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

I appreciate that the authors have made significant additions to the text since the last round. I have indicated major revisions because there are still many aspects of this work that remain unclear. Some of this needed clarification arose due to additions made since last round, and some questions remain from last round so I have tried to be more specific in my comments. My comments this round pertain mostly to the science rather than language. I acknowledge that the authors are likely non-native English speakers. Unfortunately, non-standard grammar and awkwardness of some of the sentences will deter some readers and in cases will contribute to confusion about the science (as they did for me), and consequently reduce the impact of the article. I recommend getting a careful read-through for grammar and awkwardness before the next round.

- In this revision round, we have double-checked the grammar and awkwardness with additional English correction service.

Given the edits to the manuscript, some reorganization is needed. For instance, much more than validation happens in Section 3.1. The paragraphs should be split in several places to divide distinct topics, as I've noted in my comments. Some places also lack adequate transitions between topics.

- Previous section 3.1 have been split in two sections (3.1 Quasi-steady, ocean environment near the ice front, 3.2 Validation of simulation results). Moreover, we have modified several places to clarify the transitions between topics.

It's clear to me now what the definition of PISW is. However, I do believe that the definition of IOBL will be misleading. I would have expected that the IOBL would include all of the PISW thickness and that the 5% heat flux definition would have been used for IOBL bottom and not its top. It's not clear to me in what sense that would be considered a boundary layer (if not related somehow to shear at the boundary) or what the bottom of the IOBL is. I might have missed it, but I don't think it's stated explicitly what you mean by sub-shelf plume. My best guess was the full-cavity depth outflow. If that's right, I think that would also be confusing for readers, as plume usually denotes flow driven by buoyancy whereas this seems to be a geostrophic flow. If you decide to keep this notation, it should be clear what these terms mean.

- As the reviewer mentioned, 5% heat flux definition have been used for IOBL bottom (not IOBL top). In this study, we defined the IOBL as the boundary layer where PISW (not sub shelf plume) and its thermodynamic impacts are dominant. In the revised manuscript, we have explained this explicitly.

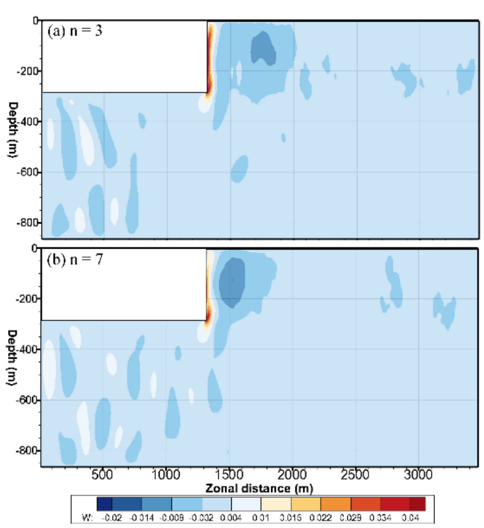
There are a few instances where the choice of terminology could be improved. I'm not thrilled with the choice to call the different initial and boundary conditions different "turbulent states" as that seems to imply that there is a transition in the mode of turbulence or the kinds of instabilities that occur. There are also instances where the authors use a "downward force" framing, which doesn't reveal what specifically is influencing the relative buoyancy of water masses. See also a later comment about more specific terminology for "ice front circulations."

- As the reviewer mentioned, those terms were not clear. To clarify what we explained, we have amended those terms in revised manuscript. (1. turbulent states -> turbulence intensity, 2. downward force -> downwelling, and 3. ice front circulation -> inner and outer overturning cells)

Some aspects of the ice-shelf front circulation remain unclear. Are there convective downwellings throughout the outer cell or only at the convergence of the two cells? You show the horizontal velocity but there is no way for the readers to gauge the strength of these circulatory cells. A streamfunction plot would be the most clear but a vertical velocity plot might work if it's easily interpretable.

- Although weak downwelling motions by salt flux are throughout sea surface, distinct feature of downwelling is observed at the convergence of the two cells. To show this, we have added the vertical velocity contours in weak (n=3) and strong turbulence (n=7) cases to supplement material.

*New supplement material (Figure S5)



Furthermore, it's unclear in the text why you think that the outer circulation should be a cell. Why would there be upward velocity at the outermost edge of the domain given that there is a radiation boundary condition? What would set the size of this cell in the real world? (I appreciate that the authors added more discussion of length scales with respect to the ice-front circulation.)

- In this study, we have observed the negative velocity (toward ice front) at sea surface, downwelling motion at the convergence of the two cells, and positive velocity at 300 m depth. Through these values, we speculated that there is outer overturning cell. Because this overturning cell is caused by highly shear flow (sub-ice shelf plume), it is similar to reattachment flow (backward facing step) near the geometry. According to Rygg et al. (2011), reattachment length in highly turbulent (Reynolds number > 2.0×10^6) flow of ocean is approximately 7-10 (secondary cell (inner) ~ 1.3–1.5, primary cell (outer) ~ 5.3–8.4) of geometry height. Because ice shelf thickness was 280 m and Reynolds number in this study was approximately 9.0×10^6, we can speculate 364–420 m of inner overturning cell and 1,484–2,352 m of outer overturning cell. In the revised manuscript, we have discussed the length scales of overturning cells.

