Dear editor and reviewers:

We would like to thank both reviewers for their constructive comments that have much improved our manuscript. We have sincerely considered each of the reviewers ‘comments and have endeavored to include those thoughts in the revised manuscript.

Specifically with respect to the reviewers' comments, we retained mainly four points, which were addressed in detail in the revised manuscript and in the response-to-reviewers document attached. First, it has been pointed out that the resolution of Medical Ct-scanner may not be sufficient to accurately resolve all sizes of air inclusions in sea ice and most applicably in columnar sea ice. Secondly, reviewers argued that the observed inclusions are mostly drained liquid inclusion and not actual bubbles due to brine loss during core sampling. Thirdly, the reviewers commented on the potential effect of storage (-20°C) on the size and amount of gaseous inclusions observed. The fourth and final overarching comment was directed to the relevance of sea ice tank experiments as analogs to natural sea ice.

In the initial reviewed version of the manuscript the voxel size was exactly 0.195 x 0.195 x 0.597. In order to improve the resolution at the behest of the reviewers, new image data were generated by changing the field-of-view and the image re-construction process. The combination of these procedures improved the resolution by 200% in the transverse (x-y) plane. In the revised manuscript, final image size is 1024 x 1024 pixels with a pixel resolution of 0.0977 mm x 0.0977 mm with an unchanged slice thickness of 0.6mm. All the data were then reprocessed at this resolution. The region of interest eliminated the imperfect edges of the core sample due to the sampling method. The image thresholding methods for selecting air pixels were also re-evaluated. Following the application of all these changes the overall profiles of air volume fraction and our conclusions thereon remained unchanged.

We continue to assert that the CT X-ray imaging instrument employed here is a useful tool to compute air volume fraction in sea ice samples. No other pre-existing method is able to quantify vertical profiles of air volume in sea ice. Profiles of air volume fraction in sea ice cannot be deduced from thin section analysis. Air volume fractions from density measurements are of such low precision they are unreliable at best in computing sea ice air volume fraction. CT X-ray imaging provides a relevant quantification of the vertical air volume fraction profile at a voxel scale of 0.0056 mm^3, which we assert is a dramatic improvement in resolution with respect to sea ice physical properties. Most bulk physical parameters in sea ice are measured in five-centimeter sections of vertical cores in a very small 1-2 mm thick subsample, which is currently considered fine resolution in the sea community.

However, the resolution of the CT X-ray imaging instrument in the revised manuscript may still not be sufficient to fully resolve the smallest air inclusions in columnar sea ice. To accurately resolve the smallest bubbles in columnar layer, the use of a Micro CT imager is probably more applicable. Of course using a Micro-CT imager would creates another trade-off between the resolution and sample size; Micro-CT imagers like the

Bruker SkyScan 1174 for example require additional scan time (and/or a cold stage), using samples no larger than 3 x 3 cm. This much smaller subsample size creates problems: (i) any sea ice core would have to be subsampled repeatedly at each depth to accumulate the image data necessary for full-core reconstruction of air volume fraction, (ii) the calculation of air volume fraction in that instance would likely be heavily biased by the occurrence of bubbles > 1mm in diameter in each 3 x 3 cm subsample, (ii) as with the mass volume technique, the cutting process will likely affect the calculation of air volume fraction due to the preferential cutting of larger air inclusions. Ideally, one would combine computed porosity and analyses from the Siemens CT- imager employed in our manuscript with some number of subsamples using a micro-CT imager for higher resolution morphometric analysis of the smallest bubbles; these considerations will be accounted for in future work.

At the request of the reviewer we have presented the results by ice type (bottom columnar, columnar, frazil and snow ice) to determine the potential for the misdiagnosis of drained brine inclusions as air inclusions. Brine loss as a result of the core sampling method should be most prevalent in the bottom permeable columnar layer. So, in the revised work, we provide individual statistics for each of the delineated layers distinguished between the three ice regimes throughout. In submitted response-to-reviewer document attached with the revised work, we extensively support the methodology used for the classification of air inclusions in our study.

The storing temperature finally potentially influences our computation. Storing sea ice at -20°C alters the sea ice microstructure and its inclusions (e.g. Cox and Weeks, 1986). Light et al., (2003) proceeded to a cooling sequence (-2°C to -25°C) and a warming sequence (-25°C to -2°C) on ice thin sections. According to their results cooling sea ice caused inclusions to shrink in size including the disappearance of the smallest air inclusions, while warming increased the size of existing air inclusions without forming new bubbles. Considering that the smallest bubbles could have disappeared and some have shrunk in size, our computed air volume fraction should be considered as a minimum estimate of the true air volume fraction. In the absence of a method that preserves the natural temperature gradient within sea ice immediately and without change upon extraction, ex situ analysis of sea ice samples after storage at low temperatures is an established protocol.

How accurately artificial ice experiment represents in situ processes is a long-lived question. Ice tank experiments are currently used to proceed to sea ice research. Tank experiments have several advantages over field investigations. The ice tank offers the possibility of refining field measurements by carrying out experiments under fully controlled environmental conditions. Work on physical, biogeochemical, and sedimentological aspects of growth processes of artificial sea ice using the large outdoor tank complement observations from both the Arctic and the Antarctic. In the manuscript we are referring often to small experiment with artificial ice (Nomura et al. 2006) or mesoscale experiment (Killawee et al. 1998; Tison et al. 2002; Kotovitch et al. 2015). Changing the scale of the experiment to larger experiment, allows coming closer to the reality. In this respect, we would like to point out that that the SERF experiment goes a step further. The pool is 600 times larger than mesocosms used in the INTERICE suite of artificial ice experiments for instance. However, it's difficult to define what could be "typical" Arctic Ocean conditions, as ice and snow thickness present strong contrasts across the whole Arctic Ocean. We feel that assessing how "typical" can be the SERF conditions compared to Arctic ocean is beyond the scope of the paper, as we are not extrapolate our results to the overall Arctic Ocean. However, we are confident that this experiment is close enough to in situ conditions to discuss specific process like bubble formation and describe a new method to estimate air volume fraction.

Reference

Cox, G.F.N., Weeks, W.F., 1986. Changes in the salinity and porosity of sea-ice samples during shipping and storage. J. Glaciol. 32 (112), 371–375.

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INTERACTIVE COMMENT ON “IMAGING AIR VOLUME FRACTION IN SEA ICE USING NON-DESTRUCTIVE X-RAY-TOMOGRAPHY” BY O. CRABECK ET AL.

Author response to anonymous Referee #1

General Comments from the referee:

This is an interesting paper and the authors should be commended for applying an existing tool (medical CT) to a new problem. Other strong points of this paper include: good discussions of the processes affecting air inclusions in sea ice; a solid comparison of alternative methods of measuring sea ice air inclusions; thorough analysis; and thought provoking presentations of the data (figures). I have two primary concerns, assumptions underlying the study that are hinted at, perhaps, but should be addressed explicitly.

