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The Cryosphere
Discussions

Interactive comment on "High-resolution simulations of interactions between surface ocean dynamics and frazil ice" *by* Agnieszka Herman et al.

Anonymous Referee #1

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1 Summary

This paper investigates the dynamics of the ocean mixed layer (OML) in the presence of frazil and grease ice using Large Eddy Simulation (LES). It studies the effect of wind-driven, convective and Langmuir turbulence on frazil ice, and also the effect of frazil ice on OML dynamics. The turbulent flow leads to segregation of the frazil ice and the formation of streaks of ice on the surface, which are qualitatively similar to field observations. The frazil ice can strongly influence OML dynamics, primarily through its effect on buoyancy. The study suggests several avenues for future research.

I think that the topic of the study is interesting and novel in several aspects. Previous observations have only been interpreted in a qualitative fashion and previous models have been one-dimensional rather than the three-dimensional calculations presented here. The paper is very well written and the analysis performed is thorough with most of the limitations clearly explained. There are a few relatively small weaknesses discussed below which the authors can use to revise their manuscript. However, overall, I think the paper is excellent and should be *accepted subject to minor revisions*.

2 General comments

1. **Model formulation:** there are some limitations/assumptions of the model that should be discussed more clearly or considered in further or future calculations.

   The hydrodynamic equations (1–4) assume that the concentration of frazil is small. If this were relaxed, they would need terms like $(1-C)$, where C is the total frazil concentration, in various places (see e.g. Jenkins and Bombosch, 1995).

   The frazil model doesn't consider crystal growth (which is a reasonable starting point and is well discussed). However, I didn't understand why only three crystal sizes were used rather than a much better-resolved crystal size distribution? Presumably, this is not a very expensive part of the overall calculation? Was the sensitivity to the number of crystal size classes tested? It will certainly be essential to include many more when crystal growth and nucleation are considered (as mentioned some of the cited references). Another subtle issue is the assumption that the crystals have a constant aspect ratio. An alternative is to assume they have a constant thickness, which is arguably more reasonable from a crystal growth point-of-view. The crystals remain disk-shaped because it is energetically much easier to grow radially than in thickness.

   The results presented here are clearly very sensitive to the frazil terminal velocity (figure 3). I think the authors should consider comparing their calculations with laboratory data (e.g. of McFarlane et al. 2014). They should also consider crystal-shape effects (assuming eq. 16 wasn't designed for disk-shaped particles).

2. **Sensitivity of results:** The authors choose a particular OML-average volume fraction of 0.00168 for each category, so in total 0.005 (i.e. 0.5%). This is actually rather high. I think there should be better discussion of the sensitivity of results to this choice (e.g. F_ρ must increase with increasing ice concentration, but is the sensitivity linear or are there nonlinear feedbacks?)

3. **Comparison with observations:** The paper makes some comparison with observations, particularly the streaks of ice visible at the surface. However, the comparison is mostly qualitative. This is fairly well discussed in the final section; a forward link could be added in the final paragraph of page 20.

   A more quantitative comparison would be preferable. A starting point would be to devise and calculate statistical measures of the band size and spacing in the numerical calculations and then consider whether these are affected, for example, by wind speed. This could additionally be used to compare plots in Supplementary Fig. 9 quantitatively.

3 Specific comments and technical corrections

4. **P2, L20:** 'does have influence' → 'influences'.

5. **P3, L6:** suggest adding review article Daly: Frazil ice dynamics, CRREL Monograph, 84, 46 pp., 1984.

6. **P4, L14:** suggest expanding discussion of laboratory observations.

7. **P5, L3:** explain briefly why turbulent conditions are necessary.

8. **P5, L4:** parenthetical remark a bit confusing, I would delete whole remark and instead change 'buoyancy' to 'convection' or 'buoyancy-driven convection'.

9. **P6, L10–12:** is this good for frazil, especially the bigger crystals?