I'd like to see you explain more thoroughly how these simulations relate to the real world a bit better given that understanding observations is given as the main motivation. Are your simulation conditions specific to a season? I assume the observations are from summer, so is there sea-ice freezing here year-round? How does your choice of SST of -1.9 degC relate to observations and season? If there were these two circulation cells, wouldn't you see a signature in sea ice advection including convergence and ridging or do you not expect these features to be long-lived or to migrate? Besides oceanographic observations, what might validation look like? I appreciate that the authors added a citation to basal melting observations but they say it's from a channel. Are there no satellite estimates of basal melt on broader scales? Was Nansen not included in the Adusumilli et al. 2020 dataset https://doi.org/10.6075/J04Q7SHT?

- As the reviewer mentioned, we have added the explanation for Nansen ice shelf and ocean environment in 2.1 section (observation part) of the revised manuscript. Unfortunately, we cannot find distinct features of sea ice advection to clarify the existence of overturning cells. We have compared the simulated basal melts with satellite estimate of basal melt (Adusumilli et al., 2020, Dow et al., 2021).

*Modified part in the revised manuscript:

The melt rates obtained in this study are significantly low compared to those reported by Wray (2019) (0.45–0.95 m yr^{-1}) and estimated via the Cryosat-2 satellite observation during 2010–2018 (1 ± 0.6 m yr^{-1}) at the NIS ice front region (Adusumilli et al., 2020). However, the melt rate in our LES results is comparable to the observed melt rate, considering the difference in the thermal driving (0.032 °C in our LES simulations and 0.14 °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.

I appreciate that the authors added some text pertaining to wind stress forcing, but I do believe that there should be more discussion of how this might affect the strength of the inner overturning cell since that is one of the main results of the paper.

- Because wind stress by katabatic wind reduced the shear stress between the sea surface and sub-ice shelf plume, strength, and horizontal scale of the outer (primary) overturning cell may be decreased if there is wind stress. The weakened outer cell weakens the inner (secondary) cell. However, the strength and scale of the inner overturning cell may be similar to this study because wind stress at the sea surface imposes at the upper region of the inner overturning cell. To sum up, if the wind stress effect is included, we speculate the similar scale of the inner overturning cell and decreased scale of the outer overturning cell. In the revised manuscript, we have added the discussion for wind stress and its impact on two overturning cells.

I appreciate the caveat the authors added for frazil ice dynamics, but I strongly feel that there should be more discussion (at least a few sentences) of how inclusion of frazil dynamics might change your key findings. Unless it is the case that you don't get pockets of supercooled water, in which case that should be stated.

- As the reviewer mentioned, frazil dynamics and its process highly affect plume characteristics and ocean circulation. We have discussed more about how inclusion of frazil dynamic in PISW might change inner overturning cell.

The main remaining gap in the Methods is the determination of heat and salt transfer coefficients. This is crucial to the interpretation of freezing and melting rates. It should also be made more clear in Section 2.2 why you choose the initial and inflow velocity profile you do and how it varies with depth in and outside of the cavity.

- We have added the detailed explanation for heat and salt transfer coefficients, and velocity profile of initial & inflow boundary condition.

I don't like to harp on this, but I'm still confused by the flux profiles shown in Figure 9. The sub-grid fluxes should increase at the boundary because the turbulent length scales decrease near the boundary. Instead it looks like both resolved and sub-grid fluxes go to 0 at the boundary. If I just can't see the values, perhaps an inset for the PISW would be helpful. Figure S4 also appears to show 0 buoyancy fluxes at the boundary.

- As the reviewer mentioned, SGS fluxes should increase and resolved fluxes should decrease at the boundary (go to 0 at boundary) because turbulent length scales decrease. In the previous plot, we had a mistake for the vertical dimension. SGS momentum flux has to be shifted to 1 upper point of vertical grid.

11: It is unclear how imposing a velocity profile is related to different turbulent states

- We have amended this phrase to clarify the relationship between velocity profile and turbulence intensity.

12: The flow of the abstract needs improvement. “To resolve” and “to simulate” make it unclear how these objectives are related.

- To clarify what we meant, we have amended abstract.

14: State more clearly which properties are used to validate findings.

- We have amended these phrases to clarify what we focused on validation.

16: It is unclear what the distinct features are.

- We have amended this phrase to clarify what are distinct features.

18: It is unclear how this sentence is related to the previous sentence.

- In the revised manuscript, we have removed this.

19: Writing needs improvement here.

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

31: I think you mean shear generated by tides.

- As the reviewer mentioned, we have amended this phrase.

32: “thermohaline process” is always unclear to me. I’d prefer that you specify melting or thermohaline stratification, mixing, or something else.

- We have amended this phrase (thermohaline process -> brine rejection).

36: I wouldn’t say that these processes make it difficult to investigate, just a complex problem.

- As the reviewer mentioned, we have amended this phrase.

58-60: Improve the logical flow of these sentences

- As the reviewer mentioned, we have amended these phrases to improve the logic.

61: More realistic than what?

- This phrase was not clear. We have removed the part for realistic boundary condition in the previous phrase.

67: Citations needed.

- We have amended this phrase with additional reference.

74: I think you mean that the melting/freezing boundary condition is the same but the way you’ve written it makes it sound as if you impose the same melt/freeze rate in all runs.

- As the reviewer mentioned, this phrase was not clear. We have removed this phrase and added the additional phrase for simulation set up and objective.

74: Keep the language consistent. I prefer that you stick with “turbulence intensity” rather than switching to “turbulence state” for clarity.

- In the revised manuscript, we have amended all “turbulent state” to “turbulence intensity”.

