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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.

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I (Ken Hughes) was lead author of the paper on which a number of comparisons are made in the current manuscript. Consequently, I have a unique perspective on this paper. I have included a number of comments that would improve the clarity of the manuscript, which I read with interest. I am not, however, an official reviewer, and so am providing these comments for the authors' reference.

The authors modify an existing two-dimensional, time integrated ISW plume model to (i) test its ability to recreate observed supercooling and SIPL thickness observations and (ii) via a rigorous sensitivity study, investigate which parameters have disproportionate

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influence on the spatial distributions.

The idea behind the study is worthwhile as McMurdo Sound provides arguably the best location against which an ISW plume model can be test. Indeed, the two-dimensional model is a noticeable improvement over the 1D approach used by Hughes et al. (2014).

Major issues

Line 5.27 "some tuning of model parameters" overlooks some key details

1. Why is the outflow only 3km? Observations in Hughes et al have ISW outflow adjacent to the ice shelf across the whole Sound. 2. There is no mention that frazil ice classes in this paper are well below the mode size predicted by Hughes et al. (2014) of 2.5mm radius

Line 2.25 Somewhere in the paper, a description of the plume dynamics beneath the sea ice is warranted. Hughes et al. (2014) used a constant plume thickness out of necessity, because one-dimensional models can't easily deal with a lack of forcing (ie a gravitational component beneath a slope). It appears from Figure 1 that the plume in this paper tends toward geostrophic balance. Is this true? Put another way, does having two spatial dimensions and the time dimension avoid the issues faced by Hughes et al.

Line 6.29 Although the overall spatial distribution of SIPL may be graded as "excellent", it predicts a large areas where the thickness is 15m, twice the observations. Similarly, the "poorly" graded NVM model is closer to observations near the outflow. In other words, comparison between the models in terms of SIPL is not a simple as calling one "excellent" and the other "poor".

There is no citation, let alone discussion, of Holland and Feltham (2005, doi:10.1017/S002211200400285X), which specifically deals with the issue of the vertical distribution of frazil ice.

On a similar note, there appears to be no real attempt to constrain Z*, the suspen-

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sion index. A large range is proposed, but the reader is not given any idea what a reasonable value is.

Minor issues

Line 1.13: this is not a new frazil-ice-laden ISW plume model, but rather an existing ISW plume model with an improved frazil-ice treatment.

Line 1.15: This sentence should written as the dependence of supercooling in MMS on the SIPL thickening rate, not the other way as it is currently. In other words, the current sentence says the dependence of x on y is investigated, yet two sentences later, it says x can be expressed as a function of y.

Alternatively, reword to something like "Using this model, we test how ISW supercooling in MMS influences SIPL thickening rate".

This issue also arises for the heading of Section 4.2

Line 2.10 A reference to Galton-Fenzi et al. (2012) is warranted in this paragraph as they develop a three-dimensional model with frazil ice dynamics. It is not a plume model, per se, but it is does involve a a non-vertically-uniform approach to frazil ice dynamics.

Line 2.16 There is a slight issue with using MMS as an example for areas where marine ice production should be reassessed as it is no longer termed marince ice beneath sea ice.

Line 3.16 Hughes et al. (2014) improved upon the method of using T_sc at mid-depth. This is noted by Cheng et al. (2017) but is not noted here.

Line 4.30 Standard notation for ISW plume models has basal freezing as m' and frazil ice growth as f'. Is there a reason that here basal freezing is f' and frazil ice growth is omega'?

Line 7.11 Describe the type of observations and model results. Something like "...

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investigated with satellite altimetry and two-dimensional model results." (wih the actual adjectives based on the respective, cited papers)

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