

Interactive comment on “Frazil-ice growth rate and dynamics in mixed layers and sub-ice-shelf plumes” by David W. Rees Jones and Andrew J. Wells

Anonymous Referee #1

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This paper presents theoretical ideas that are topical and the subject of a variety of recent modeling papers of interest to The Cryosphere. The writing is extremely clear, concise and the free from errors. This clarity meant that I learned a good deal from this paper, and its abstract and conclusions are particularly succinct. The authors review the topic fairly, assigning proper credit.

The authors introduce three different parameterizations of crystal growth, and nicely fit previous work (including their own) into these three parameterizations. They then compare the effect of these parameterizations on the dynamics of overall ice production. Numerical methods are used to solve the governing equations, and the new approach

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essentially increases the number of crystal size classes of a previous model to pseudo-continuous. They then consider various aspects of the growth of a body of frazil ice in a “well mixed” layer and in a buoyant plume.

The only two substantive comments I have relate to the later sections of the paper. First, in section 3, I would have liked a very precise definition of what the authors mean by a “mixed layer”. I assume temperature and (if appropriate) salinity are uniform in their mixed layer. But what about velocity, or is a stagnant layer only being considered? The authors mention that they take depth-averaged frazil concentration in the plume model, but what is the assumption for their well mixed layer?

Second, in abstract the authors state that they “apply our model. . . . to a buoyant plume under a floating ice shelf. ” However I was confused about what aspect of their model they were applying since Fig 8 seems to be a sensitivity test to changes in parameters in SJ04 and SO94. It is not clear to me how the development in sections 1-3 are incorporated into the buoyant plume model, and I was disappointed that this was not demonstrated more explicitly. Perhaps this could be easily clarified by explaining how f1 to f3 fit within SJ04 and SO94.

Technical Corrections p. 3, line 25: Does the expression for Nu come from Galton-Fenzi et al (2014)?

p. 3, Line 26: “diffusivity of salt in water”

p. 4, line 9: I would have found it reassuring to be told that C is the volume occupied by ice crystals per unit volume of mixture

p. 5, line 29: please comment on the consequences of ignoring flocculation and crystal break-up.

p. 5, line 32: possibility of confusion due to use of δ for delta function and thermal boundary layer, possibly resolved by a subscript on thermal boundary layer.

p. 6, line 1: possibility of confusion due to use of ε for turbulence intensity.

p. 6, line 15” “calibration”

p. 7: section 3: Please define precisely what you mean by a “mixed layer” in terms of profiles of temperature, salinity, velocity, ice crystal concentration, etc. How would the presence of waves affect your “mixed layer”?

p. 9, line 4: “D = 1 m” I assume it is 1 m?

p. 11, line 13: Contradictory statement that sediment would act as nuclei since you state on p.6, line 5 that Daly expects this to be unlikely.

p. 11, line 26-31: “experimentalists”? Which experiments are you referring to in this paragraph?

p. 12, line 9: 1 m/s is a rather rapid current for the ocean.

Fig 5 & Fig 6: cropping of symbols on abscissa

p. 13, line 9: why is f_2 called the first growth law? Is it not true that $f_2 \approx 1$, rather than $\ll 1$ (see p.3)

Fig 6 caption: by “crystal size” I believe you mean crystal radii?

p. 14, line 4-5: Please outline how equation (25) is manipulated. I assume equation (24) is substituted into (25) and integrated?

p. 15, line 6: please remind the reader that U indicates turbulence intensity and n_{max} indicates secondary nucleation.

p. 15, line 7-13: is it possible to discuss the experimental behavior of secondary nucleation in relation to the model predictions?

p. 16, Table 1: it would be helpful to have the parameters names on the table as a reminder to the reader.

p. 17, Section 4: As stated above I did not understand because it seemed to me that the authors had introduced three growth models, and then did not use any of them in

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their frazil-laden plume study. Instead they ran sensitivity tests on two “old” models.

p. 18, line 16: “narrow slightly”? Do you mean D becomes slightly smaller?

p. 18, line 20: supercooling is “similar” to what?

p. 18, line 34: “(panel g)”? Do you mean panel c?

p. 19, Fig 8: why is f in the legend since it does not seem to change between model runs? Should it say f1 or f2 or f3?

p. 19, Fig 8: In the caption we are told that calculations with f1 are similar to SO94. But surely it would be more relevant to this paper to show f1 results, rather than a sensitivity study of an “old” model?

p. 18 & 19, Fig 8: I suggest reminding the reader which is the “slow” growth and which is the “fast” growth model by explicitly stating this in the legend. I got rather confused in the discussion on p. 18.

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