Review of tc-2014-88

This is a review of the manuscript *Imaging air volume fraction in sea ice using non-destructive X-ray tomography* by O. Crabeck et al. Below I cite from the Cryosphere Discussions manuscript tc-2015-157 in *italic font*.

General comments

Summary

The paper presents an analysis of air volume fraction and gas content of seawater ice grown in an outdoor tank facility. The technique, X-ray tomographic imaging with a clinical CT, has not been applied before to analyse the air volume and air inclusions in sea ice. Most of the results presented are based on the results from the CT imaging, or on the comparison with other methods to determine air volume in sea ice. The paper however also presents measurements of gas content of bulk sea ice based on gas chromatography. These measurements are in particular compared with the CT results to draw conclusions about the formation and redistribution of gas bubbles in sea ice. It is further proposed that the results obtained for air porosity and bubble characteristics might be relevant for improved parametrisations of fluid transport through sea ice, and that the applied techniques are an important novel research approach.

The approach in the paper, to apply X-ray tomographic imaging with a clinical CT to the analysis of air pores in sea ice, is interesting. However, intuitively I would not expect that a clinical CT scanner with a spatial resolution not too far from a millimeter, is suitable to describe air inclusions, at least not in young sea ice for which mostly bubble diameters less then a millimeter have been documented in the literature (mentioned by the authors). A paper that introduces this method should therefore first demonstrate a proof of concept and, due to the limited spatial resolution, be careful in terms of any conclusions. In my opinion the paper leaves many open questions ranging from CT data acquisition to the interpretation of the results and their uncertainty. This is my first concern. My second is the effect of sampling and storage on results, in particular brine loss during sampling: How may it influence and bias the results both from tomography and the gas chromography? Third, I am missing a clear distinction between columnar ice and the granular surface ice, that appeared to form during the experiment through the infiltration of snow from below. Also the brine loss (producing features that may appear as bubbles) is likely largest near the bottom and surface of the sample, making it useful to divide the ice vertically into regimes and types. Finally, I am not sure, to which degree the results from the sea ice outdoor tank experiment, obtained for the case of thin sea ice with a relatively thick snow cover leading to snow ice formation, are relevant for natural sea ice in general.

Main concerns

I. X-ray tomography, description of methods and results.

1. At which temperature was the CT imaging performed and how long took it to a scan, including sample placement in the scanner? The authors mention that cores were stored at -20℃, yet do not provide such information. This is an important

issue, because any warming of the ice cores would imply remelting of brine and the formation of air bubbles.

2. Scanning time? I suppose that this was only a few seconds, but it is not mentioned. Please consider: could absorption of X-rays have created heating, internal melting, and thus bubble formation?

3. Spatial resolution. The authors report voxel volumina of 0.25 x 0.25 x 0.6 mm, and I suppose that 0.25 x 0.25 mm refers to the the horizontal pixel size within a slice of 0.6mm thickness, and the slices are stacked along an ice core. (This could be better described in the text where *Tomograms were acquired continuously every 0.25mm along each 0.6 mm-thick slice* is not clear to me). Most important for the interpretation of the results seems to me that voxel size is not the same as spatial resolution. Spatial resolution cannot be better than two times the pixel size, and is as a rule of thumb often just 3 times the latter when it comes to 3-dimensional objects. For the resolution along the cores (0.6 mm) this would yield a spatial resolution of 1.2 to 1.8 mm. However, when looking for some documentation of the Siemens Somatom Definition CT in the web, I came across that the instrument may oversample in the direction along the slices, yielding a better isotropic voxel size of 0.33 mm. (also here more information should be provided by the authors). In any case, spatial resolution is likely rather between 0.5 and 1 mm, rather than 0.25 mm. This should be clearly pointed out and discussed to some degree. It is for example possible to estimate spatial resolution from transects as shown by the authors in Figure 2d; Actually, the latter figure indicates that it takes roughly 3 pixels between a low (air) HU value near -1000 to the ice HU level of -100. Finally, other information could also be of interest for the reader regarding the spatial resolution (e.g., number of projections).

4. Segmentation of X-ray images into air/ice. The review of segmentation techniques (p. 5212-5213) is useful, but could be moved into an appendix (or even just refering to a review paper of them). While the authors explain their choice of the two algorithms used (based on subjective interpretation), this does not involve an uncertainty estimate. I therefore recommend to also report manual threshold estimates that are based on subjective interpretation of bubbles (as the authors reported when choosing the algorithms), to get an idea of the uncertainty. Due to the low resolution such a test is of particular importance. Due to the rather different nature in the formation and air bubble population it may also be useful to check if different thresholds are obtained for granular and columnar ice.

5. Air volume fraction (3.4.1). Mean and standard deviation of air volume fraction for the total ice cores are, due to the rather different ice types, not very meaningful. These values should be at leats reported for granular surface ice and columnar ice. I would, due to the potential of brine loss, also report a third type - the bottom layer, and possibly distinguish initial granular ice and later forming snow ice (see III below). In Fig. 5 this could be color-coded. This distinction between the ice types is also useful, because it seems likely that the CT spatial resolution is insufficient to properly describe air volume in columnar ice, while it might be sufficient for granular ice formed from snow.