First, the authors need to discuss the potential ramifications on their work of temperature changes during the coring/storage/analysis process. These cores were -4 ◦C to -8◦C at the ice atmosphere interface, and -1.6◦Cto -2◦C at the ice-water interface. They were stored at -20◦C, and gas was extracted in a cold room at -25◦C. These cold temperatures would have produced changes in the size of brine inclusions and air pockets and the influence of this on the results andinterpretation should be discussed fully (perhaps in Section 4.3).

We fully agreed that storing temperature affects sea ice microstructure as well as the size and the amount of inclusions.

In the revised manuscript, we discussed the potential effect of storing temperature in section 4.1. :

> *The storing temperature finally potentially influences our computation. Storing sea ice at -20°C alters the sea ice microstructure and its inclusions (e.g. Cox and Weeks, 1986). Light et al., (2003) proceeded to a cooling sequence (-2°C to -25°C) and a warming sequence (-25°C to -2°C) on ice thin sections. According to their results cooling sea ice caused inclusions to shrink in size including the disappearance of the smallest air inclusions, while warming increased the size of existing air inclusions without forming new bubbles. Considering that the smallest bubbles could have disappeared and some have shrunk in size, our computed air volume fraction should be considered as a minimum estimate of the true air volume fraction. In the absence of a method that preserves the natural temperature gradient within sea ice immediately and without change upon extraction, ex situ analysis of sea ice samples after storage at low temperatures is an established protocol.*

We are currently designing an experiment, which combine Medical CT-scan and micro CT-scan (Bruker SkyScan 1174) to evaluate the effect of storing temperature on air inclusions within columnar and granular sea ice.

Second, I believe the conclusion is a bit far-reaching. Although I tend to agree that, "air volume fraction should be an important inclusion in parameterizations of sea icepermeability," I'm not sure that the authors "introduce new perspectives on processes regulating gas exchange at the ice-atmosphere interface". They show large bubbles and high air volume fraction in the upper (granular) portion of the sea ice. For these bubbles to play a role in gas exchange, they must be connected. The authors did not actually test permeability, or demonstrate it by analyzing pore connectivity in the vertical dimension. Nor did they monitor changes in a single sample over time and changes in temperature. Thus the assertion that these large air pockets are important in gas exchange at the ice-atmosphere is based on the rules governing permeability, rules which are based on columnar sea ice. And yet, the large bubble-high air volume fraction layers are in the granular ice. I would like to see these two points addressed explicitly in the final paper.

We agreed. We are not able to discuss the effect of air inclusions in term of permeability. However we introduce new perspectives for gas exchange and transport. In the revised manuscript, we discussed this new point (section 4.4):

> *The presence of large bubbles and air volume fraction > 5% in the top of the ice cover should potentially mediate gas fluxes over sea ice. Partitioning between gaseous phase and dissolved phase is of paramount importance for gas transport in sea ice, as it control the direction of transport – upward versus downward as well as the magnitude. If the gases are in the dissolved phase, they will be mainly transported downwards with the brines, like the other solutes. Few exceptions are the gas diffusion within the brines network that transport gases both ways in function of the concentration gradient (Crabeck et al., 2014a), and upward brine expulsion at the ice-air atmosphere. If the gases are in the gas phase (i.e. bubbles), they are only transported upward due to bubbles buoyancy. Kotovich et al., (2015) observed that air-ice gas transfer coefficients for CO_2 in young permeable artificial sea ice is 5 times larger during ice growth compared to ice decay. They suggest that this difference is due to the formation and transport of bubbles during ice growth. This process appears to provide a very efficient pathway to transport gases within and out of the ice. Indeed, 1D modelling suggests that bubbles migrating upward out of the ice contribute to 80% of the CO_2 fluxes from sea ice to the atmosphere during ice growth (Kotovich et al., (2015).*

In conclusion and perspectives, we formulated the sentence in term of perspectives and future work rather than conclusion of our own work.

As a result of the presence of large bubbles and higher air volume fraction measurements in sea ice we introduce new perspectives on processes regulating gas exchange at the ice-atmosphere interface, and note that further work should investigate, the effect of air volume fraction on sea ice permeability parameterizations.

Specific comment from the referee:

Page 5208, line 26 – I'm not sure what is meant by "the evolution of gas concentrations". This should be reworded for clarity.

Agreed, we clarified the sentence in the revised manuscript:

Section 2.3. We therefore compared (i) the gas concentrations profile measured in bulk ice to (ii) the theoretical inventory predicted by the solubility in brine at atmospheric saturation; the maximum concentration of O_2, N_2 and Ar in the dissolved phase when the brine is not supersaturated, Carte, 1961; Lubetkin, 2003; Zhou and et al., 2013).

Page 5217, lines 17-18 – Here the authors should cite Golden et al. again. But, they should also note here and in their later analysis that the Rule of Fives they allude to is specific to columnar ice.

Agreed, we added the reference and we highlighted that this threshold is specific to columnar sea ice.

Page 5219, lines 17-19 – Here the authors state that, "traditional methods can hardly be used to validate the imaged data at the same resolution." This is true, BUT scanning electron microscopy does provide such an opportunity, and has been used to validate thresholding in microCT analysis (see Lomonoco et al., 2009, citation at end of this review).

Thanks for this suggestion. We are working on the design of experiment which combines computed porosity from the Siemens CT- imager employed in our manuscript with some number of subsamples using a micro-CT (Bruker SkyScan 1174) imager for higher resolution morphometric analysis of the smallest bubbles. We consider highly using SEM technology as a complementary tool in our future work.

Page 5221, line 12 – Bubbles as small as 0.019 mm are reported. Somewhere, here or on page 5211, the spatial resolution of the instrument should be explicitly stated.

The spatial resolution in the first version was 0.195 x 0.195 x0.6 mm, then the smallest diameter (in the x-y plan) recorded was equal to the pixel size. In the revised manuscript, we improved the resolution in the x-y plan (0.097x0.097x0.6 mm) and because it is ambiguous to report exact diameter from mixed pixel, we classified bubble diameters into three categories at a millimeter scale: micro bubbles ($\emptyset$ < 1 mm), large bubbles (1mm < $\emptyset$ < 5mm), and macro bubbles ($\emptyset$ > 5mm). In the revised manuscript, the resolution of scan is clearly mentioned and discussed.

Page 5223, line 24 – If the authors are going to cite their Rayleigh number results, they need to show the data.

We decided to delete this sentence, because discussing and illustrating the Rayleigh number; both in the method and results section does not provide sufficiently new and relevant information to support the discussion. Citing the permeability threshold of 5% in columnar sea ice is sufficient to assume that there is convection in the bottom part of the growing ice sheet.

Page 5226, lines 14-19 – "We systematically observed an increase of the bubble size and a decrease of the bubble density in the granular ice (Fig 11a), suggesting the presence of coalescence processes." The word "systematically" here is confusing, and the observation of larger but fewer bubbles doesn't necessarily mean coalescence, does it? Likewise, it seems a bit of a leap to say that the bubble geometry shown in four lateral slices in Figure 13 derives from coalescence unless the authors have observed their development over time. Larger bubbles could be related to post depositional processes, but could they not also be related to snow density and microstructure and fluid flow during flooding?

