Response to comments

We wish to thank editor for their valuable comments, which will help us to improve readability of our manuscript. We addressed each of the comments in turn below. Our responses are colored by green.

Editor comments

- General comments

Thank you for your submission of a revised version of the manuscript on Large-eddy simulations of the IOBL near the ice front of Nansen Ice Shelf, Antarctica. You have addressed the reviewer's comments by supplying additional details in the main text, as well as new figures in the supplementary material. Thank you for these new contributions.

Based on my own reading of the latest version of the manuscript, I believe significant additional improvements are still required to elevate your manuscript to publishable quality. I have attached a file with a list of further comments and suggestions for improvement, in particular to help improve the readability of the manuscript. At present, significant portions of text, including the abstract and introduction, lack coherence and often use poorly defined terminology, which make it difficult to follow the narrative or glean the key messages. Conclusions are not always firmly supported by the results. I think these shortcomings can be addressed without the need for additional experiments or figures, and I therefore welcome a revised version of your work, which takes these suggestions and comments into account.

- As the editor mentioned, there were shortcomings in previous manuscript. Based on comments and suggestions, we have rearranged the structure of manuscript and rephrased the shortcoming parts to avoid confusion.

- There were many comments from marked pdf file. For the crucial comments, we addressed each of the comments with our responses.

- Abstract

12-13 It is unclear how you define 'sub-ice shelf plume', and its relation to the IOBL. To avoid confusion, consider omitting this sentence here.

- We have omitted the part for “sub-ice shelf plume” to avoid confusion.

16-18 This sentence seems out of place and I'm not sure these details should be included in the abstract.

- We have removed these details of this sentence.

26-28 Readers might find this sentence confusing, because you have not defined what you mean

by ice shelf plume. So far I assumed you would be simulating the ice shelf plume in your model. If this is not the case, how do you 'parameterize' the plume?

- As the editor mentioned, we simulated the ice shelf plume in our model. This sentence can be confusing for readers. We have removed this sentence.

- Introduction

34-37 I think this paragraph is better placed after line 45. First you introduce the ice shelf-ocean system, explain that this is a complex environment, but that observations could help; then you point out that observations are difficult to obtain; then that ocean models can help improve our understanding.

- As the editor mentioned, we moved and merged this paragraph to observational effort part.

58 The concept of a sub ice-shelf plume is used heavily throughout the manuscript, but has not been defined. In particular, how does it relate to the IOBL? Here it sounds as if they are 2 separate entities, but isn't the IOBL a part of the plume? Please make it abundantly clear what you mean by 'sub ice-shelf plume' and how it distinguishes from the IOBL.

- We have added the explanation for the concept the IOBL and sub-ice shelf plume.

62 You need to make abundantly clear early on (maybe even in the abstract), that you focus on the dynamics in the vicinity (within kilometers) of the ice front, and that you do not include the grounding line in your simulations. Otherwise, readers will assume/expect you are doing cavity-wide simulations, and won't know you are only looking at ice-front dynamics until you describe the experiments in the next section.

- We have added the paragraph for the frontal region of the ice shelf.

72 I think before this paragraph you need to give a short description of what LES are, and how they have been used to study ice-ocean boundary processes so far.

- We have added the paragraph for LES description and its relationship with IOBL processes.

77 I think lines 77-83 need to come earlier, where you motivate your focus on ice front dynamics.

- We have moved this part and added paragraph for the frontal region of the ice shelf.

83 So far it was unclear to me what the overall objectives of your work are. It would be nice to see some of the next paragraph (L83-85, L90-95) earlier in the introduction, potentially even after line 45.

- We have rearranged the introduction to clarify the importance and objectives of this study.

- Methodology

153 Are you not trying to model the IOBL, and if so, would ambient conditions not need to be set away from this layer, rather than 'within' the IOBL?

- As the editor mentioned, “within the IOBL” was not clear for describing ambient conditions. We rephrased this part.

155 in situ: please refer to Sect 2.1, Fig 1 and strongly reiterate that measurements are taken in front of the ice shelf, not below the ice shelf.

- In revised manuscript, we have reiterated that measurements are taken in front of the ice shelf.

185 A general comment about lines 185-190: It might be better to move these to the beginning of the section, so you first describe your domain, carefully name/identify the different (types of) boundary, and then describe the conditions you impose at each boundary.

197 again I think the description of the ice shelf geometry needs to come earlier in your description of the domain geometry and before you introduce the various boundary conditions.