6. Air inclusion morphology (3.4.2). As for the air volume fraction, mean and standard deviation would be much more informative when divided into different ice classes. In the frequency distribution plot (Fig. 9a) this could be color coded.

7. Comparison to density-derived air volumes (Fig. 10a and b). The authors claim *The density (M-V) derived air volume profiles were generally slightly larger (Fig. 10a) but the two methods generally agree* (p.5218, L21). However, there is just one profile with agreement (From Jan 25), while at the other two dates the density-derived air volumes often are several times larger than the CT-derived ones. For the columnar ice the difference seems to be roughly an order of magnitude. Also in this comparison one should distinguish between granular and columnar ice.

8. Image interpretation. The authors claim to show in Fig. 13 *the formation of a macro bubble by coalescence processes*, which is rather speculative, as the image is not a time-lapse image. It simply shows the vertical profile of several bubbles and it cannot be said if these bubbles are merging or splitting.

9. Grey image averaging (could be tried). As an overall assessment at that stage, I would not rate the clinical CT observations as suitable to derive proper estimates of air volume fraction in sea ice. This is likely due to limited spatial resolution. There might be an approach that the authors could try on the basis of their data, yet without performing a segmentation. Equation (1) for the tomographic intensity may be modfied to replace the linear attenuation coefficient μ as the volume averaged sum of brine $\mu_b v_b$, air $\mu_a v_a$ and ice $\mu_s v_s$, where v_x denotes volume fractions. This equation may then, using the derived v_b, and assuming suitable values for the attenuation coefficients of air μ_a, ice μ_s and brine μ_b (at the imaging temperature) be solved for v_a (note that $v_b + v_a + v_s = 1$).

II. Effect of brine drainage on results

1. An increasing air volume fraction towards the bottom (Fig. 7 and 8) is likely a consequence of brine loss during sampling. As mentioned, it is therefore highly recommended to report the statistics of the bottom layer separately.

2. As an example, I wonder if air bubbles shown in Fig. 9b, left slice for Jan 14, 3.6 cm from the surface (and thus 0.4 cm from the ice-water interface) are really enclosed bubbles. I would rather expect them to be emptied brine pores

3. Brine loss also leads to an underestimate of the brine volume V_b. This affects the computed saturation factor SAT_f. Please estimate this potential bias.

4. How does brine loss affect the results for the granular surface ice? This effect may be estimated by distinguishing the CT-derived air porosity into an open and a closed fraction.

III. Granular snow ice versus columnar ice

The authors describe (p.5226, L8) that the granular ice layer thickened from 0.5 to 4 cm during the experiment. As the maximum snow thickness reported was 9 cm for ice of 8 cm thickness (January 16), this snow ice has likely formed due to surface flooding combined to upward suction of brine into the snow by capillary forces. The authors mention Rysgaard et al. (2014) for a more detailed site description, where it is reported that the snow was removed on January 23 (i.e. before the 3rd core was taken), followed by 8 cm new natural snow fall the days after. I assume that this removal was performed for the whole tank and thus has also affected the results in this paper. This removal would have created upward movement of the freeboard, and thus potential drainage of brine from the granular snow ice that is no longer below water level.

1. With the above assumption one can expect 4 distinct ice types: initial granular ice (0.5 cm), granular ice formed from snow, columnar ice and highly permeable bottom ice. These ice types will differ in terms of microstructure, initial air content, potential sampling and storage biases (brine drainage, bubble formation) and detectability of bubble populations by CT. I therefore strongly recommend to distinguish any results for these ice categories throughout the paper.

2. Some observations are only available at coarse vertical resolution (e.g. saturation) averaging over several ice types and this should be clearly mentioned and taken into account in the discussion

3. At least for the bubble populations I would find it very useful to distinguish the initially formed granular (≈ 0.5 mm) surface ice from the snow ice that formed later.

4. The authors give some information about air bubble density and air volume fraction that distinguishes granular and columnar ice in Fig. 11. However, this figure puts a lot information into one plot and it is very difficult to read. Also Fig. 14a, providing the vertical distribution of air bubbles, is hardly readable and does neither separate different ice types.

5. In Fig. 7 the authors have distinguished ice of different characteristics. However, the red shaded layer appears to be somewhat arbitrary selected: how are upper and lower bounds of this layer defined?

IV. Relevance of the experimental results and conclusions for sea ice microphysics and chemistry in general
During the tank experiment with (i) snow ice formation (9 cm snow on 8 cm thick ice), (ii) artifical removal of the snow and (iii) recurrence of a similar snow cover, very likely considerable fluctuations in the freeboard, and thus vertical movement of brine. In particular the granular surface ice has thus formed under specific conditions, and any conclusion drawn may be just valid for this ice type. The following examples indicate the importance to account for this (as well as spatial resolution of the CT scanner and other methods) in the discussion.