We agreed, as we don't have any time-lapse picture, we couldn't state that there are coalescence processes. We reviewed our interpretation in the revised manuscript. However according to our image, we believed that is reasonable to speculate that bubble can merge.

> *Macro bubbles are exclusively found in granular layer. They seems resulting of aggregation of discrete bubble like an aggregation of soap bubbles A succession of 0.6 mm thick transversal slices at 2.46 cm depth from January 25 is shown in Fig.14. In the first slice at +2.28 cm depth (Fig. 14, far left panel) four individual bubble bases are identifiable from which a single top bubble is formed*

at +2.46 cm depth (Fig.14 far right panel). The rapid freezing of slush in porous snow could potentially produce bubble aggregation.

Page 5228, lines 15-18 – Note my second concern in the introductory paragraph of this review. It may be reaching to say that the authors "introduce new perspectives on processes regulating gas exchange at the ice-atmosphere interface". They show large bubbles and high air volume fraction in the upper (granular) portion of the sea ice. For these bubbles to play a role in gas exchange, they must be connected. They did not actually test permeability, or demonstrate it by analyzing pore connectivity. Nor did they monitor changes in a single sample over time and changes in temperature. Thu the assertion that these bubbles are important in gas exchange at the ice-atmosphere is based on the rules governing permeability of columnar sea ice. And yet, the large bubble-high air volume fraction layers are in the granular ice. I think the authors should couch the conclusions in those terms.

We fully agreed, and revised the introductory paragraph and the conclusions according to your suggestions. See previous comments.

Technical Corrections

All of the figure captions need editing for typographical errors and clarity. For example: Figure 1: "every black dots represent", should read "every black dot represents".

Figure 2, there is a typo, "withe" for "white". Captions for figures 3-5 and 11-13 should not begin with "shows". Figure 6: The caption should explain the discontinuities in the second and third thin sections.

Figure 7: "threshold" should be "thresholds" and "extend" should be "extent". And so on. I found the captions to Figure 10-13 particularly awkward.

We revised the fig caption.

In fig.6. (Fig.4 revised manuscript). For January 25, the ice core has been cut in 3 sections to suit the glass plate, resulting in three separate pictures. For January 16, the whole ice core was put on a single plate, at the end of the shaving process a crack damaged the top part, we cropped the crack out of the picture.

Page 5207, line 18 – Add New Hampshire (or NH) after Lebanon. Done

Page 5207, line 25 – Should "porTable" have an upper case T in it? Done

Page 5211, line 1 – What is a U-channel? (explain). It was the energy channel we used, we revised the sentence.

Page 5214, line 22 – I think this should read, "In the following section
...
" rather than
"paragraph." Done

Page 5219, line 22 – I believe M-V should be M/V, Yes , done

Page 5222, lines 16 and 17 – Something is missing in this sentence(s) We revised the sentence.

Page 5223, lines 5 and 7 – I believe the authors mean "increase" instead of "accumu-lation" Yes, we revised the sentence

Page 5225, line 6 – "create" should be "creates" Done

Page 5225, line 8 – Add a space between "stable" and "bubble" Done

Page 5225, lines 11-12 – This is not a complete sentence. Yes, we revised the sentence

Page 5225, lines 15-16 – This is a complete sentence and I think I understand it, but itis awkward as written. We agreed and changed the sentence.

Page 5225, lines 20-22 – This short paragraph seems to float here by itself. It needs to be anchored with references to your figures, at least. Agreed, it is now attached to a paragraph and linked to the table 6

Page 5225, line 28 – "by surface processes due to snow falls" would be more succinctly worded, "as a result of snow fall." The all paragraph has been revised.

Page 5226, lines 1-4 – "on" should probably be "by". Here the authors say the snow layer "was able" to flood the ice by producing negative freeboard. Did this in fact hap-pen? Through cracks? Explain.
No, It did not happened through cracks. The specific conditions of the seawater incursion are detailed in the revised manuscript (section 3.1)

Page 5228, lines 2-3 – This sentence is unclear. What is meant by "At any growth step"?

We revised the sentence: *In growing sea ice, a local maximum exists in the vertical just above the permeability transition, confirming the important role of this transition zone in shaping the vertical air volume fraction distribution.*

Reference

Cox, G.F.N., Weeks, W.F., 1986. Changes in the salinity and porosity of sea-ice samples during shipping and storage. J. Glaciol. 32 (112), 371–375.

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Light, B., Maykut, G.A., Grenfell, T.C., 2003. Effects of temperature on themicrostructure of first-year Arctic sea ice. J. Geophys. Res. 108 (C2), 3051. http://dx.doi.org/10.1029/2001JC000887.

INTERACTIVE COMMENT ON "IMAGING AIR VOLUME FRACTION IN SEA ICE USING NON-DESTRUCTIVE X-RAY-TOMOGRAPHY" BY O. CRABECK ET AL.

Author response to Sonke Maus (Referee #2)

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?**

Core has been scanned at room temperature. The worst-case scenario is 12s scan time and maximum of one minute in total (including sample manipulation). The analysis was performed on a subsampled dataset excluding sample surface. We don't expect any melting inside this subsample. Maximum 12s scan time. The CTDI was 28.7406 mGy (dose for one slice). 1Gy= 1J/kg. (Ref. Siemens). Applying 28 mGy to 3.064 g of ice will increase the temperature by $14.8*10^{-6}$ °K . Yes there will be heating but not enough to create any melting. Then, no internal melting occurred during this short time (nor void formation due to melting of brine). We added some details about the scanning time and temperature in the revised manuscript.

> 1 axial slice of ice = volume approx of 3342.6mm3
> 0.9167g/cm3*3342.6mm3= 0,0009167g/mm3*3342.6mm3 = 3.0641g
>
> 1Gy=1J/kg
> 0.0287406Gy=0.0287406J/kg
> 0.0287406J/kg*0.0030641kg=0.000088064J=88.064x10-6J
>
> Q=M*Cp*dT
> Cp for ice at -20C = 1943J/kgK
>
> 88.064*10-6J=0.0030641kg*1943J/kgK*dT
>
> dT=88.064*10-6/5.9535463
>
> dT=14.8*10-6K

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 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 is4 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).

Thank you for this important comment on the resolution; we did indeed associate the spatial resolution to the pixel size, which is a misunderstanding from our part. We agree that spatial resolution, defined as the smallest measurable element, is not equal to voxel resolution.

The data acquisition was done in spiral mode with a pitch factor of 0.6, rotation speed was 1s/rotation and the collimation was 6mm. The X-ray source was set at 120 kV and 150 mAs. These configurations produced 1152 projections for each reconstructed axial slices. As the image size is limited by the manufacturer to 512 x 512 pixels, the pixel resolution is defined by the chosen field of view (FOV).

