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.

Anonymous Referee #1

- General comments

The authors have made some significant improvements on the previous iteration of this article, and I think it could be close to being of publishable quality. My main reservations are still the ways in which this study links to other studies of the ice shelf edge specifically. The paragraph discussing the work of Garabato et al. (2017) and Malyarenko et al. (2018) is good, but these two works should be discussed in detail in your introduction as they are very relevant to the situation you are describing, and I think your work links with those studies very well. You mention that your work agrees with the picture presented by Garabato et al. (2017), whereby centrifugal instability causes turbulent mixing which changes the settling depth of the ice shelf plume. I am not totally convinced that you are seeing this mechanism, so I suggest that you assess the potential instability from your simulations (i.e. look at the vorticity and compare it to the criteria for centrifugal instability). You could also check the Potential Vorticity to see if you are likely to see any symmetric instability which was ruled out by Garabato et al. (2017). You could also show a plot of the spatial variability of the dissipation rate, or the TKE, to demonstrate whether enhanced turbulence is associated with the plume reaching the ice shelf edge. I think you should expand on how these simulations would change if you included winds, with reference to Malyarenko et al. (2018) who discuss this scenario.

- As the reviewer mentioned, we have discussed observational studies on the frontal region in the introduction of the revised manuscript.

- Moreover, we have added the analysis for relative vorticity and the criteria for potential instability in the supplementary material (Figure S6, S7). We can observe the symmetric instability as well as centrifugal instability.

- We have expanded the discussion for the situation with wind effects (for the katabatic wind case).

Modified or added parts:

Introduction:

Observational efforts of meltwater behavior and ocean circulation near the frontal region of the ice shelf demonstrate various mechanisms at different locations around Antarctica. In the frontal region of the Pine island ice shelf, Garabato et al. (2017) revealed that the ascent of the meltwater outflow causes vigorous lateral export, affecting the settling of meltwater at depth. The intrusion of relatively warm surface waters and high basal melting near the ice shelf front was observed in the Ross ice shelf (Hogan et al., 2011). Moreover, Malyarenko et al. (2018) suggested the existence of a “wedge” of fresher water in the Western Ross sea and it is formed from meltwater near the ice shelf front.

Supplementary material:

