Referee Report #1

I appreciate the effort the authors have made to revise the manuscript, including the revision of simulations under a modified set-up. The authors provided useful clarification on many of my initial questions about the simulation set-up; however key questions still remain or are raised by the new set-up that should be clarified before publication. The new dynamics examined by the manuscript are generally not explained clearly enough, particularly the driving forces behind the ice-shelf front circulation cell. Na et al. mention 3-d turbulent structures several times in the manuscript but do not describe them. I'd like to see descriptions of the eddies in the IOBL (mentioned on line 177) and those in the PISW. This could help readers understand the simulated dynamics. I appreciate that the authors have offered more information about the oceanographic observations, but the manuscript could still benefit from more discussion of the relationship of LES results to both the oceanographic observations presented and melt rate observations.

Major revisions:
Methodological questions/concerns that should be addressed in the manuscript text:

- Lines 135-139 do not provide enough detail on how the theoretical velocity profiles are utilized in the model and how this relates to the solution of friction velocity. These are really important details for understanding the momentum fluxes in the ice-ocean boundary layer, the validity of PISW results, and what the steady-state friction velocity means in Figure 2.
- I'm troubled by the momentum and heat flux profiles shown in Figure 9. These profiles appear to show that momentum and heat fluxes go to zero at the ice shelf base, implying that there is negligible melting and drag. I found the text addressing these fluxes (paragraph starting on line 279) hard to understand. Can you also relate these fluxes to the spatial evolution of PISW and IOBL as they are advected (i.e., are they gaining or losing heat or momentum)?
- The calculation of the freezing rate in the open ocean is not included in the methods. Is it permitted only at the surface or throughout the water column? There should be associated caveats in the Methods and Discussion about potential frazil ice effects not considered in your simulations, with citations to existing literature on frazil ice effects.
- You imply on line 329 that you aren't using a dynamic SGS model but on line 110 you have included a dynamic SGS equation.
- The thermodynamics of the ice-shelf front are not addressed. Do you allow lateral melting?

The introduction of an ice-front circulation cell warrants further explanation of these dynamics than is currently included in the manuscript. On line 187, the authors write "the development of this circulation is mainly induced by the downward force of salt flux by sea ice formation and the shear stress of sub-ice shelf plume." How does sea-ice formation relate to downwelling? I'd expect convective mixing. What role does sub-ice shelf plume momentum play? Since winds are excluded, how might the results change if winds were included? Is the hypothesized circulation cell compatible with observed sea ice advection patterns? How is this similar to or different from the role that ice-shelf meltwater plays in this study: Malyarenko, A., Robinson, N. J., Williams, M. J. M. & Langhorne, P. J. 2019. A wedge mechanism for summer surface water inflow into the Ross Ice Shelf cavity. Journal of Geophysical Research: Oceans . 10.1029/2018JC014594

The relationship between LES results and oceanographic observations also warrants further explanation. You say (line 216) that the signature of PISW is in the CTD profiles but it is unclear what this signature is in relation to Figure 4.

The agreement between the simulated ice-shelf melt rate distribution and the observed melt rate distribution also needs to be discussed.
The explanation for heterogeneous PISW upwelling is unclear to me. I think it would help to see a planar view of ice-shelf cavity circulation. I wonder if the boundary conditions imposed may be influencing the circulation to a greater extent in the high turbulent shear case.

189: "The circulation pushes the sub-ice shelf plume with downward forcing, making that stratification line near ice shelf is moved to about 350 m depth." Clarify the relative importance of downwelling and mixing for deepening the halocline.

204: This paragraph would be a good place to include a comparison of PISW depth and meltwater fluxes between the 4 cases.

226: "Because the amount of the PISW in the strong turbulence case is larger than that in the weak turbulence case, its turbulence energy spectra within IOBL (297 m) is the lowest." This needs more explanation.

243: "it is shown that the LES model adequately resolves the oceanic flow beneath the ice shelf with the proper thermohaline dynamics by the melting effect beneath the ice shelf and the freezing effect at the sea surface" This is a very general statement. What thermohaline dynamics do you have confidence in?

246: "there are shear forces caused by the momentum of the sub-ice shelf plume and the buoyancy force..." This is not adequately explained. What is the relationship between the stratification, momentum fluxes, and buoyancy? It's worth reminding the reader that the ice shelf based is not sloped so buoyancy does not drive mean flow.

Figure 6: The x-axis appears to span the whole domain, but there should only be freezing at the sea surface in the open ocean part of the domain.

Figure 7: Specify whether this figure includes or excludes the region of underdeveloped turbulence.

The features we're seeing in Figure S3 should be explained in the text. I wasn't able to figure out the difference between right and left panels in this figure.

317: It sounds as if you're saying that PISW is transferring momentum to IOBL in the ice-shelf cavity. Where is this momentum coming from?

322: Explain how the circulation is similar to centrifugal overturning.

330: How will the findings be used to interpret observations?

Minor revisions:
23: "which in turn slows sea level rise" This can be misleading because the rate of sea level rise may increase when ice shelves are removed but not necessarily when the ice sheet reaches a new equilibrium without the ice shelf.