10. **P12, L9:** should 'd' etc be italicized?

11. **Sec. 3.3.3:** I think this section could have had more discussion of uncertainty. I would imagine that (18) is a more robust relationship than the others.

12. **P15, L9:** where does the latitude come into the calculation? I assume only in Coriolis term but the role of rotation didn't seem to be discussed much.

13. **P15, L12:** vertical boundary conditions on frazil concentration (I saw some earlier discussion of boundary conditions for CROCO in general, but presumably these references don't say anything about frazil).

14. **P17, L1–4:** how/why were these chosen? If you turn on crystal growth in future, results will be extremely sensitive to supercooling.

15. **P17, L16:** I would make it clearer that the phrase 'this choice' is referring only to the uniform distribution, not to all the other choices.

16. **Fig. 5:** Quite busy but just about readable, consider removing intermediate U_a.

17. **P20, L12:** expand discussion of interaction with pycnocline

18. **Fig. 7:** I found the main plots confusing and think they need a clearer x-label and caption. Is this a horizontal average? Do the PDFs integrate to 1?

19. **Sec. 5.2:** This section gives an impression that buoyancy and drag are similarly important, but the graphs suggest that the all-forcing result is very similar to buoyancy, which suggests buoyancy is much more important than drag.
20. **Fig. 9:** Thin lines very hard to see and distinguish. I would make all lines thicker and use line style to distinguish.
21. **P24. L1:** Typo? (Fig. 11g)?
22. **P24. L5:** Typo in word 'important'.
23. **P28. L19:** In a different way to F_ρ?
24. **P30. L17:** Could also mention flocculation?
25. **P30. L31:** The editor may wish to consider the journal's policy about code availability. My opinion is that code by correspondence is less good (in terms of reproducibility) than code made publicly available with a doi.
26. **Supp. eq. (2):** r appears on both LHS and RHS.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-189, 2020.

High-resolution simulations of interactions between surface ocean dynamics and frazil ice

Review H. Heorton

This paper documents the development and simulations from a 3D model of the Ocean Mixed Layer OML, within a polynya. The paper is very well presented and I particularly appreciated the extended description of the developments of OML models that allowed for this study to be undertaken.

The paper includes an extended section describing existing methods of analysing the mixing regimes for the OML, and then applying them to an OML with suspended particles. This is then expanded for latent heat polynyas, with the likelihood of each mixing regime discussed. The authors conclude that all three mixing regimes are likely in coastal polynyas but Langmuir turbulence is likely to dominate.

Then follows a full description the model that is thorough and easy to follow for such a complex model.

Two groups of simulations are presented. First a model with no frazil to hydrodynamic coupling under various atmospheric forcing. The mean states of these model runs are described showing the structure of the ocean currents and distribution of frazil crystals. The surface distribution of crystals and lateral currents are presented and contrasted with observations. Then a second group of simulations are presented that show the effect of adding in frazil-related processes. The results from these runs are compared with the first group to show the influence of each process.

The main finding presented are:

Adding the influence of frazil crystals to the net density of the ocean has the greatest influence on the vertical profiles of ice crystals.

The observed surface ice crystal collection is dominated by the largest size class of crystal. These observed surface features, whilst mainly driven by Langmuir circulation are also the result of multi-scale processes.

The results show that ice crystals or different sizes will be ‘sorted’ by their floatibility, with larger crystals at the surface and smaller crystals at depth. This true in previous work and is also true for this complex 3d modelling study.

I recommend the paper for publication with first a few minor corrections as listed below (mainly relating to referencing within the paper and adding citations). Also I have a few more questions that occurred to me when reviewing the paper that I’d like to see addressed.

