effect." The analysis presented in Section 3.1 does not demonstrate this. You don't show evidence for sufficient resolution (e.g., a comparison of resolved vs. subgrid energy) or the proper refreezing effect (e.g., a combination of the right theory and match to observations).

- In new simulation, we will discuss the model validity with proper melting effect and latent heat and salt by frazil ice formation with our hypothesis.

Line 240: Needs further discussion in text to explain why vertical velocity is not zero given boundary conditions.

- We will discuss this based on Monin-Obukhov similarity at ice surface in revised manuscript.

Line 257: "strong velocity gradient" in what direction?

- It was vertical gradient.

Line 264: the definition of IOBL should come at the first mention of IOBL in the Results or Methods.

- We will add the IOBL definition in the results section.

Figure 11: Is IOBL depth the same for both regions? Inset: Is the arrow for the inset meant to correspond to a certain depth? Is sigma the same for both runs? Are there multiple PDFs for each region overlain, because I was expecting to see a single curve for each region as opposed to the scattered points?

In previous simulation, IOBL depth between inner and outer regions was slightly different (inner: 438 m, outer: 447 m).

Line 292: "This water plume refreezes. . ." this statement is only true for some ice shelves.

- Reviewer's comments is correct. We will amend this sentence.

Line 304: "we used the LES model to expand the one-dimensional observation profile in oceanic region to the three-dimensional flow-field in oceanic region and the sub-ice shelf region." This statement is imprecise and possibly misleading. It would be more accurate to say that you used the observational profile as initial and boundary conditions and then investigated spatial and temporal variability arising from brine rejection and mixing.

- It will be corrected as the reviewer mentioned.

Line 306: "We assumed that the LES results for the sub-ice shelf region are validated if the LES results for the oceanic region are validated." See validation comment above.

- Based on new simulation and its validation, we will suggest this statement in revised manuscript.

Line 307: "Via an evaluation of the refreezing effect" It's not clear what this evaluation consists of and how it supports the validity of the model.

- Based on new simulation and its validation, this part will be changed to "via an evaluation of the melting effect" in revised manuscript.

Line 314: You haven't provided much explanation of the causes of heterogeneous freezing rates and what controls the scale of turbulence features.

- We will remove contents for the freezing rate beneath the ice shelf.

Line 315: what is the scale of the ice front eddy and what controls it?

- In new simulation, reversed flow and ice front eddy were not observed.

Line 316: what determines the IOBL depth and does it match the observations?

- We determined the IOBL depth as the depth at 1% of heat flux at ice shelf bottom. In future study, we will consider this with new observations using AUV, glider.

Line 319: "this study can be improved by comparing LES results with observations and its feedback" What do you mean by "its feedback"?

- "Its feedback" meant the modification of constants or model parameters by the comparison between LES result and in-situ observation. We will rephrase this sentence in revised manuscript.

Line 321: "If a database for flow physics in various parameters is completed" I can see what you're getting at here, but the wording here is awkward and it's unclear what you mean by "a database for flow physics.

- In this sentence, database meant the model parameters (e.g. turbulent exchange coefficients) according to various flow environments. We will rephrase this sentence to clarify original meaning.

Line 328: "convergence trend in temporal variance" of what quantity?

- When we mentioned the temporal variance, we examined time series of the friction velocity.

● Technical comments

Make it clear in the introduction why you use the term "refreezing" as opposed to "freezing" There are a few places in the text where you say that water melts, but I think you mean to say that the water mass has a contribution of meltwater from the ice shelf.

- Because there is no refreezing beneath ice shelf in new simulation, we will not use the "refreezing" in introduction section. Text for water mass will be modified as the reviewer mentioned.

Specify which version of PALM you are using.

- We will add the version of PALM (version 6, r4536) at the first mention of PALM in methodology section in revised manuscript.

Line 17: "high shear impact"?

- We will amend this to high velocity shear in revised manuscript.

Line 46, 184: "the shear impact" is confusing if what you mean is more similar to "the direct impact" and does not relate to velocity shear.

Line 107: Sub-grid parameterizations need a citation.

Line 181: include a citation for the original definition of the swirling strength criterion

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Line 182: "Due to" to "At its"

Line 182: "apart from the ice" to "below the ice"

Line 206: "with few dissipations" to "with little dissipation"

Line 235: "explores" to "explore"

Line 274: "to IOBL the flow"

Line 277: reference for the flatness factor

Line 281: strange to say that the vertical velocity fluctuations "have" 3 sigma.

- We will remove this part in revised manuscript.

Line 302: "the numerical approach including the LES" is confusing. Do you just mean LES on its own?

- Yes. We just meant LES. We will amend this in revised manuscript.