In the first manuscript, we used of FOV of 100 x 100 mm, leading to a pixel size of 0.2 mm (exactly 0.195 mm)[1]. We agree that this resolution might not be sufficient to accurately resolve small air inclusions. In the revised manuscript, we applied the smallest FOV available. The smallest selectable FOV is 50 x 50 mm providing a pixel resolution of 0.0977 mm. As this FOV is too small to contain the whole sample, four reconstructions have been produced and concatenated together using Matlab. A final image size of 1024 x 1024 pixels with a pixel resolution of 0.0977 mm with a slice thickness is 0.6 mm is thus produced. The reconstruction algorithm used is Siemens

[1] In the first version, I wrote “ the voxel size as 0.25 X 0.25 X 0.6 mm”, where I should have wrote “0.2 X 0.2 X 0.6 mm” or “ 0.195 X 0.195 X 0.597mm”, then the smallest bubble diameters detected in the X-Y plan were 0.20,0.21 and 0.19 mm for Jan 14,16 and 25, respectively (Table 2, first version).

SAFIRE (Sinogram Affirmed Iterative Reconstruction) with three iterations. The convolution kernel is J70h, a medium-sharp filter.

In the revised, we improved the spatial resolution by 200% leading to new voxel size 0.0977 x 0.0977 x 0.06 mm. There is a possibility to obtain a smaller slice thickness by using the UHR (Ultra High Resolution) mode. Using this mode increase the scan time by a factor of 5 and increase sample warming. To minimize melting, we discarded this option.

The air volume fraction was computed in the x-y plan every 0.6 mm as the area occupied by air pixel on the area of the transversal slice. Regarding the new resolution and the fact that most parameter in sea ice are computed from coarse resolution (5 cm section), I believe it is reasonable to state that the air volume fraction is computed with high resolution or submilliter scale (every 0.6 mm).

Regarding, the morphometric analysis (bubble diameter), we agree that spatial resolution, defined as the smallest measurable element, is not equal to voxel resolution. Most of the voxel in columnar sea ice are mixed pixel (Hu-value between the Hu value of air and Hu-value of ice), then bubble diameter cannot accurately be measured from a single mixed voxel element. In this context, instead of reporting true diameter, we reported bubble class at millimeter scale. In the revised manuscript, we highlighted the problems linked to the mixed voxels.

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 i0nterpretation 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.**

   The image thresholding methods for selecting air pixels were also re-evaluated on the new data set.

   We ran several algorithms on (i) the entire three data-sets and (ii) on separate part of each dataset. Based on (i) a visual interpretation and (ii) the stability of the algorithms, we selected a threshold. We limited the number of algorithm used to 3 instead of 6. Secondly, we defined a manual threshold according to the new data set and we defined a last threshold for mixed pixel in which there is at least 50 % of air in the mixed pixel.

Line 308: Finally, the tomographic intensity of "mixed pixels" which appear as varying shades of grey is dependent of the proportion of air (V_{air}), ice (V_{ice}) an brine (V_{brine}) in the pixel and the proportions of the tomographic intensities of those constituents (air (Hu_{air}), ice (Hu_{ice}) and brine (Hu_{brine})) in the following mixture model:

$$Hu = (V_{air} \times Hu_{air}) + (V_{ice} \times Hu_{ice}) + (V_{brine} \times Hu_{brine}) \quad (3)$$
$$1 = V_{air} + V_{ice} + V_{brine} \quad (4)$$

The TI of pure ice crystals is determined using the mode of the histogram containing all the data from each core ($TI_{pure\ ice}$ = -74HU). Brine TI values ranged from 60 to 500 depending on brine salinity; we selected the middle point of that range (TI_{brine} = 200HU). Finally TI_{air} = -1000HU. According to the mixture model (equation (3)), any pixel TI ≥ -400 HU contains at least 50% air and is therefore selected as part of an air inclusion (Table 1).

Table 1. Mixed pixel model

V_{air} =50%	V_{air} x Hu_{air}	0%<V_{ice}<50 %	V_{ice} x Hu_{ice}	0%<V_{brine}<50%	V_{brine} x Hu_{brine}	Hu value*
0.5	-500	0	0	0.5	100	-400
0.5	-500	0.1	-7.4	0.4	80	-427.4
0.5	-500	0.2	-14.8	0.3	60	-454.8
0.5	-500	0.3	-22.2	0.2	40	-482.2
0.5	-500	0.4	-29.6	0.1	20	-509.6
0.5	-500	0.5	-37	0	0	-577

*$Hu = (V_{air} \times Hu_{air}) + (V_{ice} \times Hu_{ice}) + (V_{brine} \times Hu_{brine})$

The manual threshold was close to what algorithmic threshold suggest. While the thresholds range over 83 Hu unit, the ANOVA test (P<0.005) showed that the three thresholds give similar results.

The air volume fraction is presented as the mean of the air volume fraction results computed using the three selected thresholds. The potential range of the V_a from each of the three methods is represented by the standard deviation of this mean (table 2). On average, we reported V_a ± 43% in columnar sea ice and V_a ± 16% in granular ice.

Table 2. V_a as the mean of the air volume fraction computed using the three selected thresholds

Slice Depth (cm)	Ice type	Cl-Ridler TI =-370 HU	TI =-400 HU (for Va >50%)	Manual threshold TI = -453HU	Mean	Stdev	Rstdev (%)
0.5 January 16	Granular	5.62	5.12	4.73	5.15	0.34	8.6%
1.4 January 16	Columnar	0.86	0.66	0.49	0.67	0.18	26.8%

We selected three thresholds using three different approaches, we are not totally sure about what you expect regarding your questions of manual threshold and uncertainties? Could you give us some additional clarifying guidance?

Like air inclusions are substantially larger in granular ice, we could resolve most of the air porosity by using a lower threshold, and ignored the mixed pixel. Then, by ignoring the mixed pixel, we would conclude that the granular ice contain exclusively large bubbles (diameter >1 mm), while the presence of mixed pixels witness that bubble smaller than 1 mm are also a characteristic of granular ice. As a first study on air inclusions by x-ray tomography, we prefer using the same threshold and consider for further work the potential of using different thresholds in function of the ice type.

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.**

Thanks, for this comment; we agreed that presenting the results by ice type is more useful and pertinent than the results of the total ice.

In the revised manuscript, we differentiated granular from columnar ice. We reported individually the results from the bottom columnar permeable ice layers and distinguished between initial frazil layer and snow ice layers.

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.

It is true that the density derived air volume fraction is larger than the CT-scan computed air volume fraction, but they generally agree, as demonstrated by correlation coefficient of 0.98 (in the revised manuscript). The problems linked with density derivate measurement is the error on the dimension. Dimensional errors in sample preparation can be large enough to occasionally results in gas volume estimates that are negative, a physical impossibility. Considering an ice cube (t =-5 C , Si= 5) with a target ideal side length of 5 cm, a deviation during the measurement of 0.7 mm would drastically affect the air volume fraction (table 3) .

Table 3. Effect of dimensional error on brine volume and air volume fraction computed by mass-volume density measurement using state equation from Cox and Weeks (1983).

