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The Cryosphere
Discussions

Interactive comment on "Large-eddy simulation of the ice shelf-ocean boundary layer and heterogeneous refreezing rate by sub-ice shelf plume" *by* Ji Sung Na et al.

Anonymous Referee #1

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General comments

There are important gaps in our knowledge of sub-ice shelf ocean dynamics, particularly in the freezing regime. Na et al. have set their sights high in this study, but the manuscript as submitted does not provide sufficient evidence of model validity. In fact, there are several theoretical reasons to believe that the dynamics are not adequately represented in the model. Their argument for validity chiefly rests on the match between simulated temperature and salinity profiles and observations. Since the model has observations as initial conditions and an inflow boundary condition, it is unclear how far from observations the simulations could evolve. Furthermore, their presenta-

tion of the results and discussion of the turbulent dynamics must be more thorough to substantiate many of their scientific conclusions. This study could be publishable after an expanded discussion of model limitations and scaled-back claims to model realism, more elaboration of simulation results, and an expanded discussion of existing literature on frazil ice dynamics.

Specific comments

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

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

I have philosophical concerns about the manner in which the authors validate the model. Authors argue that the model is valid because the LES results match the observations, but the model is initialized to observations and has inflow that roughly matches the observations. Therefore, the argument seems to be that the model does not drift too far from observations, which may be too weak an argument for model validity. It is also unclear to me where in the LES domain you are evaluating the agreement with observations. There should be a more thorough discussion of model limitations and a discussion of their possible impact on simulation results. There should be an explicit argument addressing whether your LES model can capture freezing dynamics in the absence of frazil ice dynamics. Frazil ice dynamics significantly influence IOBL evolution as documented in previous literature, which also should be cited (including the work of Galton-Fenzi). The lack of ice shelf slope should also be discussed. Furthermore, the parameterization of heat, salt and momentum fluxes at the ice base were developed for the ice melting case by McPhee et al. 1987. The applicability of this parameterization as well as the gamma_T, gamma_S exchange coefficients to the freezing case needs to be discussed.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Figure 5. Show the inertial subrange in wavenumber space. You mention that the low wavenumber values don’t fit the -5/3 slope but aren’t these wavenumbers outside the

inertial subrange? Why are these spectra so noisy? Could this indicate insufficient spatial or temporal averaging? You don’t specify these details in your description of the methodology. It’s also unclear why there are several curves for each case and depth. Furthermore, why are the spectra evaluated in 1-d?

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

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

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

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

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

Line 257: “strong velocity gradient” in what direction?

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

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

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

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

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

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

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

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

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

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

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

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

Technical comments

Make it clear in the introduction why you use the term "refreezing" as opposed to "freezing"

There are a few places in the text where you say that water melts, but I think you mean to say that the water mass has a contribution of meltwater from the ice shelf.

Specify which version of PALM you are using.

Line 17: "high shear impact"?

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

Line 107: Sub-grid parameterizations need a citation.

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

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

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

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

Line 235: "explores" to "explore"

Line 274: "to IOBL the flow"

Line 277: reference for the flatness factor

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

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

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

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

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

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

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