86: Only mentioning “refreezing” here implies there isn’t melting

- Instead of refreezing, we have added frazil ice formation and basal melting.

110: It would be best to specify in the text what H_k, S_k, K_h, K_m are. I might have missed it, but there should also be an explanation for the primes.

- We have added the explanation of these terms and primes.

114: Specify z here.

- We have added the definition of z (distance from the wall).

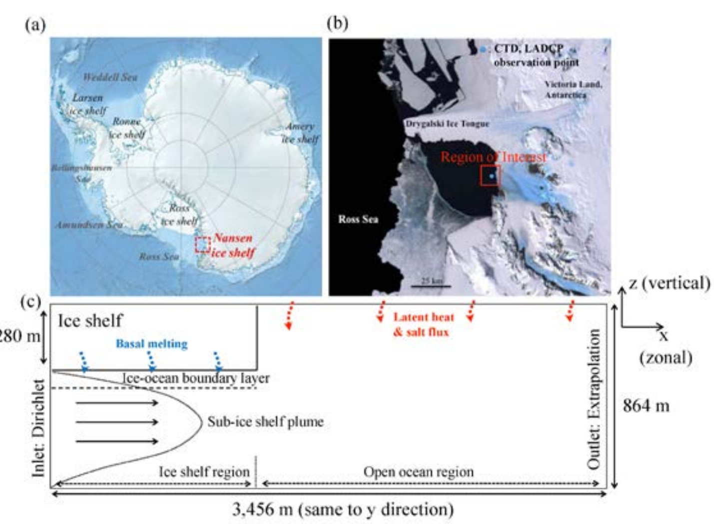
115: Relate the filter length to the model resolution.

- We have added the definition of filter length.

Figure 1. It think it would be helpful if the velocity profile looked more like the initial or mean profile. Can you label IOBL and sub-ice plume separately?

- As the reviewer's comments, we have amended Figure 1

*Modified Figure 1



128: This is confusing since wind stresses are 0 in your simulations, right? Also, the friction velocity should be determined based on the difference in velocity of air and water at the surface. I assume you've done this but it should be stated.

- As the reviewer's comments, we have amended this phrase.

*Modified phrase:

We used 0.026 m s^{-1} of friction velocity in the calculation of thermal and salinity change by frazil ice formation, although the effect of wind stress in momentum change was excluded to focus the relationship between sub-ice shelf plume and the development of ocean circulation.

136: It's unclear how the transfer coefficients are determined.

- As the reviewer's comments, we have added the explanation for transfer coefficients.

*Added phrases:

Based on high resolution LES study for heat and salt transfer coefficients which were described as a function of friction velocity and thermal driving, $1/\Gamma_\theta$ and $1/\Gamma_S$ at the ice shelf base were 8×10^{-3} and 2.6×10^{-4} for the basal melting, respectively (Vreugdenhil and Taylor, 2019). For the frazil ice formation at the sea surface, the same coefficients of the previous study of sea ice formation in polynyas were used because thermal driving in this study was comparable to its thermal driving (Heorton et al., 2017).

139: It's hard to believe that melting is negligible unless thermal driving is extremely low since IOBL is rapidly rising along the front.

- In this phrase, we have removed the part (melting is negligible). We have discussed more melting or freezing effects at lateral side of the ice front in discussion section.

140: I don't see the sea surface boundary condition described in this section. The scalar flux boundary condition is in Figure 1 but it should be stated here as well. Is the velocity boundary condition no stress/free-stream? I think it would be helpful to readers to explicitly state that wind affects scalar fluxes but not momentum fluxes.

- As the reviewer's comments, we have amended this phrase.

145: cite Figure 2

- As the reviewer's comments, we have mentioned Figure 2.

145: Mention that the profile is vertically symmetric and z is the distance from the boundary, justify that choice in relation to observations and mention the depth interval over which it's applied. It's unclear here what the initial velocity is above the ice shelf base.

- As the reviewer's comments, we have amended this phrase and added the explanation of initial velocity above the ice shelf base and no wind stress at the sea surface.

145: Discuss this choice in relation to ice-shelf cavity overturning circulation. Is there reason to believe that the baroclinic velocity is small under Nansen? If the IOBL does rise along the ice front, then the velocity structure due to the IOBL will not be reflected in the profile you implement.

145: Generating momentum through an inflow boundary condition may affect the flow differently than imposing geostrophic flow through pressure gradients. I'd like to see you at least acknowledge possible limitations.

- Because we do not know velocity structure and profile within IOBL and beneath ice-shelf cavity, we have imposed the theoretical velocity profile with parameterized melting effect. The flow started from the inflow boundary condition transited and modified under imposed forcings. Owing to issues mentioned by reviewer, we excluded the developing region in analysis of analysis. We have mentioned this limitation.

197: When after 14h is the averaging occurring?

- Last 3 t* period in all cases. We have amended this phrase.

198: This paragraph could be broken into several. I recommend describing the circulation and PISW generally, then discussing differences between the cases.

- As the reviewer mentioned, we have separated this paragraph.

198: In this paragraph there should be more references to panels within Figure 3.

- In the revised manuscript, we have added the references for Figures.

199: Use "end-member" instead of "extreme" unless you believe those n values are extreme in the sense that they are unlikely to be realized in the real world.

- As the reviewer's comments, we have amended this term.

202: Would "two overturning cells" be an appropriate description? "Ice-front circulations" and "ocean circulations" are quite general and it's strange to have circulation in the plural.