Temperature (°C)	Salinity	Length (cm)	Ice cube volume (cm3)	Masse (g)	Density (g cm3)	V_b (%)	V_a (%)
-5.00	5.00	4.93	119.82	113.75	0.95	5.1	-1.3
-5.00	5.00	5.00	125	113.75	0.91	4.9	2
-5.00	5.00	5.07	130.32	113.75	0.87	4.7	6.3
Relative standard error		**±1.4%**	**±4.2%**		**±4.4%**	**±4.1%**	**±163%**

Knowing that, there is large agreement in the sea ice community than density measurement is not reliable and accurate enough to resolve the air volume fraction. We believed that the two profile fitting on January 25, may just results of some luck. We believed that the correlation coefficient of 0.97 shows that the technic have the same trend overall.

The point is not to compare the two techniques to show that they yield similar, they don't. The point was to show that the profile from the two techniques are similar in shape, but that our argument is that the CT data is more accurately resolving sea ice air volume fraction than the mass-volume technique. On average, we measured V_a ± 16% and V_a ± 43% in granular and columnar layer, respectively, while on average we measured V_a ± 163% using density measurement.

6. **Image interpretation. The authors claim to show in Fig. 13 the formation of a macrobubble 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.

We agreed, as we don't have any time-lapse picture, we couldn't state that there are coalescence processes. We reviewed our interpretation in the revised manuscript. However according to our image, we believed that is reasonable to speculate that bubble can merge.

> *Macro bubbles are exclusively found in granular layer. They seems resulting of aggregation of discrete bubble like an aggregation of soap bubbles A succession of 0.6 mm thick transversal slices at 2.46 cm depth from January 25 is shown in Fig.14. In the first slice at 2.46 cm depth (Fig. 14, far left panel) four individual bubble bases are identifiable from which a single top bubble is formed at 2.28 cm depth (Fig.14 far right panel). The rapid freezing of slush in porous snow could potentially produce bubble aggregation.*

7. **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 modified to replace the linear attenuation coefficient as the volume averaged sum of brine $_b v_b$, air $_a v_a$ and ice $_s v_s$, where v_x denotes volume fractions. This equation may then, using the derived v_b, and assuming suitable values for the attenuation coeffcients of air $_a$, ice $_s$ and brine $_b$ (at the imaging temperature) be solved for v_a (note that $v_b + v_a + v_s = 1$).**

Thank you for the suggestion. The resolutions limit the bubble diameter that can be measured. At the actual state, we don't have any accurate calibration for the linear attenuation coefficient for brine or pure ice We are considering further analysis using multi energy scan and establish some calibration for brine and ice. Also multi energy scan requires more time and may be not feasible for ice due to the potential warming.

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**

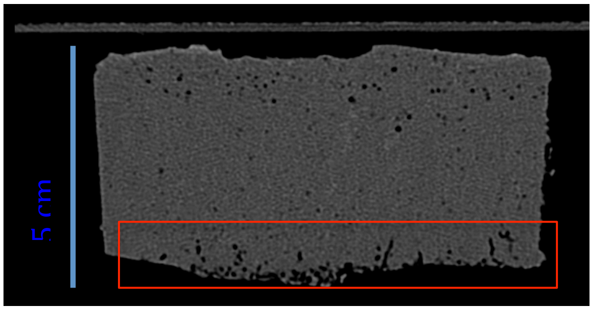
therefore highly recommended to report the statistics of the bottom layer separately.
2. As an example, I wonder if air bubbles shown in Fig. 9b, left slice for Jan 14, 3.6 cm from the surface (and thus 0.4 cm from the ice-water interface) are really enclosed bubbles. I would rather expect them to be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

We agreed that brine loss might happen during ice core extraction. In the first version of the manuscript, we did eliminate the analysis of the bottom-drained slice. In the revised manuscript we reevaluate the region of interest (we deleted couple of more slices while the distinction between drained brines and air inclusions were ambiguous) and we reported individual statistic for the bottom permeable columnar layer. However, we strongly believe that the reported inclusions both in columnar sea ice and granular layer are actual air inclusions and not drained brines.

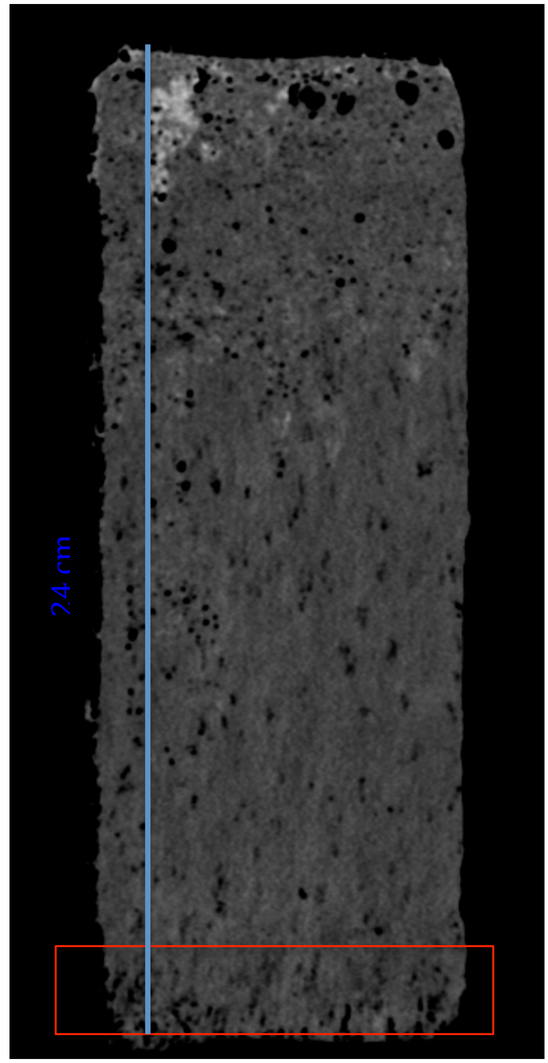
In columnar layer, brine features appear as (1) large channel (mm diameter) and (2) liquid brine film at the ice crystal boundaries (diameter < mm). In the early stages of growth, a region of high permeability, often called the "skeletal layer", is observed in the lowermost few centimeters of the ice from which convective gravity drainage removes dense brine [e.g.,*Eide and Martin , 1975*]. However, once the sea ice thickness exceeds 5-10 cm the convective downflow is dominated by brine channels. Brine channels are the preferred pathways of downward-moving brine during desalination. They are usually vertical. They seem to have a larger diameter close to the mush–liquid interface [*Aussilossous et al, 2006*]. **The channels do not span the full depth of the ice layer**. **As the sea ice cools, the solid fraction increases at the expense of the brine channels. The channel are seen to be active for brine drainage only close to the growing interface [*Aussilossous et al, 2006*].** According to the reference therein, brine drainage can only affect the bottom permeable layer. According to the percolation theory, brines are susceptible to flow out of the columnar layer while the brine volume fraction reaches 5% following *Golden et al.,* [1998, 2007] and from 3.9 to 6.9 % according to *Pringle et al.,* [2009].