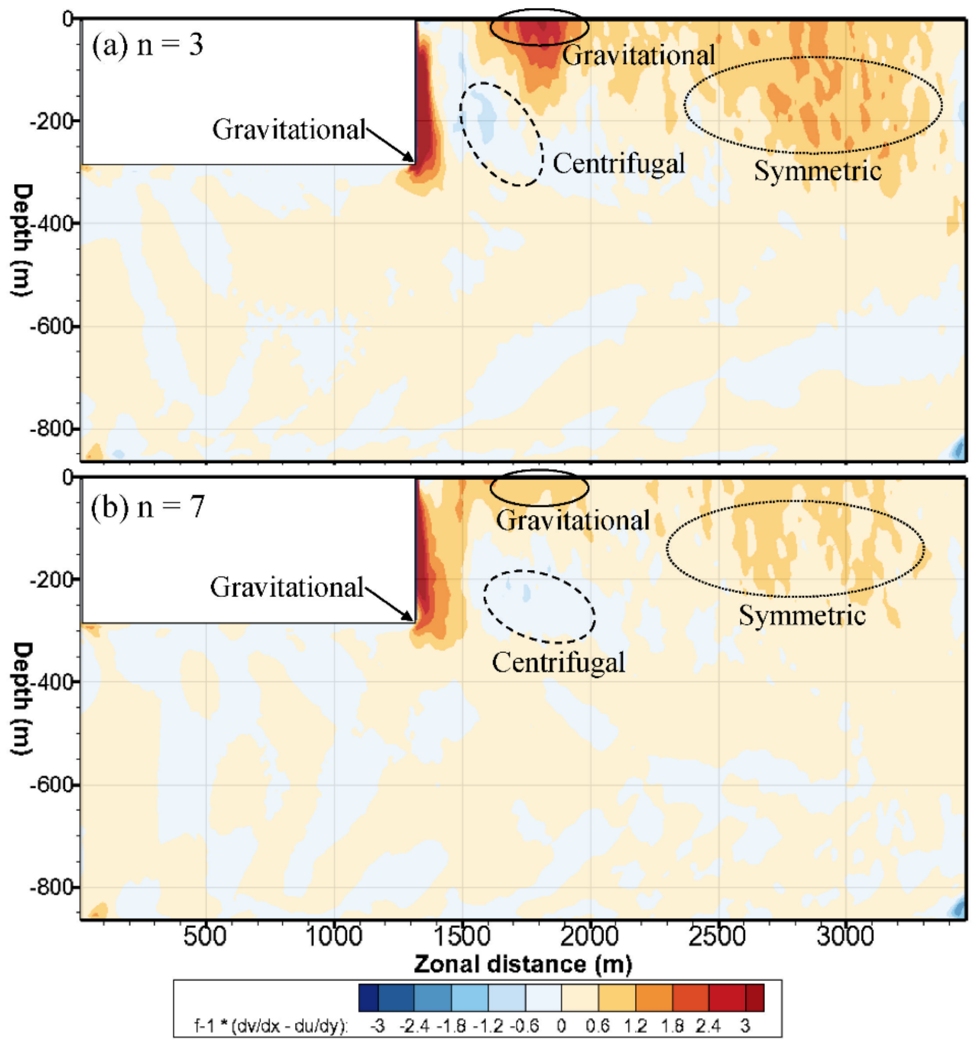

Figure S6. xz contours of z-direction, relative vorticity in the cases with weak turbulence (n=3) and strong turbulence (n=7). Positive values represent the region for the symmetric instability (vertical shear), whereas negative values represent the region for the centrifugal instability (lateral shear) in well-stratified fluid ($N^2 > 0$). Gravitational instability occur at the region of concentrated salt flux by sea ice formation ($N^2 < 0$). PISW upwelling right after the ice front can be classified to gravitational instability (inset profile of Figure S7a).

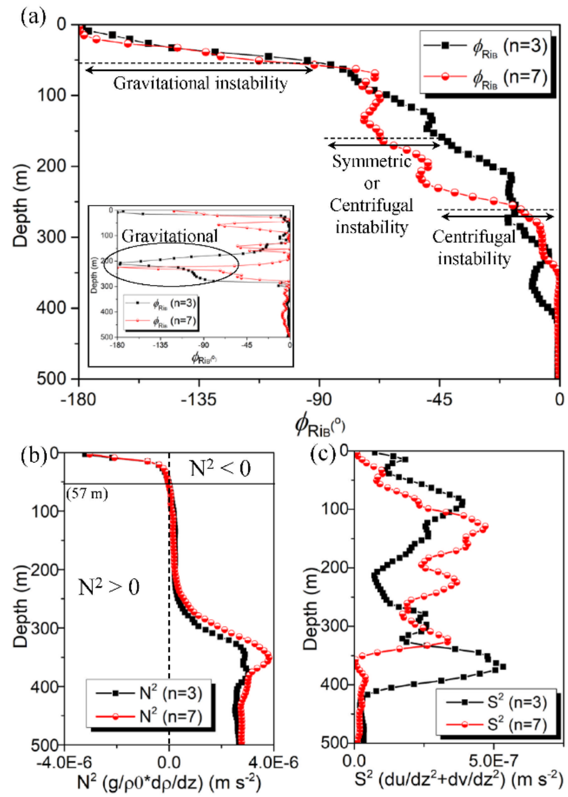

Figure S7. (a) Vertical profiles of angle of balanced Richardson number to identify the type of possible instability in the whole ocean region. Inset figure in (a) is for that in the frontal region from the ice front to 24 m. Vertical profiles of (b) buoyancy and (c) flow shear terms of Richardson number in the cases with weak turbulence (n=3) and strong turbulence (n=7).

Discussion:

Differ to centrifugal instability in the Pine island ice shelf, we can demonstrate the symmetric instability in this study (Figure S6 and S7). This difference is caused by the different directions of the current near the sea surface and the exclusion of the katabatic wind effect.

In Terra Nova Bay, the northeastward katabatic wind is dominant and it drives the along-front current (Guest, 2021; Malyarenko et al., 2019). If we included the wind effect at the sea surface, the horizontal mixing may be enhanced, advecting the fresh meltwater of the PISW layer near the ice front to the open sea. In terms of overturning cells, the strength and horizontal scale of the outer overturning cell may decrease, as the wind stress reduces the shear stress between the sea surface and the sub-ice shelf plume.