Can the authors comment on the chosen initial distribution of frazil ice crystals? I was expecting to see a sensitivity study of this chosen distribution and quantity of suspended crystals. I understand that this study is focussing on the interactions between crystals and the hydrodynamics with no thermodynamics implemented. Did the authors test other initial conditions? I think the paper needs a statement/discussion on the validity of the chosen distribution to allow the reader to understand the context of the simulations and to allow for the interpretation of these results amongst observations of frazil crystals within polynyas and other model simulations. For example in our paper Heorton (2017) we have many examples of the crystal concentrations at depth, although this is a 1D thermodynamic process model.

Can the authors confirm whether they focus on latent heat or wind driven polynyas? The temperature conditions suggest a latent heat polynya where the temperature of the ocean restricts the growth of ice. But for these conditions what is the expected frazil crystal desnsity and how physically realistic are the choice of crystal concentration and ocean temperature? I realise that these quantities are of secondary importance when thermodynamics are ignore and you focus on the crystal/flud interactions. However an idea of context will help future work where others may wish to compare your results to observations or other models.

I am also interested in the limitations of the model in terms of the intial conditions and time span. I will give some context to my question: We found in our paper that steady state condtions were possible for a wind opened polynya when modelling the thermodynamics properties of ice crystal growth (I am aware that you model a latent heat polynya). A crucial part of the steady state was secondary nucleation of small crystasls. This is crucial as small crystals have lower 'floatability' (as the authors here call it, a useful term) so can be more easily mixed downward, and thus replace the larger crystals that reached the surface (and were removed from the simulation in our model). This steady state was able to persist for several days of model simulation for particular model parameters and atmospheric forcing. However when chosing other parameters (see the results table in Heorton (2017), such a state was not achievable in the model. For example when reducing the OML turbulent mixing rates it was possible to precipitate all the frazil crytals to the surface and thus break the model. Balancing such phenomena was a major challenge of our model development and therefore I ask whether similar limitation were present in the authors model which contains very sophisticated modelling of the ocean turbulence and small scale flows. In particular, are there cases where all crystals collected at the surface? which model run has the greatest time variation in the vertical distribution of ice crystals? What are the model limitations due to the lack of thermodynamics? As the OML supercooling and secondary nucleation driven frazil crytal 'blooms' occur on time scales shorter than the model simulations shown here, how to the authors think that such processes will integrate with their model. I considered asking you to include time series plots alongside figures (5,6,9,10) to show how much variation occured for each simulation, though I don't think this is neccessary as the rest of the paper is so well presented and very extensive. However I think it a good idea to indicate how much variation from the initial conditions there are, for example how long did the surface increase in c3 for Ua=30 m/s Ta = -1.5 deg case in figure 6 (g,h,i) take to form and how stable was the feature? Did c3 continue to rise throughout the model run (with the layer of increased c3 getting increasingly shallow with time) or is the feature in the figures semi-stable? Similarly with figures (7,8,12), at which time point did such features start to occur and did they then remain for the rest of the model run? When running our model of frazil crystals in the OML, two runs with differing spatial distributions of crystal concentration, typically had differing patterns of time evolution. Do the authors find the same with their model?

Minor points

Pg1
L22 The use of 'obviously' is not very helpful in section with no references.

Pg2
L1-2 I agree with this sentence but it needs references for the situations listed. For example - Air-Ice-Ocean Interaction McPhee 2006

L3-5 again this sentence is good, but I'd like to see some references.
L5 'distinctive feature' definitely needs a reference

L11 and L 16 I think here you refer to sea-ice-climate models, or the sea ice component of a climate model
L 17 a reference to these observations is needed here where they are introduced
L 20 again a reference for the additional physical processes that are caused by the presence of frazil ice are needed.
L 28 I see you have the references included here. A link from the above paragraph to the area of the paper with more detail is needed to aid a reader seeking greater detail.

Pg4
L 19 - 21 consider splitting this sentence up as it is difficult to read. Also this sentence describes the limit of the presented modelling study. Crystal thermodynamics are not implemented. This needs to be very clearly stated.