- We think that overturning cells are appropriate. As the reviewer's comments, we have amended these terms in the revised manuscript.

203: You might want to reconsider where it makes most sense to present melting and freezing fields. You mention sea ice formation without presenting Figure 6.

- As the reviewer's comments, we have amended the structure of manuscript.

205: Might be helpful to the readers to specify that the downward force due to salt flux is the negative buoyancy flux.

- As the reviewer's comments, we have amended this phrase.

207: Stratification line is unclear. Can you provide the pycnocline contour or something similar in Figure 3?

- In this contour, stratification line and its down were not observed. We have removed this phrase and discuss this at vertical profiles.

210: To me, this indicates that your simulation results are sensitive to the inflow boundary condition, even far from that inflow condition. This should be clearly stated and why this is the case should be discussed a bit more.

- In discussion, we have added the limitation of prescribed inflow condition.

214: The sentence beginning "Positive buoyancy" is a rough transition from the previous sentence. I was expecting you to elaborate on the magnitude and scale of those circulatory cells next. PISW would be better introduced in a new paragraph

- As the reviewer mentioned, we have modified the structure of manuscript.

214: Rather than the arrow for PISW shown in Figure 3, can you contour this water mass on all panels? I imagine it's mostly the blue contour in salinity but the reader would need density in order to understand which regions are positively buoyant. I would also appreciate a contour for the IOBL.

- To contour this water mass (PISW), we have added the potential density (isopycnal lines)

217: Cite Figure 7

- As the reviewer's comments, we have mentioned related Figure.

218: Outer ocean is a confusing term. Do you just mean open ocean?

- We have modified this term (outer ocean -> open ocean)

219: rephrase to avoid "-2 degC of potential temperature." It's unclear how this detail is important. It would be more meaningful in relation to the surface freezing point.

- We have amended this phrase to emphasize the lower temperature than surface freezing temperature and vigorous frazil ice formation.

221: Sentence beginning "Below 400m" and the following sentence together are unclear.

- To clarify the stratification feature and change of isopycnal line, we have amended these phrases.

223: This paragraph could also be broken into several.

- As the reviewer mentioned, previous paragraph was broken into three parts (observation interpretation, circulation and velocity, and different turbulence intensities) in the revised manuscript.

225: It's unclear at this point how varying the velocity profiles helps you evaluate the cause of the ice-front circulation patterns.

- In the revised manuscript, we have removed this phrase and added paragraph for the interpretation of observations.

227: I think you need to be more clear about what exactly the CTD, ADCP data show. Here the reader might think that the data suggest the circulation cells you simulate, but I don't think they establish those cells.

- In the revised manuscript, we have added paragraph for the interpretation of observations.

230: It's more clear in the text to use directions (toward or away from ice-shelf front) rather than positive/negative. And what is this difference (stronger in observations or simulations)?

- As the reviewer's comments, we have added the directions and the phrase for this difference.

231: “The difference is from...” How do you know this? How can you exclude wind forcing as the reason? By downward force, do you mean that the freezing rates are stronger/weaker or that the water mass properties are different and thus the relative buoyancy is different?

- As the reviewer mentioned, wind stress also a candidate for strength of overturning cell. In discussion section, we have added the phrase for the cause of underestimated strength of overturning cells.

Figure 4: Explain why simulated velocity profiles look so similar. Also, it’s unclear

- We have added the explanation of similar velocity profiles in all cases.

233: Explain why the differences in ML temperature and salinity can be attributed to differences in ice-shelf melting from observations rather than differences in PISW dynamics.

- Based on contour and plot of frazil ice formation, total temperature in upper mixed layer of the open ocean in strong turbulence case is lower than that in weak turbulence case. We have amended this phrase to clarify this.

240: Here I think you’re arguing that your LES results explain a feature in the observations. This should be stated explicitly. However, if this is the cause then this process has a weaker effect in the simulation than in reality because the pycnocline is not depressed enough. Do you need these large-scale circulation cells to explain the observations or would regular small-scale convective mixing from sea ice formation suffice?

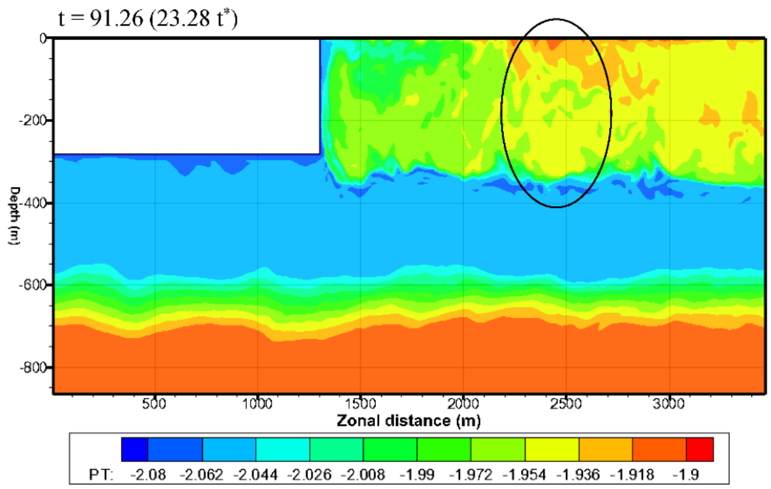
Figure 4 makes it clear that the simulated thermocline is too weak relative to observations. This should be discussed.