The authors provided more detail about the CTD and ADCP data collection, which was appreciated. One remaining detail that would be useful to the reader is the distance between the ice front and the observations.

Specify t* in hours.

Thank you for clarifying the velocity orientation in this revision. I think it's also worth pointing out to the readers that the zonal velocity is perpendicular to the ice-shelf front geometry (especially in the caption for Figure 3), even though this can be seen in the new figure.
128: I think the notation should be S(z_1) rather than Sa(z_1) since you use S_b.

261: The way this is written, there is an apparent inconsistency in the strong turbulence case that it has the smallest mean shear gradient, highest TKE, yet you say that TKE production is proportional to mean shear gradient and turbulent shear stress.

300 "we used the LES model with proper boundary conditions" "proper" isn't appropriate here, as it is quite subjective.

Section 4.2. It would be helpful to mention the temperature difference between PISW as it exits the cavity and the sea surface freezing point.

319: "showing that this result is in agreement with the previous study of Jenkins (2016)" This is too general. It's simply that both this study and Jenkins have Ekman layers below the ice shelf, right?

327: "constant turbulent coefficients in SGS model" Which coefficients? I thought you were using a dynamic SGS model.

335: It's unclear what effects you'll be examining with the "vertical distribution of pressure"

336: "With better understanding of various parameters on basal melting" Which parameters?
The schematic diagram (Figure 10) is a great addition to the manuscript. I do find it somewhat confusing to include katabatic winds in the schematic when they do not play a role in your explanation of the dynamics, particularly as they appear to be opposed to the ice front circulation pattern.

There are several places where the meaning of the text is unclear:

16: "In the strong turbulence case, there are distinct features in basal melting and flow characteristics." This is too vague.

30 "The driving forces for basal melting in cold water cavity are shear force by tidal mixing and the thermohaline process by sea ice formation" Both of these forcings need more introduction and explanation here.

35 "Therefore, because driving forces from the ocean and opposite forces by the meltwater merge within the boundary layer" I don't know what is meant here. What forces?

43: "Similar features for weak stratification" Which features are you referring to?

48: "controlling the shear impact of its momentum" Please clarify

57: "In order to find out the effects of various forcing clearly" Please specify which forcings

185: "velocities in two cases" Which two cases?

211: "upper streamwise direction" Would be more clear to put in terms of zonal/meridional or parallel/perpendicular to ice front

302: "Additionally, we set to ambient values" set what to ambient values?

356: “it means that stratified forcing by PISW has a nonlinear feature for flow shear by strong turbulence”

332: “this study can be improved by comparing LES results with observations and their feedback” “and their feedback” is unclear.

There are also several places where the grammar needs revision:

9. “but there is a poor understanding of the fluid dynamic, thermohaline physics of the IOBL flow”

11: “velocity’s theoretical profile” >> “theoretical profile for velocity”

28: “The sub-ice shelf oceanic environment can be divided into broad classifications” >> “The sub-ice shelf oceanic environment can be divided into two classes”

38: “physics in ice shelves” >> “physics below ice shelves”

69: “Nansen Ice Shelf (NIS; cold-water cavity)” >> “Nansen Ice Shelf (NIS), a cold-water cavity,”

72: “while remaining thermohaline forcing by the melting and freezing”

177: “are highly fluctuated in” >> “greatly fluctuated during”

178: “As turbulence within IOBL is stronger, the magnitude of fluctuation is larger.”

189: “Noticeable difference between the two cases” >> “A noticeable difference between the two cases”

194: “with different momentum along to streamwise direction in two cases.”

307: “for resolving the IOBL and oceanic flow in reality” >> “for simulating the IOBL and oceanic flow more realistically”

Referee Report #2

General comments:

This study uses large-eddy simulations (LES) to examine the effect of turbulence in the ice-shelf-ocean boundary layer (IOBL) of Nansen Ice Shelf. The simulations are based on recent observations of ocean conditions near the Nansen Ice Shelf. The dynamics are forced by a neutrally buoyant plume moving beneath the ice shelf and penetrating into the open ocean. Key results are the basal melt rate of the ice shelf and the freezing occurring at the ocean surface (modelling sea ice formation), both of which increase when the plume is more turbulent. The authors also attempt to discuss some interesting heterogenous structures in terms of Ekman layer dynamics, but I did not follow this explanation as it stands.

The study is of some interest and has been improved from the previous iteration. The new simulations have an idealised velocity input condition to model a plume with four different levels of turbulence. The temperature and salinity input conditions are broadly based on observations taken further away from the ice shelf. The scientific premise (what happens when turbulence is increased?) is much clearer. There are few LES of sub-ice shelf flow, in particular with the aim to match observations. I think that the study has potential for publication, but I have some comments that I would like to see addressed first.

Specific comments:

1. The authors vary only the shape of the input velocity profiles across the four runs. As the set-up is so constrained (in terms of setting specific input profiles of velocity, temperature and salinity) I wonder what portion of the results are caused by the input conditions versus the dynamics in the system. What do the authors expect would happen if there was no basal melt? Would there still be plenty of mixing in the sub-ice shelf flow, and would we still see the increase in surface freezing at the ice edge? Similarly, if the surface freezing was turned off, how would this change the outflowing plume dynamics?

