The second round comments on: Evidence suggesting frazil ice crystal formation at the front of Hisinger Glacier in Dickson Fjord, Northeast Greenland by Fleur Juliëtte Rooijakkers, Ebbe Poulsen, Eugenio Ruiz-Castillo, and Søren Rysgaard

by Sergei Kirillov, Centre for Earth Observation Sciences, University of Manitoba

In this paper, the authors examine an interesting dataset obtained in the marine-terminated glacial fjord in East Greenland and try to characterize the processes occurring at the front of the glacial terminus based on the obtained vertical profiles of temperature and salinity, and also some basic isotopes.

I admit that the authors have considerably improved the paper in terms of terminology that made it much clearer. The new version of the text is much easier to follow and understand. Below there will be my suggested minor edits, but I would like to start with one intermediate and one major concerns that I still have.

The intermediate concern is related do the TS-analysis presented in chapter 3.1 (Lines 135-160). I am not a big fan of TS-analysis in general, but I don't think the authors use it properly. And my main criticism is about the idea of using this analysis for the surface and pycnocline/thermocline layers and identification of source waters. I think the TS method may be a good tool when examining the contributing ratios within the triangle of mixing for intermediate or deep water. But the surface layer is largely controlled by air-ocean interaction, e.g. radiative heating (that may vary spatial in the fjords because of their steep walls) or local sources of terrestrial discharges. All this may result in, for example, considerable fluctuations of water temperatures and all discussion about the positive and negative slopes and trends shifting direction (Lines 151-153) largely uncertain. I would recommend the authors either exclude this part completely or carefully reconsider the results of the analysis they conduct here.

The second concern is about the frazil ice story. In the new version of the paper, I found nothing that would have changed my mind and made me accept this idea. Here I will try to explain again why I doubt the formation of frazil ice takes place in the vicinity of the terminus or, if it does, can't lead to warming of the Polar Water layer up to 40-km away from the glacier. The frazil ice forms when two waters with different thermohaline properties forms the interface favourable for double-diffusive mixing which is molecular i.e. mainly laminar process. The presence of such interface is possible when fresh and warm (compared to ambient water) subglacial discharge meets salt cold Polar Water. However, due to the large difference of densities, this subglacial plume raises quickly to the surface in a form of turbulent vertical flow. Turbulent flow suggests that salt and heat are exchanged at the same rate between the plume and ambient water and, therefore, does not create double-diffusive interface. Under these conditions, some ice crystals still can form and even stay intact until they reach the surface layer. However, I expect that most of the crystals will be instantly dissolved back into the liquid phase as turbulent flow move them from the zero-degree fresh plume into the saltier ambient environment taking back the latent heat that was released a bit earlier.

Let's see further. For a moment, let's assume the authors are right and the ice crystals remain in the plume (undissolved) separated from the ambient waters that receives and holds all latent heat. How do the authors explain spreading this heat up to 40km from the terminus? What physical mechanism is responsible for such redistribution? Strong horizontal (tidal?) mixing? But this implies strong turbulent mixing at the terminus as well and, consequently, moving the frazil ice crystals from the plume to the surrounding water where they get quickly dissolved.

And one more speculation about the thermal budget of frazil ice. The authors reported that the total volume of meltwater to be refrozen to explain the excess heat in Polar Water layer is equal to 3.4e7 m3 that gives 3.4e10 kg. With the latent heat of fusion 3.3e5 J/kg it gives 11.2e15J. Implying the used fjord width of 3,000 m, surface layer thickness of 15 m, and let's say 10 km distance from the terminus, the absorption of such amount of energy when the crystals melt at the surface should have resulted in water temperature decrease in the entire surface layer by +5.9C! And this is without considering the supraglacial cold freshwater discharge. Figure 4a shows everything but not such a cooling next to the terminus. In fact, the cooling effect must be even larger because the plume is hovering near the glacier, not over 10km that I used in my rough calculations. The only possible (I am not saying correct) explanation of the absence of such cooling effect is if the warming of Polar Water is a result of much longer process and if that warming took many years, not one season. But by suggesting this you must admit fully stagnant conditions in the fjord.

My conclusion here is that the author have to explain all these physical discrepancies or to admit that there is another more plausible explanation when the observed positive temperature anomaly in the Polar Water next to the glacier or negative anomaly in PW at the distance from the glacier (beyond 40km) were formed upstream and were just advected into the fjord. If the authors follow one of these options and modify the manuscript accordingly, I don't mind to consider my concern as a minor one in my final report and don't need to see the manuscript again.

Some suggested non-critical edits:

Line 66: I have nothing against Nix Geilfus, but is it really the first time the VSMOW was used and reported?

Line 86: I don't think that more than 200km distance between Zackenberg and Ella is good for sharing the fluxes, but it's probably fine if you use average values.