I have a series of smaller points which I also think need to be addressed:

4. “Coherent structure of the ocean dynamics near the ice front” needs rephrasing, I think this is not the correct use of the phrase coherent structure

- We have amended this phrase without coherent structure part (line 11-13).

5. Change to "to simulate the varying turbulent intensities"

- We have amended this phrase as the reviewer mentioned (line 13).

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23. "Frazil ice" is misused throughout the manuscript. Frazil ice refers to ice that forms due to the supercooling of water, where the water pressure has increased and so the freezing temperature has increased, so ice begins to form within water that is below its own freezing point. I think you mean to refer to sea-ice formation, or simply generic freezing.

- We have changed "frazil ice" to "sea-ice formation" throughout the manuscript.

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52. "well-mixed feature (20–30 m) of the temperature and salinity induced by a strong tidal forcing and a weak stratification structure was observed" needs rephrasing e.g. "a well-mixed boundary layer was observed in both temperature and salinity, induced by a strong tidal forcing and a weak stratification. A moderate melt rate was observed, despite the low thermal driving, due to the observed shear-driven turbulence."

- We have amended this phrase to clarify what we meant.

Modified phrases (line 51-53):

In the Larsen C ice shelf which is a cold-water cavity, a well-mixed boundary layer (20–30 m) was observed in both temperature and salinity, induced by a strong tidal forcing and a weak stratification. A moderate melt rate was observed, despite the low thermal driving due to the observed shear-driven turbulence (Davis and Nicholls, 2019).

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79. You have not used Monin-Obukhov similarity theory in your set up – just the standard three equation model. Monin-Obukhov theory would give a correction to the dependence of heat and salt flux on the difference between far-field temperatures/salinities and those at the ice base, as discussed in Vreugdenhil et al (2019). You have only included a linear dependence for your heat and salt fluxes, so I would not term that Monin-Obukhov similarity theory.

- We have amended this term to surface fluxes.

Modified phrase (line 84-85):

To include thermohaline effect by the sea-ice formation at sea surface and basal melting at the ice-shelf base, surface fluxes in both temperature and salinity were used.

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166. 1/Gamma_T, 1/Gamma_S, usually these are defined as Gamma_T and Gamma_S, why have you used the inverse?

- As the reviewer mentioned, these nomenclatures were confusing. We have amended these terms without the inverse.

Modified phrases (line 172-173):

Γ_θ and Γ_S are the non-dimensional transfer coefficients of heat and salt, respectively, determined from the near-wall physics.

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270. Your discussion of the 'baroclinic eddy' needs more clarity. I think you could remove the whole paragraph starting from "this PISW layer blocks an intrusion of the outer ocean" to line 274.

- As the reviewer mentioned, the previous discussion of the baroclinic eddy was unclear. We have removed this part and rephrased this.

274. A comparison of the scale of your spatial patterns to the Rossby radius of deformation is useful, but you should first define the Rossby radius of deformation and make it clear how you have calculated it.

- We have added the definition of Rossby radius of deformation and how we have calculate this.

Modified phrases (line 275-280):

However, heterogeneous patterns of the freezing rate are observed in the strong turbulence case, because the PISW layer near the ice front is wide with a weakened inner overturning cell, permitting the larger baroclinic disturbance caused by sloped isopycnals. This heterogeneous pattern of the freezing rate is comparable to the disturbance scale (2,066 m), as identified from the Rossby radius of deformation which represents the length scale the rotation effect is dominant. This scale is obtained by depth-averaged buoyancy frequency and depth between the sea surface and IOBL bottom.

Figure 6. I think you should plot lines rather than a scatter, to better show the power spectra. You should also list in the caption how exactly these spectra were calculated i.e. was it a single point velocity measurement through time? Or was it a spatial assessment of the entire velocity field? The spectra are very noisy, so it's difficult to see the slope – if you average spectra across multiple times, or some spatial average, then you will get smoother spectra which may be more useful.

- These one-dimensional spectra were obtained by a spatial assessment (meridional direction) of the time-averaged velocity field. Through more spatial averaging, we can get smoother spectra that can indicate its slope. We have added the explanation for the calculation process in the caption of the revised manuscript.

Figure 6. Your second plot seems to show shallower slopes than k^(-5/3)? Is this a k^(-1) slope? Please include the slope on this plot. If it is k^(-1) then that could be indicative of a Batchelor style regime rather than an inertial subrange, which would be worth commenting on.