- The pycnocline of LES results is similar to observation as shown in vertical profiles of salinity. It means that downwelling of salinity flux is enough in the LES simulation. However, weak simulated thermocline near the 350 m was observed. This is caused by weak strength of large-scale overturning cells. In the revised manuscript, we have amended the additional discussion for this.

241: If PISW upwelling can explain the temperature and salinity excursions in the mixed layer, then why don’t we see it in Figure 4? I imagine this could be because the simulated profiles are time-averaged. Do you ever see such features in the instantaneous profiles? If not, why might this be?

- R-Figure 1 is the instantaneous contour of potential temperature at t = 91.26 (23.28 t^*). We can observe the PISW advection (excursion of temperature and salinity) to open ocean region.

R-Figure 1.



241: “the comparison of quantity and its characteristics” this is too vague.

- We have removed this part and added the detailed explanation of comparison.

250: This wavenumber seems too precise

- We have amended the wavenumber range.

Figure 5 would be easier to interpret if the points were colored on a linear scale with distance from the inlet boundary

- We re-plot turbulence energy spectra with linear scale of distance from the inlet boundary.

251: To say that these spectra follow a -5/3 slope feels like a stretch. Also, are you analyzing wavenumbers that are too large here for your resolution? It would be helpful to have marked the transition between resolved and sub-grid turbulence.

- With new figure for energy spectra, we have amended this explanation with grid resolution and turbulence scale.

255: How do these factors suppress turbulence?

- In the revised manuscript, we have removed this phrase.

270: What is the origin of these baroclinic eddies? Are they convective instabilities associated with salt flux or something else?

- This baroclinic eddies are caused by the gradient of meridional velocity and density difference between local salt maximum and PISW. We have added this explanation.

280: I think it would be better to say "caused by melting"

- As the reviewer mentioned, we have amended this phrase.

290-294: I found these sentences confusing. Do we only know melt rates in a basal channel? How can your thermal driving values be so different from observations?

- In this study, we only consider the effect of sub-ice shelf plume, whereas observations include the effects of Antarctic surface water and sub-ice shelf plume. In the revised manuscript, we have amended these phrases to clarify the difference between LES results and observations.

Figure 8: it's hard to tell the difference between u and v lines.

- In the revised manuscript, we have amended this figure with another shape of v.

296: This doesn't look like a high-speed current to me.

- In this part, we have amended this phrase without 'high'.

Figure 9: The Ekman length scale looks super large.

- Ekman length can be identified in meridional velocity profile of Figure 8. This large scale of momentum fluxes represents effects of sub-ice shelf plume and Ekman layer.

Figure S2: These differences in the Ro radius don't appear to be large enough to explain differences in flow heterogeneity.

- Main parameter for heterogeneity of freezing is the PISW layer and inner overturning cell near the ice front. We have amended this phrase for Figure S2 to clarify this.

310: this makes it sound like buoyancy and momentum are directly related rather than indirectly related.

- This phrase was not clear. We have amended this to clarity what we meant.

312: It's unclear to me how you determine the PISW top and why it wouldn't be coincident with the ice base.

313: I was expecting the 5% heat flux to determine the IOBL bottom.

- In the previous manuscript, the notation for IOBL top, PISW top was confusing. It is IOBL bottom and PISW bottom, not the top. We have amended these terms.

325: I would have expected you to bring your study into this paragraph. You have a HSSW-dominated regime and yet only ice-shelf melting and not freezing.

325-344: I think you spend too much time reiterating topics already laid out in the introduction.

- In the revised manuscript, we have focused on the interpretation of our results and its discussion with various limitations.

345: oceanic region >> open ocean?

- As the reviewer mentioned, we have amended this phrase.

346: the way this is worded it makes it sound like the far-field values are derived from the interfacial values rather than vice versa

- As the reviewer mentioned, this phrase was unclear. We have amended this phrase.

348: what do you mean by trend here?

- We have amended this phrase with specific physics.

353: "change in plume characteristics" change with what? Maybe just that inclusion of frazil dynamics changes plume characteristics, which requires rephrasing this sentence.

- We have added the paragraph for the relationship between the inclusion of frazil ice dynamic and plume change.

358: I would think that heat and salt exchange would be less vigorous in the stably stratified IOBL than in a convecting ML. I imagine that some readers would be thinking the same and it would be helpful to address why this is the case.

- We have removed this phrase.

360: I don’t understand how you get circulation cells over the same depths that the velocity is set to 0. I don’t remember this from the Methods either. Is this text correct? Is this only an initial condition, because here it sounds like it’s fixed for all time?

- Our description for top boundary was incorrect. We set zero velocity for initial condition and impose Neumann boundary condition (gradient is zero) for the momentum at top boundary. We have amended this explanation in methodology part.

364: Again, I don’t see evidence for high speed currents

- We change this to “relatively high speed current”.

367: “Assumed” >> “calculated”

- As the reviewer mentioned, we have amended this phrase.

368: “This denotes that…” means that the location of the salt maximum would indicate driving forces and I don’t think you intend to say that.

- We have removed this phrase.

370: The wedge mechanism needs more introduction.

- We have added the introduction for freshwater wedge and additional discussion.

371: I don’t think that the existence of an Ekman layer needs to be brought up in the discussion, as it is expected.

- We agree with reviewer's comment. We have removed the part for Ekman layer.

376: It's unclear whether the baroclinic eddies mentioned here are the same or different (i.e., in scale) from the baroclinic eddies at the Rossby radius.

- Because the comparison of baroclinic eddy is difficult, we have removed the part for this.

