Review of egusphere-2026-1440

I Summary and assessment

This is a review of the manuscript *Estimation of sea ice air-bubble and brine pocket distribution for scattering and emission model parametrization* by Rasmus T. Tonboe et al.

The authors present an analysis of the composition and microstructure of pores in sea ice from the MOSAiC expedition, based on vertical and horizontal thin section analysis of sea ice cores. Results are presented data for various ice types (new ice, first-year ice, level second-year ice, second-year hummocks, and a refrozen melt pond).

First, though the relevance of microstructure for sea ice remote sensing has been pointed out more than 3 decades ago, investigations of that kind are sparse, which is a considerable knowledge gap. Hence, the effort of the authors is to be applauded considering the amount of work that this has taken. However, there are serious issues related to the data quality, image analysis, and the destructive nature of the thin sectioning process, that limit the reliability of the results and their comparability to other data sets. At present it is not fully clear to me, if the results obtained from 2D thin sections alone can be used quantitatively as input to remote sensing algorithms. As described in this review there are many aspects that should be revised and the work needs a major revision to figure this out.

II Specific comments

General topics

1. The introduction gives a good overview of the relevance of microstructure for remote sensing of sea ice. However, the general reader would profit a lot from a table or heat map that summarises the microstructural (inclusions lengths, number and aspect ratios) and bulk properties (brine and air volume fractions, brine temperatures) and ranks them in terms of their relevance for active and passive remote sensing (e.g. by +, ++, +++ etc.).

2. On average, inclusion densities obtained by the authors in 2D thin sections should correspond to the sum of 3D brine and air volume fractions. The authors find inclusion densities that, depending on ice type and location, range between 0.05 and 0.45. However, when checking the brine volume fractions for new ice and first year ice, where it can be computed from the ice salinity (and the temperature in the cold room), one obtains brine volume fractions that are much smaller. E.g., for young ice (Table 2) the inclusion densities (0.09-0.18) obtained by the authors are a factor of 2-3 larger than the nominal brine volume fractions (0.05-0.07 based on given ice salinities). In many samples this would imply air volume fractions of 0.1 or higher.

Such air volume fractions of 0.1 are unlikely for young and first-year ice, for which 3D observations mostly show values in the range 0.005-0.02 and maximum of 0.04 (Crabeck et al., 2016; Maus et al., 2021; Salomon et al., 2021) and also high accuracy density data seldom show air volumes exceeding these ranges (Pustogvar and Kulyakhtin, 2016). Hence, considering other studies, the determined inclusion fractions appear to be a factor of two too large. While the authors try to explain this with brine drainage during sampling and processing - this should also have affected other studies, and due to the cold conditions under which many MOSAiC cores were taken, I would not expect that these samples should be outstanding in this respect. Also the density values tabulated by the authors (from FY ice and young ice) are not supporting such high air volume fractions.

3. The air volume based on the inclusion densities derived from thin sections should be quantified in the table. This can be done by subtracting the brine volume based on salinity (that also should be listed, when possible) from the inclusions density.

Also two explanations of these too high apparent air volumes should be better discussed:

- May non-destructive thin-sectioning lead to much higher inclusion density due to some surface and melting effects?. I am not aware of studies that have clearly shown this.
- In the freeboard one can expect larger air volume fractions, and the tabulated data appear to show largest inclusion densities there. This difference between results for the ice above and below freeboard should be outlined and quantified - highlighting the freeboard data in images.

4. I recommend a supervised segmentation to circumvent the problem of too large inclusion density. As known from earlier work (Perovich and Gow, 1991, 1996, e.g.,) the method of thin section analysis can hardly distinguish between air and brine inclusions. Perovich and Gow (Perovich and Gow, 1991, 1996) thus approached the segmentation problem as follows. For young and first-year ice they neglected the air volume and set the segmentation threshold to that grey level at which the inclusion density matches the theoretical brine volume based on salinity and temperature. When density measurements are available, as for some MOSAiC data, one may set the threshold porosity to the brine volume plus the air volume based on density.

5. Such a supervised segmentation, previous point 4., should give more reliable length scales. The authors have argued that changes in the segmentation threshold in the ranges tested have little effect on inclusion lengths, but they only describe a threshold change that increases the inclusion density (Section 5.1 and Fig. 16). However, as the applied segmentation seems to give too high inclusion density, this should be changed and the effect on length scales determined.