- As the reviewer mentioned, power spectra in the high wavenumber regime are close to the k^{-1} slope, representing the Batchelor regime rather than an inertial subrange. We have added the k^{-1} slope in new Figure 6.

Modified Figure 6:

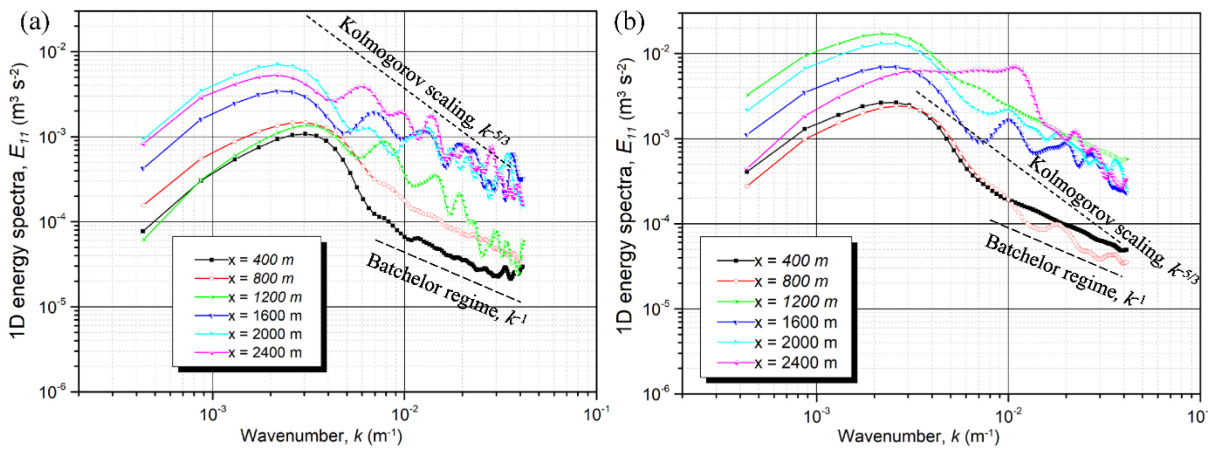

Figure 6: One-dimensional turbulence energy spectra at a depth of 291 m at the PISW within the IOBL. (a) n = 3 and (b) n = 7. Different shapes and colors represent the values at different zonal distances: 400, 800, 1200, 1600, 2000 and 2400 m. These power spectra are obtained by y-direction (meridional direction) spatial assessment of time-averaged velocity at each x location. The –5/3 slope (Kolmogorov scaling) represents the regime of inertial subrange, whereas –1 slope (Batchelor) represents viscous-convective range in high-Schmidt number.

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344. I think the way you are calculating thermal driving is different from the method given in Wray (2019). You have temperatures and salinities taken from ocean observations, so that should not give a significantly different thermal driving. I imagine that the main differences between your simulations and the observations in terms of melt rate is associated with the lack of winds in your simulations, but there are so many idealisations in your simulations that could be the cause of this disparity.

- In the study of Wray (2019), he used the ocean temperature of -1.86 ºC and freezing temperature at 140 m depth to obtain thermal driving (0.14 ºC). Whereas, we used the ocean temperature of -2.06 ºC (sub-ice shelf plume) and freezing temperature at -2.116 ºC at 280 m depth to obtain thermal driving (0.056 ºC). We have demonstrated this difference in ocean temperature was due to intrusion and interaction of surface water due to wind and tidal effects (line 348-350).

Modified phrases:

However, the melt rate in our LES results is comparable to the observed melt rate, considering the difference in the thermal driving (0.056 °C (2.116 °C – 2.06 °C) in our LES simulations and

0.14 °C (2.0 °C – 1.86 °C) in the study of Wray (2019)). In this study, only the effect of sub-ice shelf plume (formed by HSSW) was considered, but the observations included the effects of relatively warm Antarctic surface water and sub-ice shelf plume, resulting in a difference in the thermal driving and melt rates. If the melt rate in this study is assumed to be 0.12 m yr^{-1} (averaged value of 0.092 and 0.153), we can estimate that 12–25 % of the total basal melting near the NIS front is due to sub-ice shelf plumes. The rest portion is related to surface water intrusion with wind stress and tide effects.