In all figures, specify whether results are derived from simulation or observations or both.

- We will specify this in all figures and its captions.

Figure 1: a. Unclear what blue shading designates. Add "sea" to Ross, Amundsen, Weddell, Bellingshausen labels. b. Show ice shelf boundaries and label sea ice areas. c. label ice shelf length dimension.

- We will add the "sea" to Antarctica map. Also, we will add ice shelf length dimension.

Figure 6. b. Show local freezing point, especially given that you claim supercooled water entrainment.

- We will type the local freezing temperature in explanation for vertical profile of temperature.

Figure 9. I can't tell the sign of vertical velocity without a reference zero line.

- We will add reference zero line at velocity figure.

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.

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 directly from the in-situ observation.

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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. In new simulation, we will discuss our grid size via energy spectra, grid sensitivity study, magnitude comparison in resolved and SGS 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. Also, we can plot the resolved salt flux (R-Figure 4), and stabilized features are observed.
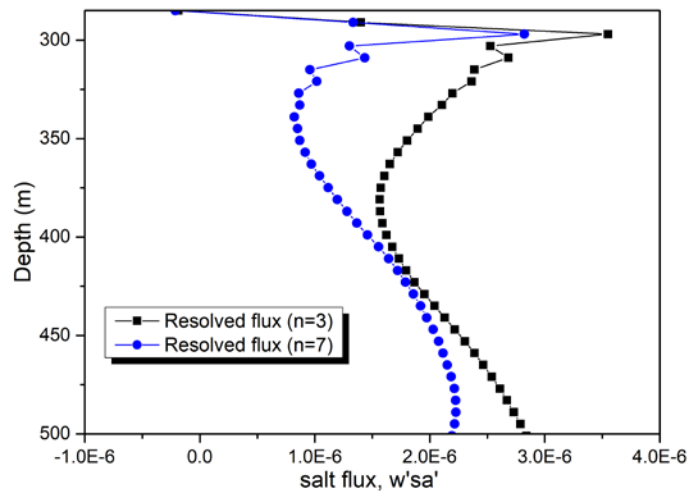
R-Figure 4. Vertical profile of salinity flux in new simulations

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 development of ice front circulation in open ocean based on new simulation results. The circulation is vertical overturning type by salinity flux at the sea surface.

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 xz contour of 3^* time-averaged salinity in new simulation, we cannot observe the density inversion near the sea surface because colormap scale and time averaging (R-Figure 2). Double-diffusive effects (only salt finger) can be observed at initial state (R-Figure 5), but it is not observed in time-averaged salinity contour.
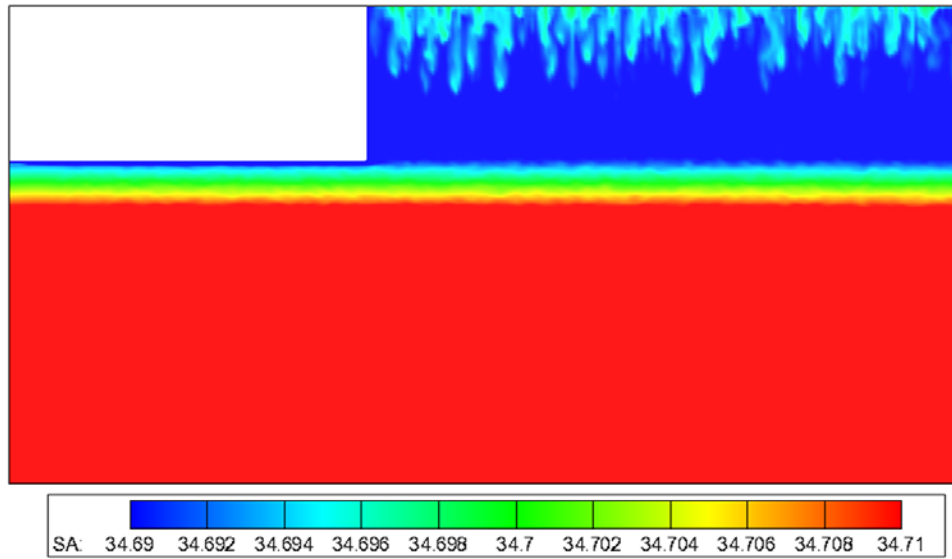
R-Figure 5. xz contour of salinity at initial state (t = 2 hour)