378: This paragraph needs to be split. You go from talking about Ri_f to a list of model limitations.

- We have amended whole discussion section with specific topics. We have moved Ri_f part in model limitations.

379: I don't know what you mean by "turbulence" here (what features?) or where in the column you're computing the Ri_f and stratification.

- In this part, we meant the relationship between stratification and turbulent mixing. We have amended this phrase to clarify this.

381: It's unclear what you mean by negative feedback here, though I assume you're talking about the degree of stratification generated by enhanced buoyancy fluxes. It should be clarified for the reader.

- As the reviewer mentioned, we have amended this phrase to clarify what we meant.

384: "This limitation can be solved using a dynamic SGS model" implies that you didn't use a dynamic SGS model. This sentence should be edited.

- We have removed this phrase and added the phrase for investigation of model constants for near wall physics.

385: What is the "claimed mechanism"?

- We have removed this term.

385: When you say "fill the gaps" I'm picturing a physics-informed interpolation or state estimate whereas here it's more of an extrapolation from one point at the ice-shelf front into the cavity. Please rephrase.

- As the reviewer mentioned, we have amended this phrase to clarify what we meant.

387: I appreciate the clarification in the parenthetical. However, it is still awkward to say "and their feedback."

- As the reviewer mentioned, we have amended this phrase to clarify what we meant.

397: inlet >> inflow boundary condition

- As the reviewer mentioned, we have amended this phrase.

399: "fluctuation" is ambiguous because it could be turbulent fluctuations or fluctuations in the mean flow.

- As the reviewer mentioned, we have amended this phrase (fluctuation -> variance).

403: "agree well" I think this sentence should acknowledge the primary misfit(s) with observations.

- In paragraph for Figure 5, we have discussed primary misfits between observation and LES results.

412: This sentence is unclear to me. The 4 turbulence cases should have different levels of shear yet similar stratification so how does this indicate that "the stratified forcing by PISW varies according to the flow shear caused by turbulence"?

- We have removed this phrase.

413: What exactly should be investigated further? Controls on stratification of the IOBL?

- We have removed this phrase.

Anonymous Referee #2

General comments:

This paper reports on large-eddy simulations of an ice shelf-edge region, inspired by observations made in front of Nansen Ice Shelf, Antarctica. The simulations are run with an idealised geometry and boundary/initial conditions for temperature, salinity and velocity that are inspired by observations. The velocity boundary condition beneath the ice shelf is varied to simulate four different regimes with varying levels of turbulence. The velocity profile between the ice shelf and the continental shelf below is modelled using a power-law velocity profile, where the power-law relationship is varied to simulate varying degrees of turbulence. A three-equation model is used to model the basal melting of the ice shelf, where changes in ice shelf geometry, volume flux input and lateral ice shelf melt are excluded. In the open- ocean portion of the simulation domain, a rigid sea-ice lid is assumed, where the freezing rates are calculated using a three-equation model. Constant heat and salt exchange coefficients are used for the sea-ice region and varying coefficients, calculated using Monin-Obukhov similarity theory, are used in the ice shelf region (this needs to be clarified, as I may have got this wrong?). The high turbulence simulations show an increased basal melt rate and a stronger Ekman layer, resulting in a modified circulation pattern in the open-ocean region.

- All your summary is correct, except a rigid sea-ice lid. Boundary condition and explanation in previous manuscript was not clear. We have amended this part to clarify this.

I found these simulations to be interesting and I think this paper is of interest to the community. Some changes and clarifications need to be made however before I would be comfortable with publication. Specifically, I think further reference to the existing literature needs to be made. I think the ice front region is a particularly interesting region, not least because it is relatively easy to observe compared with the grounding line. Your simulations are relatively idealised, but I think you should still be able to link your work to previous work in the ice shelf front region, even if the conclusion is that elements of your simulations make comparison difficult. Most importantly, the reader needs to be left with an understanding of what the implications of your simulations are for studies of the ice shelf region. In Garabato et al. (2017), they make the point that the intrusion depth of the ice shelf plume has important ramifications for the effect of ice shelf melt on the Southern Ocean (specifically in simulations). I think motivating your study with this scientific question (or some other broad question) would help clarify the point of your work to the reader.

I have a series of other clarifications and edits to the text that I would like to see which are listed below:

1. It needs to be clear that you are studying an ice front throughout the abstract and the introduction. The geometry of your situation is key to the physics of interest, so must avoid trying to say too much about generic IOBL plumes. i.e. I would revise the sentence in the abstract:

"In this study, we utilize a large-eddy simulation to investigate the role of the turbulence within the IOBL flow with sub-ice shelf plume"

To something involving explicitly focused on ocean dynamics at the edge of an ice shelf.

- As the reviewer mentioned, we have focused on ocean dynamics and its structure near the ice front. We have amended this phrase to clarify this.

Modified phrase in the revised manuscript:

In this study, we utilize a large-eddy simulation (LES) model to investigate the role of turbulence within the IOBL flow with a sub-ice shelf plume and a coherent structure of the ocean dynamics near the ice front.

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2. This line in the abstract:

"This demonstrates that the larger baroclinic eddies enforces heterogeneous distribution of positively buoyant meltwater upwelling"

Is confusing to me. How does a larger melt rate demonstrate that there is a heterogeneous distribution of meltwater upwelling? Surely the melt rate could be homogeneous but larger? This point about heterogenous/homogenous response needs to be explained more fully in the text (or excluded, as I am not totally sure what it adds to the conclusions of the paper).