In our samples, large drained brine channels are not observed (Fig. 1 a,b). We did analyze ice block where these channels have been drained during the sampling but we did not use them (fig 2c).

a.



b.



c.

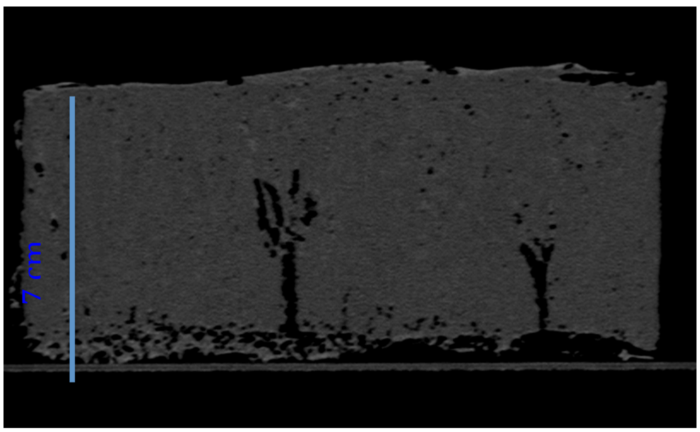


Fig 1. (a) and (b) Image from January 14 and 25 respectively, no drained brine channel appeared and all the inclusions are well disconnected , then they are air inclusions and not drained brine. The red rectangle highlighted the bottom layers excluded for the study. (c) Image from January 15 where a channel has been drained during the sampling, while that kind of features appeared in our image, the full ice block was rejected of the study. The scale is represented by the blue line

Every ice layers for January 14 were above the brine volume fraction thresholds of 5 %. Then according to the reference therein, January 14 is mostly susceptible to have been affected by brine lost during ice core extraction. While the brine volume ranged from 11% to 58 % (largest V_b observed in this study) and the air volume fraction ranged from 0.18% to 5.09% (smallest V_a observed in this study) .

For January 16 and 25, only the sea ice bottom could have been affected by brine lost, because the brine volume fraction fell below these thresholds in the internal layers. As previously mentioned “The images were individually examined and ambiguous pixels around the sea ice core sample were also removed. We have eliminated the bottom slices of each imaged ice core due to observed core brine drainage resulting from the coring method". We agreed that the increasing air volume in bottom 0.5 cm for January 16 and in the bottom 1 cm for January 25 are probably due to brine drainage, we removed this layer from the analysis. However, this is not affecting in any point the analysis and conclusion. However, it reinforces the fact that in bottom ice layers, the air volume fraction is at its lowest and made of micro bubbles.

Considering the others bottom layer, it is unlikely that the air volume fraction are drained brines, because it is actually in the bottom that the brine volume is the highest. Then, if bubbles were drained brined we would expect see the air volume fraction increased where the brine volume increased. It is actually in the bottom layers that the lowest air volume fraction is recorded (Table 4) .

Table 4. Air volume fraction versus brine volume in permeable bottom sea ice:

16 January			25 January		
Bottom permeable layer depth (cm)	Vb (%)	Va (%)	Bottom permeable layer depth (cm)	Vb(%)	Va(%)
7.7 (interface ice-sw)	20	0.42	20.9 (interface ice-sw)	20.38	0.65
6.7 (1cm away for the interface)	11.9	0.013	19 (2cm away for the interface)	16.73	0.61
5.7 (2cm away from the interface)	6.47	0.24	17 (4cm away from the interface)	8.36	0.72
			15 (6cm away from the interface)	6.26	1.4

In ice top (surface) granular layers, brine inclusions appear to be more isolated and disconnected pockets than connected channel. The cited permeability thresholds established for columnar sea ice is not relevant for granular layer. *Golden et al.,* [1998] suggested that the percolation threshold established for columnar ice could be higher for granular ice, which is more randomly distributed. Then, brine is less susceptible to leave the ice in the top layer than the bottom layer. As the ice has been moved vertically from the pool and carefully transported to a freezer at -25 C situated at less than 100 feet from the sampling site, the brines were unable to leave the ice by the top, and according the percolation threshold for sea ice, the brines was not able to be drained downward due to the rapid brine volume reduction.

For all the reason cited, we believed that the inclusions observed are air bubbles and not drained brine.

In the revised manuscript, we differentiated the bottom layers from the rest of the ice core as well as the granular layer. We limit the spatial extension of the bottom layers by the permeability threshold of 5%. We choose to work with the threshold of 5 % because (1) it is most widely used and recognized in the literature and (2) study on sea ice permeability showed that effective porosity which is the volume fraction of brine that is effectively connected for fluid transport is close to zero for brine volume fraction below 5% (*Freitag*, [1999] and *Petrich et al*, [2006]).

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

First the air saturation in bulk sea ice is calculated by computing the air solubility in the brines ($C_{saturation\ brine}$) in function of the bulk ice temperature and the brine salinity. The brine salinity is derivate from the bulk ice temperature using the temperature dependent freezing point from *Unesco,* [1978]. Once, we obtained the air solubility in the brine ($C_{saturation\ brine}$,), we multiply the computed solubility by the brine volume fraction to get the air solubility in bulk ice ($C_{saturation\ bulk\ ice}$).

$$C_{satu□ation\ bulk\ ice} = V_b \times C_{satuartion\ brine}$$

and the ratio between the gas concentration measured ($C_{bulk\ ice}$) and the air concentration at equilibrium ($C_{saturation\ bulk\ ice}$) gives the *SAT$_f$*:

$$SAT_f = {C_{bulk\ ice}}/{C_{saturationbulk\ ice}}$$

Potential salt lost lead to an underestimation of the brine volume fraction trough the computation of *Cox and weeks*, [1983], we might underestimate $C_{saturation\ bu□k\ ice}$ which lead to an overestimation the saturation factor. While it is in the bottom of the ice that we potentially have lost brine, it is there, we might have larger overestimation on the SAT$_f$. So far, the SAT$_f$ for the bottom permeable sea ice and the entire ice layer on January 14 are the smallest, and the ice layers are considered subsaturated. Then in case, our SAT$_f$ is overestimated, it will reinforce our conclusion about subsaturated bottom ice. Concerning, the SAT$_f$ at the top of sea ice, as mentioned before, salt are less susceptible to leave the top layer than the bottom layer. The increasing salinity observed in top layers (C-shaped profile) is typical of young first year sea ice and does not indicate brine lost.

The total gas content of the permeable columnar bottom of each of the ice cores (and the entire core on 14 January) were close to the concentration at saturation with respect to calculated theoretical atmospheric gas concentrations, leading to saturation factor ranging from 0.8 to 1.2. This will be referred to as "subsaturated" (SATf≤1.2). On the contrary, the total gas content of the impermeable columnar layers and the granular *surface layers of the sea ice were largely greater than the concentration at saturation leading to saturation factor ranging form 9.5 to 16. These will be referred to as supersaturated (SAT_f > 1.2).*

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.

