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.

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 (∅ < 1 mm), large bubbles (1mm < ∅ < 5mm), and macro bubbles (∅ > 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 particularlyawkward.**

We revised the fig caption.

**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 “accumulation”** 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

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