Dear authors,

I have now attentively read your responses to the two reviewers. As you will recall both reviewers recommended that the paper should not be accepted for publication in "The Cryosphere", and therefore remain in the "Discussion" stage. I felt like you should have a right to answer to their comments since a) this is not my direct field of expertise and b) some of the comments might do with a counter-argumentation.

From my analysis of the reviewers comments, the main criticisms were as follows (partly converging towards my initial comments):

1. Inadequate use and interpretation of the mushy layer concept, and partly erroneous description of its physics
2. False bottoms mainly grow from fresh under-ice water, so this is where the "mushy layer" theory and equations would probably be less appropriate
3. Crystals should float up under buoyancy and grow downwards from there
4. Topic is too narrow, the extent of the phenomenon insufficiently documented, and probably not appropriate for present-day conditions
5. There are not enough additions or insights that should be the characteristic of a proper review paper

In your answers to the referees you introduced amendments to the text from a limited fraction of the questions and comments. For the rest, you simply argued that you did not agree with the reviewers, and, in several cases, that they should go and check in the appropriate literature.

From my point of view, there still remain problems with the discussion on density instability (not that I don't believe it exists) and the way it is presented and referred to the work of Martin and Kaufman (1974). There is obviously a confusion in the text between increase or decrease of density with cooling of "brackish" water. The effect of cooling on the density will also very much depend on the level of "brackishness", and the definition that is given to it. The example of figure 7 in Martin and Kaufmann refers to fresh water, the situation would be different for salinities above 24‰, where the freezing point curve goes below the curve of maximum density…I am fairly convinced that the under ice water is much closer to the fresh-water case, and that the hypothesis of M and K holds true, and explains the initial convection of supercooled water at the interface. It is less clear however for me how the density inversion in itself is a trigger to crystal growth at the sea water-fresh water interface. This brings me to another point raised by the reviewers (and that I raised myself before) , that supercooling should lead to frazil ice crystals growth, and those should immediately rise in the column, since the density difference between ice and water is much larger than the $\Delta\rho$ induced at the interface under the double-diffusion process. As argue in your replies, there is indeed the observation of the false bottom formation in M and K experiments…However, carefully looking at the pictures in their Figure 5 and reading the associated text:

*"First, **14h** after the experiment began, the fresh water reached its freezing point. On days **2** and **3**, as figures **5 (b)** and **(c)** show, we observed both the growth of thin vertical crystal sheets from the bottom of the granular ice, and from our temperature and salinity data, supercooling at the interface. Our observations suggest that the fresh-water layer was convectively unstable, with the supercooled water*

rising from the interface to the surface, where nucleation occurred. Second, at 1500h on day ***3,*** *both the Plexiglas walls and the thermocouple mount served as nucleation sites for the supercooled water generated at the interface.* ***As*** *figure* ***5*** *(d) shows, thin vertical plates of ice formed on these sites near the interface. These ice plates subsequently began to grow laterally. This lateral growth, combined with some filling in of the interior of the tank, created a horizontal ice sheet on day* ***14.*** *Figure* ***5 (f)*** *shows the appearance of the ice shortly after the ice sheet formation."*

It appears clearly that the build-up of the false bottom heavily relies on lateral boundary conditions, and that is started after convection of supercooled water upwards and filling up of the whole under-ice water with large and long crystals. These might have clearly helped in damping the rise of the false bottom crystal growing laterally from the side at the fresh water - sea water interface. I remain sceptical that such specific and complex growth conditions could be faithfully represented by a classical "mushy layer" model.
There is also still a comment from one of the reviewers on the adequacy of the use of a full mushy layer model for ice growing from a freshwater body (especially, in my opinion, if the ice builds up quickly in most of the reservoir as an intertwinning of large crystals from supercooling in a weak-directional pattern, before the false bottom starts to develop).
Finally, there remain also the unlikeliness of simultaneous growth under heat conduction at the upper interface and supercooling at the lower interface, which is presented as one of the new inputs of the paper.
I have therefore decided to take my final decision after I receive a general feedback of the two referees on your answers to their comments. I hope you will understand that this is to ensure that I do not take my decision lightly, only on my own perception.

Thank you again for fostering the discussion,

Jean-Louis Tison