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), v meridional velocity with Ekman layer was observed in new simulations.
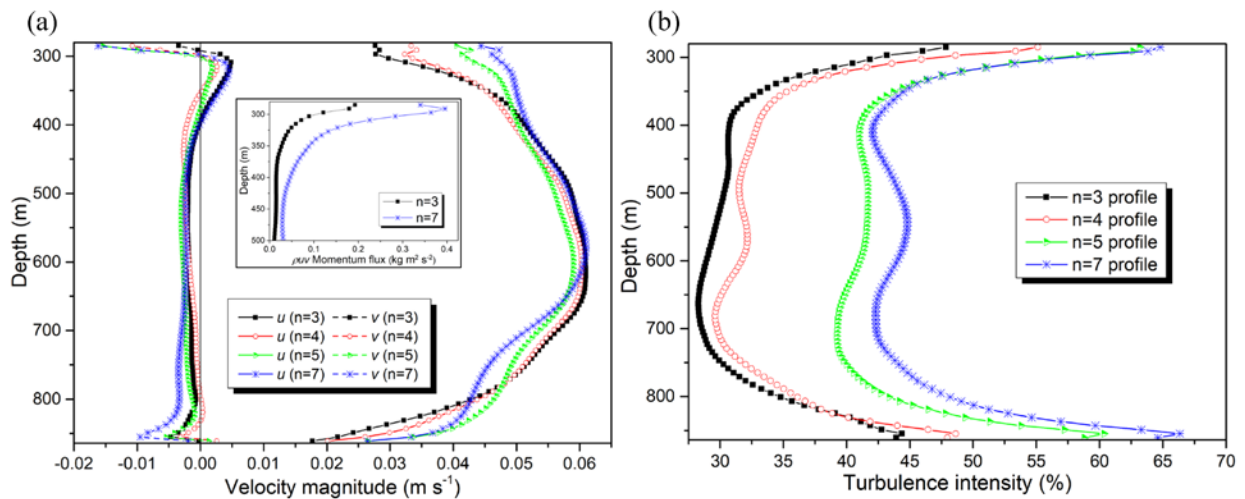
R-Figure 6. Vertical profiles of velocities beneath the ice shelf.

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. Positively buoyant ice shelf water (PISW) in our study has similar feature. 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.

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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 10^{-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 changed 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. Using this model, we can solve fluid dynamics and thermohaline physics within IOBL, but small scale physics within viscous sublayer cannot be solved. This is because we have to use turbulent exchange coefficients from high-resolution study such as DNS, another LES. 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 -1.41×10^{-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, mixing length are important factor for determining the eddy viscosity, and we refer the Maronga et al. (2015) and 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?

- As reviewer mentioned, we add the definition for u*, tau (non-dimensional variable for the relaxation of surface fluxes).

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

- As shown in R-Table 1, we set 0.005m of surface roughness, referring the low drag coefficient, C_d=0.001 case in Gwether et al. (2016).

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 IOBL depth to obtain the large eddy turnover time in IOBL.

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 was induced the vertical fluxes of


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 14 t*

   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)
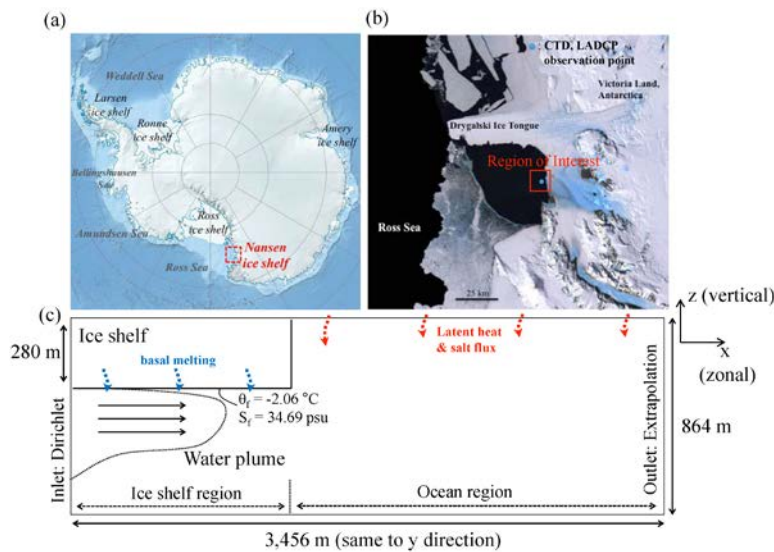
R-Figure 7. Domain information and boundary conditions

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?

- In previous simulation, there was a flow instability and rapid transition of inflow. But, there is no these effects in new simulations.

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

- We remove this analysis 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 use the different 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 will explain more the role of the stratification and stratified force by positively buoyant meltwater.

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

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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.

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, we will use same criterion.

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 was 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?

- In new simulations, we will not consider the flatness factor analysis. To observe the production and energy budget, we will add the flux Richardson number, although it has not a distinct trend in turbulence intensity.

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 in introduction 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

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