Most past studies on thin section have reported that bubbles are larger than brines in granular sea ice:

-*Grenfell,* [1983], *Perovich and Gow,* [1996] and *Cole et al.*, [2004] highlighted that the congelation ice is usually depleted in air inclusions while top granular ice is described as bubbly with larger air inclusions.

-*Gow and Weeks*, [1977] wrote about an ice core of 1.5 m thick from fast ice in Beaufort sea: " *The top most layer, extending from the surface to 3.5-cm depth, is very bubbly and fine-grained*"

-*Cole et al.,* [2004] wrote about ice core (1.6 m thick extracted from the Chukchi sea " *The Figure 2 shows a composite vertical micrograph taken with unpolarized light to illustrate the nature of the inclusions in the frazil band. Such material typically contains a significant population of gas-filled inclusions (bubbles) and does not necessarily have the cellular substructure of congelation ice; the inclusions are generally arranged more randomly than farther down in the sheet. The inclusions that formed as gas-filled bubbles are generally distinguishable from drained brine inclusions because they are larger and have the shape of an oblate spheroid*."

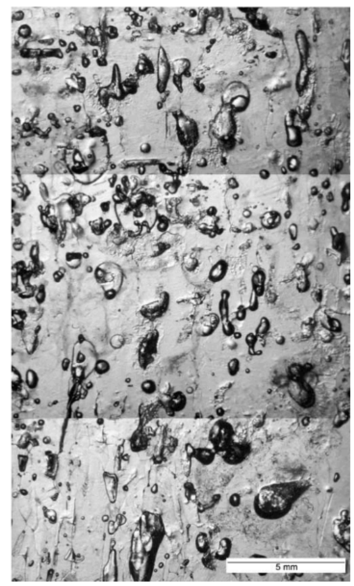


Figure 2

-*Perovich and Gow,* [1996] wrote in their abstract "*Air bubbles are much larger than brine pockets, with mean major axis lengths of the order of millimeters for air bubbles and tenths of a millimeter for brine pockets. Observations of inclusion shape factors indicate that, in general, brine pockets are more elongated than air bubbles.*"

-*Perovich and Gow* [1996] wrote about pancake ice (max 30cm thick) from an ice tank experiment. "*A notable feature is the presence of both air bubbles and brine pockets in the top section(depth of 1 cm). The air bubbles resulted from the agitated nature of the initial ice growth. Comparing the air bubbles and brine pockets in the top section, we see that there were roughly 3 times as many brine pockets as air bubbles but the air bubbles were larger, with a mean area about 5 times greater (0.28 mm 2 versus0 .046m m2)"*

In transversal plan (x-y), all the air inclusions are closed pore, while taking in account the z plan, some bubbles in granular layer may extend to the next slice as shown in figure 14 (revised manuscript). Because granular layer content more bubbles and as reported in the literature bubbles are larger than brine inclusion in granular ice, We don't think the open and close porosity could help to discriminate between drained brine and bubbles. Moreover as explained earlier, neither salinity profile (C-shaped) nor the permeability of the sea ice showed potential brine lost in top layer.

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.

First the removal was not performed on the whole pool but on the half of it, and we did not mention the details of it, because the removal of the snow did not produce any relevant isostactic rebound. Thanks to surface elevation from Lidar data, we able to retrieve the change in sea ice elevation (rebound and depression of sea ice surface).

Table 5. Change in sea ice elevation for January 2013.

Date	Time	Surface Elevation from Lidar			Change in Elevation [	Ice Thickness	Snow Thickness	Ch Elev not from Snow
		Mean [m]	Std [m]	Median [m]	cm	Cm	cm	cm
January 21,	10:00	-2.732	0.009	-2.732	5.4	15.0	6.1	-0.7
January 22,							snow cleared on 23rd	
January 23,	13:50	-2.788	0.006	-2.788	-0.2	17.5	0.0	-0.2
January 24,	09:20	-2.786	0.005	-2.786	0.0	20.0	0.5	-0.5
January 25,	10:00	-2.711	0.005	-2.711	7.5	23.5	8.0	-0.5

Lidar Data shows that ice surface (rather than snow surface) was fixed in place throughout the experiment, likely due to ice sheet freezing onto pool walls. Therefore, there was almost certainly no isostatic rebound (i.e we observed a change of 0.5 cm). After the snow clearing, a a brine skim was observed at the ice surface and later on frost

flower appeared. The presence of brine skim at the ice surface show that pores at the ice surface were full of brines [*Rysgaard et al., 2014; Galley et al.,2015*]..
Well thin sea ice clearly shows a fair amount of elasticity… we think the fact that the surface elevation did not change in the course of time doesn't prove that the ice has not been "pushed downwards" during the operations, therefore potentially allowing up and down movements of the material within the porous ice cover towards the snow cover.

Periodically the sea ice froze to the side of the pool resulting a hydrostatic pressure head in the seawater below, causing episodic percolation of seawater at the freezing point upwards through the sea ice volume resulting in wet snow ice and slush at the sea ice surface on 16 Jan. A slush layer (up to 3 cm thick) was also observed at the snow base on 20 Jan. This episodic hydrostatic pressure head and resultant upward percolation of seawater through the sea ice caused the granular layer of the sea ice volume to thicken over time, likely by the formation of snow ice layer as the slush layer froze. On January 14, the granular layer was 0.7 cm frazil ice, on January 16, the granular layer thickened to 1.7 cm (consisting of the initial 0.7 cm of frazil and 1 cm of snow ice). On January 25, the granular layer had thickened again to 4 cm consisting of the initial granular sea ice layer of 14 Jan, the snow ice layer of 16 Jan and an additional 2.3 cm thick snow ice layer (Fig. 4).

Knowing that, the porosity of the surface layer result of the formation of snow ice and freezing slush rather than change in freeboard. In the revised manuscript, we added in the result section the details of the seawater incursion events and treat separately the frazil ice from the granular snow ice where it was possible and relevant.

Flooding by seawater incursion are common natural sea ice environment throughout of the year, flooding is more common in the Antarctic but rare in the Arctic (although it may play a significant role in marginal seas such as the Greenland Sea). While in the Sea ice Experiment Research facility (SERF), the flooding was produced due to the development of a hydrostatic pressure under the sea ice , in nature, it happened while the snow load is able to depress the ice under the freeboard. *Massom et al., [2001]* explained: *"Freeboard is typically close to zero [Jeffries et al., 1998b], possibly due to cycles of snow-ice formation, implying that little additional accumulation is necessary to submerge the snow/ice interface and cause flooding [Eicken et al ., 1994]. Sturm et al. [1998] suggest that there is a "self-balancing" mechanism in snow-ice formation, whereby if a negative freeboard is observed, it has to be a short-lived phenomenon in most cases, as snow-ice will form shortly thereafter, reasserting the balance and producing a zero freeboard."* For more details, the study from *Massom et al.,* [2001] is excellent review on snow and sea ice interaction

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 distinguishing any results for these ice categories throughout the paper.
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.