In revised manuscript, we have rearranged the methodology section as the editor mentioned.

- Results

222 I think this can be removed here, and discussed in section 4, when the reader has a better understanding of experiments and results in Fig9 have been presented.

- We moved this part for IOBL depth after Fig 9.

229 It seems some periodicity also exists before 14t*. Can you comment?

- Your comment is correct. We have amended this phrase to clarify that averaged friction velocity had a temporal convergence after 14 t^*.

242 I don't understand how this follows from the preceding discussion. Can you please elaborate more?

- Main driving forces for inner cell were buoyancy forces by PISW upwelling and downwelling of local salt maximum. That is way inner cell is stretched in the vertical direction. We have amended this phrase to clarify this.

249 Melt rates are <0.3m/yr. Is this really 'vigorous'? Please avoid subjective statements.

- We removed this statement.

256 By 'observed' you mean 'simulated'? But isn't the zonal velocity imposed as a boundary condition? How is this a diagnostic model result?

- Although we imposed the boundary condition for zonal velocity, the flow gradually develops to IOBL flow as it moves away from the inflow boundary. So, we used the expression of “observe”.

306 'Similar to' needs more nuance. You need to point here that you imposed observation-constrained boundary conditions (refer to the dashed lines in Fig5). Any deviation of model data from the dashed line is due to model physics, which tend to the observations for T, but move further away from observations for S. So there is a nuanced picture here, where you are able to match some observations, but unable to explain other features, such as S and crucially, the core of cold water at -100m.

314 General comment about Section 3.2: It seems like a best match between modeled and observed temperature is found for low turbulence numbers n, whereas a best match for salinity is found for high turbulence numbers. How do you reconcile these results, and does it mean there is no optimal value for n that best describes both observed profiles of T and S? If so, 1) can you really conclude that LES results are 'similar' to in situ observations, and 2) what is the role of n in better simulating IOBL processes if no value of n provides an optimal match with observations?

- These LES results resolve *in situ* oceanic circulation and thermohaline properties, but LES results had a difference in magnitude quantitatively. This difference was caused by initial condition, boundary conditions, model physics, and thermohaline effects of melting and sea-ice formation. To correct this, additional sensitivity studies for each condition and *in situ* observation for sub-ice shelf environment will be needed.

- The reason we used turbulence number n is that we cannot obtain *in situ* profile of sub-ice shelf, not the optimal match to observations 1 km away from ice shelf front.

- In this study, we cannot diagnose what parameters cause this difference between LES results and observation. But we can resolve the oceanic environments such as overturning cells and PISW upwelling. Based on this, we have amended “similar to” to “are consistent with”.

L315-329 Consider adding this as a separate section, or move to the discussion section.

- Because this paragraph is to examine resolving turbulence and its spectra of LES results, this is about the validation of turbulence resolving. So, we have not moved this paragraph.

354 This is VERY speculative, as you are mixing (incomplete) simulations with (uncertain) observations. Your simulations don't really allow you make this conclusion unless you include effects such as a non-trivial ice geometry, non-trivial bathymetry, full development of the boundary layer and sub-shelf plume, 3D ocean circulation in the cavity etc. I would argue that a full cavity model is needed in order to be able to partition the melt resulting from frontal dynamics, and melt occurring beneath the remainder of the ice shelf.

- We agree with editor’s comment. We have removed this phrase to avoid confusing.

369-375 I'm unsure why you introduce a metric for the IOBL bottom here, and how it contributes to the discussion. Can you remove L369-375, or clarify why this is important?

- IOBL bottom and the depth of maximum momentum fluxes are highly correlated to heat entrainment by turbulence. So, we have to determine the IOBL bottom here. We have amended this part to clarify why it is important.

- Discussions & Conclusions

386 I think this belongs in the introduction.

- We have moved this part to introduction section.

435 L435 Do you have any specific cavity observations in mind? Why not simulate a full cavity geometry (including the grounding line) and only set boundary conditions at the open ocean end, for which more observations exist?

- Because this LES model needs to fine grid scale to resolve the SGS turbulence, it is hard to simulate a full cavity geometry. Vertical profile of how-water drilling can be helpful for composing the boundary conditions of LES.

447 what is the 'lateral side' of the ice shelf?

- It is vertical side of the ice shelf front. We have amended this.

488 I think this is an overstatement, since you don't present observations of T, S, LDCP data for the inner overturning cell. Perhaps say 'are consistent with'

- As the editor mentioned, it is overstatement. We have amended this part as editor’s comments.