Review of manuscript tc-2018-141

by Sönke Maus

This is a review of the manuscript *Physical and optical characteristics of heavily melted 'rotten' Arctic sea ice* by C. M. Frantz et al.

Below I cite from the Cryosphere Discussions manuscript tc-2018-141 in *italic font.*

I Summary

The paper presents an analysis of the physical and optical properties of heavily melted Arctic first year ice. At present very little is known about the physical properties of such ice that plays an important role for, among other processes, radiative transfer. The topic is thus absolutely worth publishing in The Cryosphere. Beyond standard measurements of physical properties on bulk ice samples (temperature,salinity,density) also an analysis of 3-d tomographic observations of the microstructure is presented. The article is well written and structured into the sections 1.Introduction, 2. Materials and methods, 3. Results, 4. Discussion and 5.Conclusions.

I find the manuscript interesting and well written. New observations of sea ice properties from the onset of melt to its rotten state are well presented and analysed in terms of radiative transfer. I found two weaknesses, that should be straightforward to address, which would improve the quality of the manuscript.

1. As described in more detailed comments below, there are some issues with the sample treatment, especially the flooding of samples with brine and DMP, that should be addressed. When a centrifuged sample is re-filled with a liquid, it is rather probable that part of the pore space is not filled, creating artificial air pockets or bubbles. The creation of extra bubbles may affect two of the microstructure metrics addressed by XRT, and it may also influence the interpretation in terms of scattering model results. While the authors mention the aspect of bubble formation during flooding, they could have provided a quantitative evaluation. The question could for example be addressed by a more detailed analysis of the micro-CT derived different open and closed porosity fractions (air, brine, injected DMP), rather than discussing just total porosity.

2. Anisotropy of pores and inclusions is a rather fundamental aspect of sea ice microstructure, and it is likely to play a role for many processes as well as radiative transfer (Katlein et al., 2014). Anisotropy of sea ice microstructure is not well documented yet and it is an important contribution of the manuscript to address it. However, the presentation of anisotropy in the manuscript is inconsistent, see below. To a certain degree this inconsistency appears to come from adopting the definition and determination of anisotropy as proposed by Lieb-Lappen et al. (2017).

I would like to recommend the manuscript for publication, after these too aspects have been addressed.

II Specific comments

1. Introduction

P 2, L36-42 –> *In general, the connectivity of an ice cover is known to...* –> I would put the paragraph on ice dynamics (L50-59) here together with the mentioned processes, and rather join the sentence on permeability (L40-42) with the next paragraph (L43-49) on this topic.

P 2, L36 –> *Increases in ice permeability result in an increase in the amount of surface meltwater...* –> if the amount increases may depends on other factors, so better use 'flow rate'

P 2, L36 –> *As a result of the notable connectivity of its microstructure* –> Better 'connectivity of its pore space'

2. Materials and Methods

P 3-8 –> This section describes the samples taken on three sampling dates. It would be helpful for the reader to summarise the characteristics (date,thickness, air temperature, ice salinity, freeboard) in a table.

P 8, L 148 –> I assume that pack volume was estimated for density measurements. Could you estimate the accuracy of these measurments?

P 8, L 150 –> The mentioned accuracy seems too good for a hand-held instrument. According to my information (handbook) the YSI Model 30 has a salinity accuracy ± 2 %, not ± 0.2 %.

P 8, L 153 –> To which thickness were thin sections microtomed? Could you mention a reference?

P 9, L 161-163 –> The working temperatures were -5, -2 and -1℃, and the same storage temperatures were chosen. However centrifuging was performed at the same temperature of -5℃. This may effect the microstructure considerably (e.g. for -1 ℃ brine volume might decrease by a factor of 4). Can you comment on this effect? As you mention, that the brine has been collected for further analysis, you can do so by asking: does the brine salinity correspond to the equilibrium brine salinity at the working temperature?

P 9, L 165-170 –> What is the reason to use DMP casting on the centrifuged images? This clearly complicates the analysis of XRT images, but an advantage is not mentioned. Note also that, as for the flooding with brine, flooding with DMP is likely to entrap air and thus overestimate the air porosity.

P 11, L229-232 –> I assume that the described flooding requires samples to be placed into a box or tube, which raises some questions: Were samples taken out of the flooding tube again for optical measurements? Also, I have myself attempted such flooding of centrifuged samples, but never managed to refill the original pore space - there are always pores that are not refilled. Do you have data to assess this question as for the DMP? E.g. a XRT-scan?

P 11, L239 –> The drainage in the laboratory would produce 'rotten' ice with a lot of air voids, while in the field ice may 'rot' differently, with internal melting increasing the brine/liquid content. As air voids are expected to be better scatterers, this difference should be mentioned and addressed in the discussion of Figure 10, see below.

3. Results

P 14, L289 –> How were the relative measurement errors for density calculated?

P 16, L322-325 –> The median is often a better description of a characteristic pore scale than the mean. It would be very helpful if you could plot your size distributions/histograms below the images in Fig.7 .

P 19, L364-366 –>My experience shows that the ratio of centrifuged to entrapped brine is typically in the range 0.5-4, with a value of 2 being most representative around a porosity of 0.1. So far data are limited, yet results are similar for young and old ice, showing that the ratio decreases with decreasing porosity (Maus et al., 2011, 2015). I therefore recommend to separately plot the relationship between open/closed ratio and total porosity. Doing so, I would prefer to plot the information as a fraction of open porosity to total porosity, rather than open porosity to closed porosity. The latter may diverge and makes it difficult to find a good plot scaling. There are also other arguments to do so, if one wants to interpret the results in terms of percolation theory.