In the revised manuscript, we provided new distinction between initial granular ice (i.e frazil) and granular ice formed from snow and subsurface processes. However, we can only do so for air porosity (mm scale) and to some extent to liquid porosity (cm scale) because the others parameters as bulk ice content and SAT_f are at coarse vertical resolution and does not always resolve the ice type.

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

All past studies on sea ice gas dynamics have been based on 5 cm vertical resolution measurement at the best. What you are calling "coarse resolution", in the sea ice community is considered as a fine resolution. Recently, several studies argued [*Zhou et al., 2013; Moreau et al., 2014*] that nucleation process, and bubbles migration might play an important role in regulating the sea ice gas content and air –sea ice fluxes. However none of these studies have been able to provide a realistic estimation of the air porosity. Our study is the first to compare traditional gas measurement to a realistic estimate of the air volume fraction. We truly believe that it is huge improvement. We fully agreed, that the coarse resolution does not always resolve the ice type and combining both data-sets might be ambiguous. However, we don't have the choice to make this combination. Eevery time we crossed the two scales, we carefully average the data (from sub-millimeter resolution) and reported the standard deviation as potential range over the coarse resolution.

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.
There is no fig. 14 a , then I supposed you spoke about the fig.11a and b

In the revised manuscript, Fig. 11a has been simplified to facilitate it's reading (Fig.12 in the revised version) and we provided separate graphs for each ice type for the Fig 11b (Fig.11 in the revised version) .

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 define

The red shaded area highlights the spatial extension of the physical transition in columnar sea ice between bottom permeable sub-saturated layers and the impermeable supersaturated layers. This transition layer is centered on the permeability thresholds of 5% of brine volume fraction , the lower and upper limit match the local increase of air volume fraction associated with the transition.

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 sub-saturated sea ice. However, saturation measurements have coarse (5 cm) vertical 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.

Agreed, that we did not provide enough distinction between ice type. In the revised manuscript, we provided individual statistic for the bottom columnar layer, for the intermediate impermeable columnar layer and for the granular layer. While it was possible and relevant, we differentiated frazil and snow ice layer.

However, the actual columnar bottom layers exactly match what we called in the first version 'the subsaturated sea ice' layer, the impermeable columnar layers match of what we called 'the supersaturated ice layer'. While in the revised manuscript, we discussed the bubble formation and brine movement by ice type, the foundation of discussion and conclusion did not change.

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 lost during sampling, which together with the cooling and storage process might have created what the authors call micro bubbles.

Bubbles formation at the ice-water interface have been discussed in past literature. The studies from *Killawee et al.*[1998] (i.e fresh water ice tank experiment) and Tison et al., [2002] (i.e sea water ice tank experiment) discussed the potential of nucleation processes at the ice –sea water interface. *Killawee et al.* [1998] and *Tison et al.* [2002] noticed that nucleation occurs at various saturation levels depending on convection at the ice water interface and on ice freezing rates. *Zhou et al.* [2013] wrote: Ar further accumulated in sea ice, we relate this behavior to its presence in the gaseous phase, and suggest formation of bubbles through convection processes and heterogeneous nucleation in permeable bottom '

As I highlighted in section II, It s unlikely that the air volume fraction in the remaining core bottom are drained brines, because it is actually in the bottom that the brine volume is the highest. Then, if bubbles were drained brines we would expect to see the air volume fraction increase where the brine volume increase. As mentioned in the revised manuscript, cooling caused bubbles disappearance (Light et al ., 2003).

We strongly argue that bubble can form and exist in the bottom convective layer.

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.

Regarding the result (i), we don't believe, it results from crossing coarse resolution with higher-resolved air porosity. For each ice core, we have a gas content measurement from which we deduced the $SAT_{f\ in}$ the bottom of the ice core. Moreover, past literature reported bulk ice gas concentration spanning the concentration at saturation with respect to calculated theoretical atmospheric gas concentrations in bottom sea ice as well [*Crabeck et al.,2014 a,b and Zhou et al.,2013*]. Result (ii) might be a consequence of crossing the two scales. In the revised manuscript, the results are mainly discussed for the impermeable columnar supersaturated layer, while the ice resulting from snow ice formation is discussed separately.

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.

Thank you for this comment, we fully agree. In the revised manuscript, we do not use the term accumulation anymore and clearly discuss the fact that a part of the air is trapped from snow porosity.

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 advance.

As explained earlier, potential nucleation in the convective layer have been suggested in past studies, our study is just a new observation of this process.

I don't understand what you mean by '*disappearing when the freezing interface advance*', as we show the air volume fraction as well as the total gas content increased over time.

SPECIFIC COMMENTS

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

Yes, we revised the sentence

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?

This is due to the error on the sample dimension (see previous comment)

P 5210, L 23 –> map – image done

P 5210, L 23 –> U-channels - what is this
It was energy channel used , we revised the all paragraph.

P 5211, L 4 –> pixel or voxel?

We revised the all paragraph

P 5211, L 6 –> pixels – voxels done

We revised the all paragraph

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

It was done for all the analysis

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

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

Yes , see previous comment

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

resolve
micro bubbles

Yes, we revised the sentence

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

See previous comment.

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.

We agreed and I am sorry for the confusion. 21.9 ml L^{-1} is the air solubility for sea water at 0°C , S=35. We re-done the calculation using *Garcia and Gorden* [1992] and *Hamme and Emerson* [2004] and we found exactly 21.5 ml .L^{-1}.

Then if the seawater freeze instantly, the bulk ice gas content should be around 21-22 ml L^{-1} , but because gas are rejected from the ice matrices , the total gas content is always lower than instant freezing sea water.

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.

See previous comment

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).

We agreed, as we don't have any time-lapse picture, we couldn't state that there are coalescence processes. We reviewed our interpretation in the revised manuscript. However according to our image, we believed that is reasonable to speculate that bubble can merge.

Macro bubbles are exclusively found in granular layer. They seems resulting of aggregation of discrete bubble like an aggregation of soap bubbles A succession of 0.6 mm thick transversal slices at +2.28 cm depth from January 25 is shown in Fig.14. In the first slice at +2.28 cm depth (Fig. 14, far left panel) four individual bubble bases are identifiable from which a single top bubble is formed at +2.46 cm depth (Fig.14 far right panel). The rapid freezing of slush in porous snow could potentially produce bubble aggregation.

Technical corrections

Fig. 3 –> Error bars for T, S_i and V_b? Fig. 4 –> Correct legend
We agreed that are potential errors on all these parameters however they are measurements and their precision is hardly quantifiable. In past literature, bulk ice temperature, salinity and brine volume fraction have been reported without error bars

Fig. 7 –> Log scale might show data better; error bars for V_b lacking. We revised entirely the Fig.7

Fig. 8 –> Perhaps invert colors
Fig. 8 –> Hardly readable We deleted this figure

Fig. 12 –> Scale on axes should start at 0. Consider log axes. Revised. We tried log x, but it did not improved the picture and found it less intuitive.

Ref. Kawamura (1988) is missing done
Ref. Lepparanta and Manninen (1988) is not used in the text done

Reference:

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