- As the reviewer mentioned, this phrase was not clear. We have removed this phrase in abstract and conclusions.

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3. Lines 32-35 "Shear force generated by tidal mixing and the thermohaline process during sea ice formation are the basal melting driving forces in cold water cavity (e.g., high salinity shelf water), whereas the intrusion of circumpolar deep water (CDW), which is the water well above the local freezing temperature, is the main driving force for basal melting in the warm-water cavity (Davis and Nicholls, 2019; Jacobs et al., 1992; Yoon et al., 2020)."

Needs to be split up into two sentences maybe. One saying shear forces drive turbulent mixing of T and S through the IOBL, and another saying that shear is generated by tidal mixing or by circulation, where the source of temperature and salinity mixed up to the ice base is HSSW or CDW

- In the revised manuscript, previous phrases have been separated to two phrases for warm water cavity, cold water cavity, and its driving forces.

Modified phrases in the revised manuscript:

In the cold-water cavity, shear forces generated by the tides and brine rejection during the sea ice formation (e.g., high salinity shelf water (HSSW)) are the driving forces that cause basal melting (Davis and Nicholls, 2019; Yoon et al., 2020). In contrast, the intrusion of circumpolar deep water and melt-driven circulation near the grounding line mainly cause basal melting in the warm-water cavities (Holland et al., 2020; Jacobs et al., 1992).

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4. Line 67 : This would be a great opportunity to introduce the novelty of your geometry e.g.

However, applications of the LES to IOBL at sub-ice shelf environment are quite limited. The geometry and scales of ice-ocean interaction may be qualitatively different for an ice shelf, particularly when considering the ice front. Ice shelves typically have a thickness of 100s of metres compared with the O(1 m) scales of sea ice.

- In the revised manuscript, we have added the additional explanation for our geometry scale in this study.

Modified phrases:

However, studies on the application of LES to the IOBL under a sub-ice shelf environment are limited (Dinniman et al., 2016). This is because the geometry and scales of this ice–ocean interactions are qualitatively different for an ice shelf, particularly at the ice front.

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5. Line 68 : “In this study, we performed LES experiments for the IOBL and oceanic flow including freezing effect at sea surface and the basal melting process with neutrally buoyant sub-ice shelf plume near ice front.”

Split into two sentences, first saying that you are studying the ice shelf plume at the ice front, then saying the effects you are including within your study.

- As the reviewer mentioned, we have amended this phrase. First phrase is for sub-ice shelf plume and second phrase is for parameterized melting and frazil ice formation.

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6. Introduction: Paragraph on what would we expect of this flow? Cite observations of this near ice shelf region and the ideas that people use (i.e. Garabato instability work, ice front blocking work from Wahlin et al, 2020?)

- We have amended last paragraph in introduction to clarify what we expect in this study. For implication of this study, we have added phrases for the discussion with other observation works.

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7. Line 86: “To simulate the oceanic flow with refreezing”

maybe expand on this, melting is included as well as refreezing so that’s worth noting

- As the reviewer mentioned, we have amended this phrase.

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8. Sa is confusing notationally, it might be better just as S

- As the reviewer mentioned, we have amended this notation.

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9. Line 116: you've introduced the governing equations and the turbulent closure which is great. I think this paragraph could do with a sentence that summarises the key positives and drawbacks of your chosen sub-grid scale model. For instance, I don't imagine it works well for regions where the flow becomes laminar?

- As the reviewer mentioned, we have added the phrases for the important positives and drawbacks of our SGS model.

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10. Elaboration is needed on the melt/freeze condition that you apply. My understanding is that you apply a three equation model with constant coefficients in the open- ocean/freezing region, and a three equation model with exchange coefficients calculated using Monin-Obukhov theory for the ice shelf region. Is this correct? If so you need to state this explicitly

- For the ice shelf region, we also used constant coefficients obtained from exchange coefficients of Vreugdenhil & Talyor (2019), not the calculated exchange coefficients (previous Eq. 16 have to be removed). We have added the phrases for exchange coefficients.

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11. You include citations for your three-equation model values in the table, but I think mentioning your sources in the text would be beneficial for the reader

- As the reviewer mentioned, we have added the phrases for transfer coefficients.

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12. Line 135: "where u* is the friction velocity which is calculated by the velocity at first node and roughness length", are you saying that you infer the friction velocity using a drag coefficient, using the relation U^2 = C_d u*^2 ? If so, you should state this explicitly including your value for C_d and your source for that number. The true drag coefficient is defined using the vertical shear at the boundary; however, I don't think you would resolve those scales in your simulations, so I presume you are using a drag coefficient.

- In this study, we calculate friction velocity using logarithmic law of the wall [$u_*=1/k \times u_1 \times \ln(z_1/z_0)$] with roughness length ($z_0 = 0.005$ m). We have amended this phrase to clarify this.

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13. Line 157 : "Initial profiles were set in the variation range of vertical profiles of our 24 CTD and 23 LADCP observations conducted near the ice front of the NIS." I don't understand this sentence. Are you saying that the initial profiles are taken as a mean of the vertical profiles or a smoothed version? Can you detail the exact method for choosing your idealised profiles?

- As the reviewer mentioned, our explanation for initial profile of potential temperature and salinity was insufficient. We have elaborated information of initial profiles.