The open to closed porosity ratio in this study may be biased by two factors: on the one hand, the DMP flooding may create artifical air bubbles. On the other hand are certain fractions of air bubbles and in particular disconnected brine inclusions not detected with the effective resolution of the micro-CT. The large values of open/closed porosity ratios (10-100) may therefore be in error. How much large could this error be? Could you address the question, how much artificial closed air pores the DMP intrusion may generate? This could be done by distinguishing between open and closed pores for air on the one hand and and brine+DMP on the other hand.

P 20, L378-388 –> The anisotropy measure from Lieb-Lappen et al. (2017) is used here. These authors define it this way (page 28, upper right paragraph): *A polar plot encompassing all the mean intercept lengths is created by creating an ellipsoid with boundaries defined by the mean intercept length for each direction. Any given ellipsoid can be characterized by a matrix, and the eigenvalues for this matrix are calculated, which correspond to the lengths of the semi-major and semi-minor axes. The ratio of the largest to smallest eigenvalues then provides a metric for the degree of anisotropy, with 0 representing a perfectly isotropic object and 1 representing a completely anisotropic object.*. The authors do not give any formula beyond this description, neither do they refer to any publication about (the apparently applied) mean intercept method in microstrcture analysis. There seems to be an error here, because when anisotropy is projected to the range 0-1, the ratio of minor to major axis length should the the correct definition. Also, based on the definition of anisotropy as an axis length ratio, it would be vice versa to the description in this paper and in Lieb-Lappen et al. (2017): a value of 1 would present a perfectly isotropic object and a value of 0 an infinitely long anisotropic pore.

I think therefore that the whole description of anisotropy should be checked. It is actually intuitively surprising to find the highest anisotropy in the mid horizon (as the authors as well as Lieb-Lappen et al. (2017) describe), rather than near the bottom of sea ice, where brine channels and seawater are well connected.

It is finally worth mentioning that anisotropy, if defined as minor to major axis ratio in this way, would be a problematic measure when considering through-sample brine channels. For this case the major axis is limited by the sample length and the measure would be size-dependent.

P 22, L424 –> *Submerged cores appear to have more porous ice structure.* –> Could this be supported by some of the XRT masurements? Proposing this and the following from only the photographs sounds a bit speculative.

P 22, L424 –> *By June, the salinity profile shows freshening at the ice 434 bottom, likely associated with the onset of bottom ablation.* –> Another explanation could be, as the authors proposeed earlier, that this warmer ice has wider pores and looses much more brine during sampling. Fig. 9c actually supports this. If true, then the ice may only have an apparently lower salinity. This question could be addressed by a closer look into the XRT images.

4. Discussion

P 22, L424 –> *In particular, the micro-CT work is useful for sampling much larger sample volumes, and thus central for estimating size and number distributions for the July 465 ice.* –> This claim raises several questions: 1. How may the number density of inclusions be effected by the DMP flooding process? 2. The micro-CT measurements were limited to a voxel size of 280 micron - how can optical and micro-CT number estimates be combined and compared?

P 23, L448-451 –> *Normally, sea ice with significantly smaller bulk density would be expected to float higher in the water and thus have larger freeboard. But the density reductions that occur during advanced melt result from large void spaces within the ice that are typically in connection with the ocean. As a result, such ice can have small freeboard, even if total ice thickness is still relatively large.* –> I would interpret the low densities rather due to rapid brine drainage during sampling, creating apparent low densities. This question should be further adressed. Again, the micro-CT observations may be used here for clarification, by splitting them up into brine, air and DMP porosities.

P 24, L488-491 –> *Our findings are consistent with those of Jones et al. (2012), which used cross-borehole DC resistivity tomography to observe increasing anisotropy of brine structure during spring warming. In that work, the brine phase was found to be connected both vertically and horizontally and the dimensions of vertically oriented brine channels gradually increased as the ice warmed.* –> I agree, this is consistent, and it is what one intuitively would expect. However, in the results section (P 20, L378-388) you say something different. This again underlines the above mentioned inconsistency in the anisotropy description from Lieb-Lappen et al. (2017).

P 24, L492-493 –> As you have results from microscopy and micro-CT you could quantify this results. E.g. plot both size distributions in a histogram. This would indicate to what degree the methods are comparable in the overlapping regime, and what resolution a CT-Scanner should have.

P 25, L518-522 –> As mentioned above, the drainage in the lab would produce 'rotten' ice with a lot of air voids, while in the field ice may 'rot' differently, increasing mostyl the brine porosity. Could you comment on the question, to what degree the applied model treats air and brine scattering differently?

5. Discussion

P 26, L538-542 –> See above note: I would interpret the low densities rather due to rapid brine drainage during sampling, creating apparent low densities.

P 26, L548 –> *critical difference* –> In terms of....scattering?

III Figures and References

Fig. 6 –> It would be nice to have the measured freeboard indicated in the different profiles.

Also an easy-to-see distinguishment of ponded and unponded ie would be helpful.

P 25, L520 –> *Fig. 10, dashed curve* –> the 'dashed' is difficult to see

References

Katlein, C., Nicolaus, M., Petrich, C., 2014. The anisotropic scattering coefficient of sea ice. J. Geophys. Res. Oceans 119 (119), 842–855.

Lieb-Lappen, R., Golden, E., Obbard, R., 2017. Metrics for interpreting the microstructure of sea ice using x-ray micro-computed tomography. Cold Reg. Sci. Technol. 138, 24–35.

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