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# *Interactive comment on* "Response of sub-ice platelet layer thickening rate to variations in Ice Shelf Water supercooling in McMurdo Sound, Antarctica" *by* Chen Cheng et al.

**Anonymous Referee #2**

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Summary

This manuscript identifies, and attempts to quantify, the complexity of the interactions between supercooled water, frazil ice held in turbulent suspension, platelet ice accretion and the temporal evolution of Ice Shelf Water plumes originating in the cold cavities of Antarctic ice shelves. The model used is an evolution of earlier models applied to this problem, with the significant point of difference being inhomogeneous vertical profiles of supercooling and frazil-ice concentration.

In my opinion, the material covered could be of interest to the polar oceanographic community, and its presentation is timely, considering the expansion of interest in the



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fate of the Antarctic ice shelves and the sea ice affected by their basal melt. However, I do not believe that the stated 'main objective' has been satisfactorily achieved in light of the limitations of the sensitivity studies. I also think that significant clarification is required before the manuscript would be ready for publication.

Major Points

1. Novelty of approach

a. The introduction of depth-dependent supercooling and frazil ice concentration was explored in a practical setting by Cheng et al., (2017), so it is not clear that there is a great deal of novelty in the present study.

2. Confusion around terms and purpose

a. The general purpose of the study appears to be to improve model representation of marine ice accretion beneath ice shelves. That being the case, some justification is required for evaluating the model against an ISW plume beneath sea ice, which is generating a sub-ice platelet layer (as opposed to accreted marine ice). While there is clearly a lot of connection and similarity between these, some explanation of the differences and limitations of applicability need to be provided. For example: the difference in absolute pressure and its consequence for supercooling potential; the effect of basal slope for generating buoyancy-derived momentum; the likely strength of flow and turbulence in the boundary layer and implications for size of crystal held in suspension.

b. A comment is also required on the general applicability of a model evaluated against a sub-sea ice ISW plume for the ice shelf cavity.

c. No definitions of the terms 'frazil ice' and 'platelet ice' are offered, and direct similarity between sub-ice platelet layers and accreted marine ice is implied. These terms are not interchangeable, and their use here contributes to a general confusion as to the overall purpose of the study. Use of the term ''platelet-like frazil crystals" (p1 line 28) exemplifies this confusion (for both the authors and readers). Clear definitions of these terms would help, as would a statement that the plume in McMurdo Sound is being used to evaluate model performance on the basis that it is the most comprehensively-observed plume available, despite its expected differences (which should be clearly identified) from a sub-ice shelf ISW plume.

d. The general confusion of purpose is demonstrated in that the Abstract opens with the ISW plume in McMurdo Sound (I would argue that the abstract should focus on the model rather than its application), while the Introduction is primaritly concerned with the sub-ice shelf regime.

e. I find the comment in the Conclusions (p. 10, lines 9 – 11) that “additional observations of supercooled ISW, SIPL and frazil size spectrum in western McMurdo Sound are needed to constrain the relationships determining marine ice formation beneath ice shelves”, rather strange. The recommendation surely should be to observe these parameters beneath ice shelves, if that is indeed the desired outcome?

3. Inadequate explanation of sensitivity studies and choice of values for parameters

a. Page 5, Line 26-27 justifies the tuning of parameters to reproduce the observed SIPL and supercooled ISW thicknesses, but we are not given an indication of the strength of this tuning. How do the chosen parameters for the standard run compare with the observations, and observed range of values?

b. I would expect that the sensitivity of the model could be just as significant for ar, Nice and solid volume fraction as for the parameters that were included in the sensitivity study. Further testing of the sensitivity to these parameters would be ideal, but at least a statement acknowledging (and justifying) their exclusion is required.

c. The role of settling dynamics is clearly significant here (as in sediment studies), but this is not referred to at all.

Minor Points

The thickness of the accreted platelet layer is a critical parameter for testing the performance of the model. However, the mechanism employed for transferring crystals out of the suspended frazil and into the accreted platelet ice layer is not made explicit.

The manuscript is rather heavy-handed in its use of acronyms:

'ISW' is well-known and can be used freely;

The reader may need a few reminders of what 'SIPL' represents;

Please expand 'MMS' to McMurdo Sound in all cases;

'FIC' should not be used, especially as it is variously interchanged with 'ci';

'VM' is OK;

but 'NVM' is non-intuitive;

'ASTR' is non-intuitive – is it possible to avoid this altogether?

The superscript '0' (as in T0SC) is not explained and left to the reader to figure out.

P 1, Line 27: "a necessary condition for ice crystals to form. . ." either needs a reference or should be removed, as primary nucleation is not thought possible under observed supercooling conditions.

P 2, Line 5-6: "the SIPL should not be ignored. . .". This manuscript is not offering new evidence on the role of the SIPL, so is not the place to be offering this recommendation.

P2, Line 8-9: "Owing to the paucity of direct observations. . ." And yet this manuscript relies heavily on observations collected in McMurdo Sound. I do not feel that the observations available (beyond HU14) are sufficiently acknowledged or applied.

P2, Line 16-17: There is no marine ice production in McMurdo Sound (this is an example of the general confusion identified above).

P2, Line 25-26: here it is inferred that the ISW plume thickness is allowed to evolve in the present study, but this is not stated explicitly.

P3, Line 5: The statement that “the concentration of suspended frazil ice controls the dynamic and thermodynamic evolution of ISW outflows” requires a reference.