14. Line 158-159: “The outlet boundary condition was determined to match the radiation boundary condition (extrapolation)”, could you elaborate further on this boundary condition? I do not understand which radiation you are referring to and what the form of the boundary condition is? This is important for determining the utility of your inferred circulation.

- In the previous manuscript, the explanation for radiation boundary condition was not insufficient. We have added the phrase for additional explanation of outlet boundary condition.

15. Line 160-162: Your Dirichlet boundary condition for velocity implies that you have a rigid lid. Your satellite imagery shows a region of ice-free ocean at the edge of the ice shelf. You have assumed essentially that it is ice-covered and the ice does not move (similar to land-fast sea ice). I do not think this is necessarily a problem, but it should be pointed out when you introduce your boundary condition. Are you simulating a winter-version of this ice front region?

- In the previous manuscript, our explanation for momentum boundary condition at top boundary (sea surface) was incorrect. We imposed Neumann boundary condition for momentum at top boundary, considering open ocean in polynya. We have amended this in methodology section.

Modified phrase:

The cyclic boundary condition was applied to lateral boundaries, whereas the Neumann boundary condition for momentum was imposed on the top layer.

16. Line 221: “Below 400 m depth, a well-stratification features appear in salinity distribution.” I do not know what this sentence refers to, please clarify.

- To clarify what we meant, we have added the isopycnal lines and the phrase of explanation for this.

17. Line 246 : “it is necessary to confirm that the turbulence characteristics of the LES result are similar to the turbulence characteristics of inertial subrange in which energy cascading occurred with few dissipations”, I understand most of this sentence but I do not understand the last three words. Are you just saying it is necessary to confirm that the resolved turbulence in your model follows an inertial scaling?

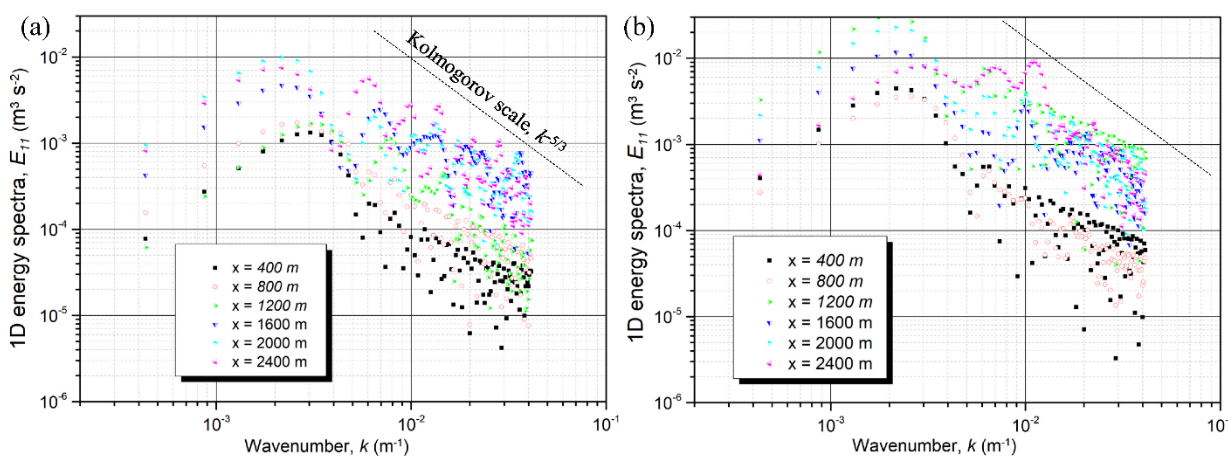
- As the reviewer mentioned, we have amended this phrase to clarify what we meant (without last three words).

18. You should include details of how you calculated the 1D energy spectra, perhaps including the

equation for your calculation. Specifically, I'm wondering whether it's calculated along a single line in the y-direction for instance? I'm slightly confused as to why you have so many fewer points at the high wavenumber end of your plot than at the low wavenumber end. Usually, power spectra show the opposite trend, as you have more points to evaluate small wavelengths with, you get a higher density of points at the high wavenumber end. If you tried calculating your spectra using a numerical method such as the Welch method, you might also get a smoother result, as currently it's difficult to determine whether the presented spectra do indeed show a - 5/3 slope or not. Also, I don't think it's a problem if they do not show a -5/3 spectrum, as you are simulating an anisotropic flow, so you might not expect a classic inertial subrange. Your SGS model may assume homogeneous isotropic turbulence, but your resolved turbulence does not need to.

- As the reviewer mentioned, energy spectra at the high wavenumber have many points than that at the low wavenumber. In the previous Figure for energy spectra, we used different wavenumber range with incorrect interval. However, spectra trend is similar to the previous plot. In the revised manuscript, we have added the explanation for calculating energy spectra and re-plotted the energy spectra distribution with correct wavenumber range.

New Figure 6. One-dimensional turbulence energy spectra:



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19. Line 317 : "Negative heat flux at 320–400 m depths denotes that some of the entrained heat by the intrusion of the outer ocean is transferred to the downward direction." I don't understand this argument. In your temperature and salinity plots, the profiles seem to be increasing with depth in the sub-ice shelf region, so why would the heat flux be negative? I think this needs further explanation

- Because previous sentence was not clear, we have removed this phrase. We have added the phrases for heat entrainment and cooling effect of PISW with convective scale.

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20. Your first discussion section seems more like introduction than discussion to me. Your results

aren’t discussed, rather the broad field and approach is discussed.

- In the revised manuscript, we have focused on our results and its discussion with various limitations.