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.

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.
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).

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

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?
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
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?
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.
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 a inertial scaling?
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.
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
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. I

suggest including the key points from this section in a paragraph or two in the introduction then removing the section.

21. If you wanted to make the point about the two circulations more convincing to the reader, you could include a snapshot of vorticity as well as zonal velocity (which doesn't show us the vertical velocities that complete your overturning.
22. You mention the work of Garabato et al. 2017, and I think you could explore the connections here further. If you claim that the mechanism in your simulations is similar, then you need to provide evidence of this fact. I suggest calculating some of the metrics used in Garabato et al. 2017, specifically the Richardson angle (Thomas et al. 2013).
23. Turbulence intensity is a confusing metric, could you instead show dissipation rate? This would make your work more directly comparable to the observations in Garabato et al. 2017.
24. Should we expect any ice front blocking effect in your simulations as in the work of Wahlin et al. 2020? If not, why not? This paper would be worth discussing with reference to your simulations.
25. Ensure your list of references is consistent (specifically the placement of the year)

Garabato, A. C. N., Forryan, A., Dutrieux, P., Brannigan, L., Biddle, L. C., Heywood, K. J., Jeknins, A., Firing, Y. L., Kimura, S. 2017, Vigorous lateral export of the meltwater outflow from beneath an Antarctic ice shelf. Nature, 542(7640), 219–222

Wåhlin, Anna K., Nadine Steiger, Elin Darelius, Karen M. Assmann, Mirjam S. Glessmer, Ho Kyung Ha, Laura Herraiz-Borreguero et al. "Ice front blocking of ocean heat transport to an Antarctic ice shelf." *Nature* 578, no. 7796 (2020): 568-571.

Thomas, L. N., Taylor, J. R., Ferrari, R. & Joyce, T. M. Symmetric instability in the Gulf Stream. *Deep-Sea Res. Part II* **91**, 96–110 (2013)