Pg5
L5 'an earlier idea by' to be removed.
L 12 can you add a definition for the Langmuir stability length
L 26 whilst you have a description of Langmuir turbulence above, a definition of Langmuir circulation and transport will be helpful here.
L 30 I see here you have linked the Langmuir circulation and turbulence. As this section contains many different terms, It would aid the reader to have definition and physics behind Langmuir circulation/turbulence/transport defined at the beginning.

Pg 8
L 18 Does this description of low wind conditions over polynyas come from an observation or your results? Can you describe or give a reference.

Pg 9
L7 Can you expand on 'preferential concentration of frazil within the turbulent OML should be limited to the largest crystals ' please? Do you mean that due to the increased rising velocity of larger ice crystals one only needs consider the largest crystals? If so I find this alarming as previous results show that a there are vastly greater numbers of small crystals for certain cases, particulalry at depth.
Equation 1-4, as the model description is complex, I would like you to briefly describe what physical quantity is being conserved in each of the governing equations. This will greatly aid the readability of the following sections.
Pg 11
L 25, which equation describes the horizontal mixing?
Pg12
Equation 12 in which equations are these transfer coefficients implemented?
Pg 13
L5 can you provide a reference for the Schiller Naumann model and use for frazil crystals.
Pg 14
L 6 what observations?
Pg 15
Figure 4 In my experience frazil volume fractions rarely exceed 0.25, this is what is expected for grease ice, and within the OML will be lower. What is the application of the parameterisation in this figure where a volume fraction of 0 to 0.5 is presented?
Pg 16
L 13 I see the choice of frazil crystal radius is sensible when considering the terminal velocities. However how does the choice of crystal classes and initial concentrations compare to previous studies? A resultant ice thickness of all suspended ice crystals of 0.5 m seems to be a large volume

of ice to be suspended within the OML. Our work with frazil crystal modelling with thermodynamics suggests values or around 0.1 m (Heorton 2017). Also with climate models the initial stages of ice growth typically result in 0.05 m of new ice. Is your chosen crystal distribution specifically for a latent heat polynya? I realise that your model contains no thermodynamics so you need to seed a high amount of crystals.
L 17 did you vary the frazil ice concentration at all?

Pg 18
Table 2 Are all parameters other than Ua and Ta derived from these choices and other model parameters? If so can you say so in the caption.

Table 3 Can you expand on the what the F symbols on the left of the table indicate within the caption? If you're including this table to inform the reader about the what was included in the runs, please make it easy to read!

L 7 is the wind aligned with the x or y axis?

Pg 20
L5 I doubt there are any equilibrium profiles of ice crystals. Can you comment on this, what are the time variations in the mean contours? Are the variation plots spatial or time variation? For your model with no thermodynamics I expect all the frazil ice to eventually be driven by buoyancy and collect at the surface.

L7 are you plots in figure 6 time averaged over 18 hours? Over this time scale I would expect a large change in vertical concentration. However my experience is from a thermodynamic model focusing on supercooling and ice crystal growth. Can you comment on the variance in time of the spatial coverage of ice crystals for the cases presented in figure 6?

L 31 as these patterns continually evolve in time, do the mean (over x and y directions) vertical profiles of crystal concentration and ocean velocity also vary in time? how long do the spatial patterns take to form?

Pg 21
Fig 7 what time point were the snap shots taken? How representative are these snap shots of the model run? How do they compare to initial model conditions? How long do they take to form?

Pg 22
Fig 8, similar comments to fig 7. These two figures are well presented and look great, but I find myself asking how representative are they of the whole model run. Additional information about the temporal changes observed in the results will help.

Pg 28
L2 which studies have shown this?

Pg 29
L9 is this statement referencable?
L14 can you link back to the the figure or discussion where this is presented?
L 34 is your discussions of high and low ice concentrations here relating to the difference between ocean with suspended crystals and a grease ice layer, or are you referring to the total ice concentration within the OML?