The Cryosphere
Discussions

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 #1

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1   General comments

The goal of the paper is to understand the relationship between supercooling, frazil concentration and platelet ice accumulation rate. The authors apply a model that some of them had published in JPO in 2017. The claimed novelty of this model is that it considers the vertical distribution of supercooling and frazil ice concentration. The model is applied to McMurdo Sound and agrees with observations better than previous models. The authors try to understand the complex behaviour of the model through a (in some ways extensive) series of sensitivity experiments.

My overall evaluation is that the paper needs major revision before it could be published in *The Cryosphere*. I do think that the paper is interesting, timely, and that the idea of improving frazil ice models by considering variation with depth is worthwhile. However, I don't think the modelling performed is particularly novel and there are important limitations to the sensitivity experiments performed that limit the significance of the results. The explanations of the results (which are arguably the main interest for a general audience) are somewhat underdeveloped. I hope the authors are able to address the specific issues, which I discuss in detail below

2   Specific comments

1. The authors claim there are two differences in their model compared to previous studies. They consider: (1) the vertical distribution of supercooling; (2) the vertical distribution of frazil ice.

   I think that effect (1) is not really novel, despite the claim 'Cheng et al. (2017) introduced ... the linear depth-dependence of supercooling into [equation] (1)' (see page 4, line 2). As I understand, several previous studies that the authors reference considered this effect. For example, Smedsrud and Jenkins (2004) mention the 'depth-averaged freezing point used to calculate [the growth rate]' in paragraph 32. Jenkins and Bombosch (1995) seem to say the same thing (around equation 15). I could certainly believe that this effect matters, but I don't think this paper can really claim to be novel in this respect, at least in the context of ISW plume models. The discussion in Cheng et al. (2017) seems to better reflect that effect (1) has been previously considered by various studies.

   Effect (2) is novel in this context, although there are some studies in other contexts. For example, see Svensson and Omstedt (1998) in Cold Reg. Sci. Tech. The authors should reference and discuss this the introduction. However, I am

not *entirely* persuaded that effect (2) is important in the context of this study. Naively, I would expect that the frazil rise velocity (say mm/s) is much smaller than the shear velocity, in which case the frazil ice concentration is almost vertically uniform in practice.

2. More broadly, it seems there is the potential for inconsistency in modelling some quantities (such as the water temperature) as vertically uniform, while modelling the frazil ice concentration as varying with depth. Could the authors discuss this issue more clearly and add some justification

3. I think the presentation of the model and the discussion of the results lost sight of the fact that frazil crystals grow by increasing in size.

   In section 2, it was not immediately clear that several quantities like c_i are a function of crystal size, although the authors do point out that crystal size determines the rise velocity.

   Then again on page 8, line 2, the authors analyze the results in terms of some chosen crystal size class that was dominant in some previous studies. Since the model calculated the size distribution (I think), the authors can interpret their results in terms of it. Picking a particular size class is odd because one of the sensitivity experiments involved changing the size classes (i.e. the discretization of the crystal size distribution). Note that the growth rate depends on crystal size, so a simple average size might not be appropriate.

   In the conclusions, page 10, line 2, the authors mention the 'complicated form of the relationship depending on suspension index'. I think this relationship might become clearer through thinking about changes to the crystal size distribution.

4. Equation (2) is somewhat hard to understand. Where does the factor 6 come from? It could be incorporated into Z_* in any case. An advection-diffusion equation in z needs two boundary conditions to determine the two integration constants. One comes from the vertical average C_i but what about the other. It looks

like neither c_i nor its gradient are zero at $\sigma = 0$. Additionally, I think the factor of σ in the denominator is a typographic mistake.

[Reading Cheng et al. (2017), it seems that this the factor of 6 is an average inverse diffusivity based on some previous studies, but I think it is important that the present manuscript discusses equation (2) more fully, given that it is the main novel aspect of the paper.]

5. Page 4, around line 15. There is a pair of papers (Rees Jones and Wells, 2015 & 2018, the latter of which you cite) which update the treatment of frazil crystal growth/melting.