1. The authors discuss air volume fraction, bubble formation and brine movement in detail for the categories saturated and subsaturated sea ice. However, saturation measurements have coarse (5 cm) verical resolution and often do not resolve ice types. In my opinion a discussion in terms of ice categories (i) granular snow ice, (ii) granular ice, (iii) columnar ice and (vi) columnar bottom ice would be more informative. As noted the ice types imply different ice formation processes, initial air content, bubble detection limits, etc... E.g., air porosity in the granular ice may simply be the remnant of snow porosity that was not filled by upward suction of

brine and flooding. In columnar however, many air bubbles are likely to have been below the resolution limit of the CT scanner.

2. The authors note (P.5223. L16) that *the relationship between saturation and air porosity in figure 12 is not straightforward* and propose an interpretation in terms of convection and nucleation of air bubbles (p. 5223, L17 - p. 5224 L24), where for example it is noted that (p.5224, L1-4) *Although the air volume fraction is low in these layers, it is somewhat surprising that the air volume fraction is >0....so one might expect these subsaturated layers to be bubble-free.* I rate this a bit speculative. What I would consider based on the figure is: the subsaturated samples all stem from the bottom regime, where desalination through convection and exchange with seawater below is present. However, from this ice also brine may have been been lost during sampling, which together with the cooling and storage process might have created what the authors call *micro bubbles.*

3. The other information that the authors highlight in figure 12 is: it appears that for large saturation (i) the latter is independent on air volume fraction (figure 12a), while (ii) brine volume, air volume and bubble diameter increase proportionally to each other. However, result (i) appears to me as a consequence of combining vertically better resolved air porosity with the saturation data of limited vertical resolution (e.g., Fig. 4). Result (ii) is interesting, but it should be more clearly pointed out that the relationship is just representing the ice grown by infiltration and flooding of snow, and for the specific conditions during the tank experiment.

4. The authors mention an *accumulation of bubbles nearest the ice-atmosphere interface* (p. 5226, L23) and discuss this in terms of gas fluxes to the atmosphere. However, due to the snow ice formation I feel that the term *accumulation* is misleading. These bubbles might simply be the snow porosity that was not infiltrated by upward movement of brine. They did not have to accumulate, because they were there.

5. In my opinion, the most interesting result from the paper is the apparent accumulation of air bubbles 3-5 cm above the ice-water interface (Fig. 7), and the interpretation by the authors that these bubbles stem from nucleation in the convective bottom layer. However, error bars appear to be relatively large. It remains to be demonstrated that this accumulation is a general result for growing sea ice. Also, it would be very interesting if the authors could offer an explanation scenario how these bubbles may disappear again, when the freezing interface is advancing.

Specific comments

P 5209, L 4 –> *dividing* I suppose you mean 'multiplying'. Otherwise this paper would require considerable recomputations.

P 5209, L 4 –> Could you explain how you arrive at 0.020 gcm^{-3}? Does the roughness of surfaces of a cube not influence the accuracy?

P 5210, L 23 –> *map* - image

P 5210, L 23 –> *U-channels* - what is this

P 5211, L 4 –> pixel or voxel?

P 5211, L 6 –> *pixels* - voxels

P 5212, L 5-6 –> Was this elimination just done for the purpose of segmentation?

P 5210, L 13-14 –> *exceeds 100* - maybe it also exceed 200? I recommend a different formulation.

P 5215, L 1 –> *sub-millimeter* - as outlined in general comments voxel size is 0.25 mm, but not spatial resolution.

P 5219, L 10 –> *accurately* - I would not use this word here as the CT does not resolve micro bubbles

P 5219, L 23 –> *generally agree* - this can hardly be said, as density-based air volumes are several times larger

P 5222, L 20 –> I cannot find where in reference Cox and Weeks (1983) a value of 21.9 mLL^{-1} for the gas content of instant frozen seawater is mentioned. These authors rather discuss and quantify how to determine gas content from density measurements and salinity measurements. Besides a correct reference please report the salinity and temperature to which this value refers.

P 5223, L 1-3 –> Note that brine loss during sampling, which is unwanted yet a process, will decrease V_b yet increase V_a.

P 5226, L 16-17 –> *suggesting the presence of coalescence processes, which we can clearly show using X-ray images* - Your images are no time series and what you can show is rather the vertical structure, and not if coalescence or splitting takes place (if it does so at all).

Technical corrections

Fig. 3 –> Error bars for T, S_i and V_b? Fig. 4 –> Correct legend
Fig. 7 –> Log scale might show data better; error bars for V_b lacking
Fig. 8 –> Perhaps invert colors
Fig. 8 –> Hardly readable
Fig. 12 –> Scale on axes should start at 0. Consider log axes.
Ref. Kawamura (1988) is missing
Ref. Lepparanta and Manninen (1988) is not used in the text