I see that a crystal size distribution, incorporating 5 size classes, is being used. But how this is incorporated is not explicitly stated.

a. P 5, Line 13: I am not convinced that the frazil ice crystals should be evenly distributed among size classes. Naively, I would expect a much greater proportion to reside in the smallest size class(es). At the very least, this treatment needs a reference.

b. Is the smallest size class small enough? (I’m not sure on this). Is there a reference to support this?

c. I cannot see anywhere that explicitly states whether crystals move between size classes as they grow/melt; or if and how they are removed from the largest size class to accrete as part of the SIPL.

d. I think more justification is needed on the collapsing of a range of size classes to a single size class for comparative purposes.

Which temperature is being used? (i.e. in-situ, potential, conservative. . .) In observations of the ISW plume in question (e.g. Dempsey et al., 2010; Robinson et al, 2014; which the authors make reference to), the upper ocean is homogeneous in potential temperature.

I presume that latent heat is being added as ice crystals grow, since the supercooling is variously referred to as being ‘released’, ‘utilized’, ‘converted’ and ‘varied’. However, how this is incorporated is not identified or referred to anywhere.

P3, Line 27: To aid clarification, could the word ‘background’ be changed for ‘ambient’ (current), since this will align with the subscript used in the symbol.

P3, Line 28: I take from this that the ‘tidal’ speed is not varying in time, but is effectively



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just an additional source of kinetic energy?

P 4, Line 8: Please identify where the use of DSC = 50 m is derived from. Is there observational support for this? Or has it come from HU14?

P 4, Line 13-14: I am confused as to why you would want to avoid frazil melting in the lower part of the plume if it is overheated.

P4, Line 19-20: What (observational?) support is there for determining that the VM model shows 'physically-reasonable and desirable characteristics'? What determines this? Is there a reference to support this statement?

P 5, Eqn (3): the fact that individual ice crystals may double in volume after precipitation shouldn't affect the thickness of the SIPL, since presumably their growth is into the interstitial spaces, and shouldn't cause the entire SIPL to expand?

P5, Line 8: My understanding is that the 'background circulation' applied in HU14 was to provide the buoyancy-driven momentum that the sub-IS plume would naturally possess, but which cannot arise automatically within a model beneath a horizontal ice base (i.e. sea ice). That being the case, this represents a departure from sub-Ice Shelf applications, and should be explained again here.

P 6, Line 10-11: I am not satisfied that there is sufficient support for the statement that 'both models reproduce the observed values of ISW supercooling reasonably well'. Looking at Figure 4a,

a. The structural difference between sites FN and NN is not captured at all;

b. The neap-tide supercooling at site C is very different to the modelled result, and the difference between spring and neap at that site is not reflected in the model timeseries;

c. The observed values at Site W do not coincide with the model timeseries values;

d. The only site that has 'reasonable' agreement is Site I, which is very close to the inflow point of the prescribed ISW plume.



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Also, the spatial resolution in the figures, especially of the ISW plume structure and SIPL thickness (figures 5 and 6) seem to greatly exceed the stated resolution of the model (1 km, P 5, Line 12). This then has implications for the statements made about how these are resolved by the model (as in P 6, Line 29). Also, this seems like fairly low resolution for the ISW plume itself, at 3 km wide.

P 7, Line 27ff: Behavior around the 'inflexion' points is particularly interesting. I think it would greatly strengthen the paper if the inflexion points could be investigated in higher resolution (especially in Z* and $\sigma$SC) to the point where the mechanism causing them could be adequately explained. There is an informal explanation offered in lines 28 – 30, but studying this in greater detail could be very useful for determining if this is a model artifact or a real phenomenon that could invite observational investigation.

P 8, lines 9-10: perhaps the wording could be more straightforward? Suggest using "... because the supercooling is used less efficiently for producing SIPL in the NVM than in the corresponding VM runs."

P 8/9 How do the critical thicknesses of the supercooled portion of the ISW plume (65 and 78 m) compare with observed thicknesses of the McMurdo Sound ISW plume?

P 9 Line 8: The sentence is incomplete? Perhaps "... there are analogous inflexion points..."?

P 9 Line 24: The sentence is incomplete? "... that have been taken instruments not specifically designed..."

P10 Line 14-15: Robinson et al. (2017) is an example of such a process study and could be referenced here.

Table 1: Is there support in the literature for choosing ar = 0.02?

Table 3: Could the names of the variables be repeated here so that the table is self-contained?

Figure 1: Why does the model data only extend as far as the borehole domain, and not to the edges of the model domain?

Figure 1: Do the location names (C, I, W, NN, FN) mean anything in the present study? If yes, can these be provided? If no, could they be simplified to A -> E without loss of generality?

Figure 1 caption: The 'oceanographic and SIPL data' are not shown in the figure, so reference to them should be removed from the caption. Unless what is meant are the locations of the oceanographic and SIPL data?

Figure 2b: The almost-vertical blue line to the right, and horizontal blue line along the x-axis appear to be mistakes?

Figure 2b: It would be helpful to add the words 'Melt' and 'Freeze' alongside the appropriate parts of the y-axis. Also, I think that the change of vertical scale is not necessary (and possibly only adds confusion)

Figure 3 caption: "... relevant processes within a supercooled ISW plume of homogeneous temperature and salinity".

Figure 6: Just a suggestion: it would aid comparison if the location dots could be colored by the measured SIPL thickness.

Figure 7a: There is no explicit specification of what 'VM30' through 'VM110' refers to.

Figure 7a and 7b: Text says that the ASTR estimate is from the last 30 days of the model runs. This needs to be repeated here.

Also, converting the ASTR to cm/day may be a more useful unit (and less misleading, given that it only refers to 30 days' worth of growth)

Figure 9: The formulae for the fitted lines could be moved out to the right so as not to become lost amongst the data points.

Figure 10: I don't think the additional enlargements provide any further useful information and could be removed.

Figure 10: Numbers on axes are too small.

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