   Regarding the need to limit the mass loss due to melting, I think this is a departure from the 'equilibrium' (i.e. steady state) assumption in your vertical ice concentration distribution. Because the local melting rate is proportional to local concentration, the ice mass should just approach zero exponentially over time. Or perhaps there is an issue with your time stepping scheme or use of an excessively large time step (how was 25 seconds chosen)?

6. Sensitivity experiments. In equation (3), there are a couple of 'fudge factors' (solid fraction, volume change) that are not varied. Changing these would be a direct, linear way to get more platelet ice in runs that had less than in observations. Can the authors explain what process the volume change parameter is supposed to represent?

   More broadly, the authors seem to tune a large range of model parameters. This limits the predictive value of the study since these parameters are unknown *a priori*. Could these parameters be used elsewhere or would one need to retune each time? Is the greatly increased drag coefficient (for example) plausible?

   Some of the sensitivity experiments seem very odd. Particularly changing the frazil size configuration (discretization of the crystal equations in size space). If the distribution were well resolved, changing the discretization wouldn't change

the results. I think the author should use more crystals size classes (at least as a test) and should use a smaller minimum crystal size. The minimum size is often very important in these models because it ends up being the size of nucleated crystals. A minimum size of 0.2 mm, therefore, seems large.

7. Section 4. I felt the explanations could have been clearer. For example, phrases like 'supercooling is utilised more efficiently' seem to be key but I didn't understand precise;y what this meant or why it happened.

8. The use of acronyms was excessive for my taste. For example, page 5, line 24 contained five acronyms. I would recommend using more sparingly. Personally, I would change SIPL to 'platelet layer'; FIC to 'frazil concentration'; ASTR to 'thickening rate'. I would change VM and NVM (in any case NVM is an odd name, a double negative, perhaps 'vertically uniform' would be better). In a similar vein, the notation is excessively complicated, with an over-use of 'modifiers' (to give a couple of examples, among many, $\overline{T_{SC}^0}$, Z_*^a).

9. The figures contained a lot of information so they are necessarily somewhat complicated. However, all figures are particularly difficult to interpret printed in black and white. In figures 7–10, I would have coloured each point according to Z_*^a, rather than dividing into bands. The complicated legends could then be replaced by a simpler colour bar.

3   Technical comments

1. Title: I think the paper is broader than just the thickening rate

2. Abstract: I would particularly avoid using so many acronyms in the abstract

3. Abstract, line 16 'choice of frazil ice suspension index': is it really a choice? The meaning of the term 'suspension index' is not clear at this stage.

4. Page 1, line 25: maybe delete 'the' before 'elevated pressure'

5. Page 2, line 14: clause that starts 'in which supercooling' belongs in or immediately after previous sentence.

6. Page 2, line 24: how important are these differences (D not constant, 2 lateral dimensions) in the context of this paper?

7. Page 3, line 5–13: split sentence

8. Page 3, line 6: 'can be quantified through' is vague. Perhaps 'is proportional to'

9. Page 3, line 8: I_{gr} not clearly defined

10. Page 3, line 9: [0,1], use a comma

11. Page 3, line 27: don't understand $U_p + U_a$ or $U_p(U_a)$? Similarly with V.

12. Page 3, line 28: italicize x and y (also elsewhere)

13. Page 5, line 7: reads slightly oddly because the 'outflow' is actually the inflow to your domain

14. Page 5, line 16: do you conserve latent heat in this the crystal volume doubling step?

15. Page 7, line 4–8: I suggest that you think about steady state solutions

16. Page 8, line 18: presumably the consumption of supercooling is by the release of latent heat? If so, I would say this explicitly.

17. Page 9, line 13: 'the differences increase with decreasing Z_*^a' seemed odd to me, because $Z_*^a = 0$ is supposed to recover the previous models

18. Table 1: some quantities not defined in main text, including $a_r, \overline{n}$

19. Table 1: T_{ini} seems to be a function of z, but I thought temperature was vertically averaged in the model?

20. Figure 3: annotations like p' will be impossible to understand unless the reader is very familiar with the frazil literature. I think w' is more usual than ω' for frazil growth.