2. I disagree with (or perhaps I did not understand) the Ekman layer explanation for the heterogeneity seen in the strong turbulence cases. I agree that stronger velocities beneath the ice shelf could lead to a stronger Ekman layer, but why wouldn't this be a homogenous response underneath the whole ice shelf? What is constraining the length scale of the heterogeneity? (E.g. could it alternatively be a baroclinic eddy with a Rossby radius of deformation?) I would appreciate more discussion on these points and an in-depth explanation of the physical mechanisms.

3. I think more can be done to compare against other cases, in particular Naveira Garabato et al. (2017). (Side note: this study seemed to be missing from the citation list in the manuscript.) The only comparison with the Naveira Garabato et al. study was L321 "These physics with the Ekman layer and the upwelling behavior of PISW are similar to centrifugal overturning instability and lateral shear proposed by Naveira Garabato et al. (2017)." I was confused by this. Are the authors saying that the mechanisms are similar? And if so, how similar and what are the differences? Again, I would appreciate a more in-depth explanation of the exciting phenomena that is noted in this study. I think this would really strengthen our understanding of the phenomena we might expect to see sub-ice shelf and on the ice edge.

L11: In the abstract "…we impose velocity's theoretical profile varying the power-law index." Consider rephrasing as I did not know what "velocity's theoretical profile" meant when I first read through. Highlight that the velocity profile is varied and therefore turbulence is also changed.

L35: “Therefore, because driving forces from the ocean and opposite forces by the meltwater merge within the boundary layer (meters to tens of meters) right beneath ice shelf, which is known as the ice shelf–ocean boundary layer (IOBL), we have to investigate the IOBL flow and its structure to reveal the basal melting physics in ice shelves (Holland et al., 2020).” This was a long sentence and a bit unclear. Please consider rephrasing.

L135: The power-law equation U=U_t (z/z_t)^(1/n). Firstly, I assume the x signified multiply and not x-direction (was written in the manuscript as U=U_t x (z/z_t)^(1/n))? Secondly, is z_t the surface roughness? Thirdly, when I quickly plotted this power law I got something that looked different to the Figure 2 inset. So how does this equation directly relate to the profiles in Figure 2 inset? Is there some normalisation coming in to play? Finally, are there other studies (apart from Irwin 1979) that have used these profiles? I am wondering if the justification for the plume shape can be strengthened by citing some more studies that use these profiles.

L152: The wind effect is excluded in the simulations by setting the surface velocity to zero, even though observations showed a non-zero value of wind. In the reply to reviewers there was some justification for the zero velocity condition, would it be possible to have the brief explanation included in the manuscript also?

Section 2: I might have missed this, but what was the surface freezing condition on temperature and salinity? Was it a Dirichlet or flux condition?

L175: How was the large-eddy turnover time calculated here?

Figure 2: There is a lot of variability in this figure, which makes it difficult to determine whether it is in equilibrated state. Are there other results that show the simulations are equilibrated?

Results: Are there any indications of inertial waves present in the simulations, due to having Coriolis parameter (f)? I am wondering if this variability would appear on the friction velocity (Figure 2).

Figure 5: Caption does not seem to be consistent with legend. What is the difference between (a) and (b) figures? If it is n=3 and n=7, then why are these values also varied in the legend?

L239: Could the spatial-averaged freezing rate values also be included here? Perhaps an earlier reference to Table 2 would help (first time you reference Table 2 is in Section 4.2 Discussion)?

L262: “Because turbulence kinetic energy production is proportional to mean shear gradient and turbulent shear stress, turbulent shear forcing is highest in the strong turbulence case.” Could a citation please be included for the first half of this sentence?

Figure 9: There are some negative heat flux values below the positive values in the IOBL. What is causing this?

Section 4.2: There are a few different ideas discussed in this section. Please consider breaking into separate paragraphs.

L324: Please include a definition for the flux Richardson value. How was it calculated in the simulations?

Technical corrections:

L34: “opposite forces” perhaps “opposing forces”?

L70: Here and in several other places there is a “the” missing (e.g. “THE main parameter…”).

L71: “For consistency in experiments, we considered different turbulence state while remaining thermohaline forcing by the melting and freezing.” Was this meant to be more along the lines of: “For consistency in experiments, we considered different turbulence states while keeping the same thermohaline forcing by the melting and freezing.”

L97: Units missing on Coriolis parameter.

L124: “…was used in whole cases.” Should this be “…was used in all cases.”?

L183: Domain center (y=1536m) is different to that stated in Figure 3 caption (y=1728m).

L223: Wavenumber is missing units.

Figure 8, Figure 9a: Are units on (rho u v) correct? Should they be kg m^-1 s^-2?

L257: “Important to not is that a noticeable trend of heterogeneous patterns of melting rate in the meridional direction is not observed.” I think this sentence is saying that the melt rate is homogenous? But it is a bit unclear, please consider rephrasing.

L259: “Freezing rate” should this be “melting rate” as talking about sub-ice flow?