6. The authors use the autocorrelation function to determine the pore length scales in vertical and horizontal cross sections. This method is known to be robust to noise. However, the autocorrelation method could also be applied to unsegmented grey level images. If one invests the effort to segment images it is more suitable to employ other pore size metrics of which several have been employed in earlier studies (Lieb-Lappen et al., 2017; Maus et al., 2021; Salomon et al., 2021; Oggier and Eicken, 2022, e.g.,). I rate it as unlikely that the pore anisotropy is properly retrieved via the autocorrelation function when the grain orientation (c-axis, direction sub-grains or plates) changes within the area for the autocorrelation (in that the latter contains several grains). There are little studies of such details for sea ice, but the master thesis by Buettner (2011) does clearly show this.

Specific comments

L 48-53. As under general topics, consider a table that states the different properties and ranks them in terms of importance for microwave remote sensing.

L 51-62. Consider to show a figure sketch where the different length scales are defined.

L 54-57. Can you discuss the question: What is better to have for microwave modelling - the autocorrelation lengths or the exact inclusion size and aspect ratios?

L 57-62. I would not give so many specific numbers here, but be more general, and keep the numbers for the discussion.

Fig. 3. More examples for the different ice types, and vertical versus horizontal cuts, would be good to show here, that the reader gets a qualitative impression.

L115-116. *we believe that the brine has drained during the processing of the FYI samples and that the dark patches/ inclusions in the sample are now filled with air.* –> This has not been documented by e.g. Perovich and Gow (1996) and I rate it unlikely for FY ice. To support this statement, at least to get an indication, one needs to melt the thin section ice, measured its salinity, and correct for the distilled water amount for gluing.

L124-L132. The ordinary and transposed directions are clearly defined, and have a physical meaning for vertical thin sections. But how do you treat the horizontal ones? In that case one should choose the ordinary direction normal to the plates (in direction of the c-axis). If not the analysis will not give the minor and major axis lengths properly.

L131. While ACF has been defined in the introduction I would repeat this here (or move definition here).

L135-L147. Two points are noteworthy: (i) This discussion about the uncertainty in the autocorrelation length may not be applicable, as there are other length scales than the sample diameter that are involved (grain size, spacing of wide secondary brine channels). E.g., when the grain orientation changes. (ii) The advantage of using an autocorrelation function is that it is robust to noise and can be applied to non-segmented images. This probably explains why the authors find little change in length scales when changing the segmentation, see section 5.1. However, the approach of using an autocorrelation length likely has limitations for microstructures with different modes of inclusion sizes and orientation. Other methods to obtain length scales (e.g. mean intercept length or ellipse fitting algorithms) may be superior.

L195. Is there any reason for chosen 2.5 and 97.5 percentiles. Choosing 10 and 90 would allow comparison with other work, for example Eicken et al. (2000).

Fig. 11. With these axis scales it is difficult to see the data trends and the difference between horizontal and vertical sections. I would plot the data without confidence - just the median values, and perhaps use an extra Figure for the largest and smallest pores

(2.5 and 97.5 percentile, or other choices).

L207. *transposed* –> similar to the note on L124-132 and the discussion of the autocorrelation approach: The L_x/L_y anisotropy for horizontal sections can only be derived properly in the following way: 1. Selecting single grains 2. Align the ordinary direction with the plates in the grains.

L203-243. The results paragraph with many numerical values is only illustrated by one Figure (11), which could be improved.

L247-248. *On average, there is a relationship between salinity and inclusion density, but we do not have enough data points to properly characterize this relationship shown in Figure 12.* –> As pointed out above (general comment 2) this relationship should be essential for the segmentation process and interpretation of the results. In the Figure you should plot the line for the theoretical brine volume that falls below all data points. You may also use a second scale on the abscissa for the brine volume (as salinity and brine volume are very close to linearly related at fixed temperature). The difference between the line and ypur data points will then correspond to the air volume fraction that your analysis corresponds to.

L305-307. *We were unable to characterize the relationship L_{obs} with the new- and first-year ice salinity and we were unable to confirm the systematic horizontal anisotropy ratio of 7 as reported by Nghiem et al. (1995b) due to an insufficient number of samples. However, the lead / new-ice sample from 24 January 2020 showed a horizontal anisotropy (Ly/Lx) of 1.4.* $->$ see general point 6 above, that the method here is not suited to derive anisotropy. In addition lower temperature may facilitate splitting of pores - see note on L303-304. Note also that Nghiem et al. (1995b) did not make own observations but only assumed such ratio based on limited observations.

L303-304. Smaller inclusions could be related to imaging at a temperature as low as -15 °C. At such low temperature the pores break up into strings of less anisotropic inclusions see (Assur, 1958; Anderson and Weeks, 1958; Weeks and Ackley, 1986) for earlier conceptual work and Mausetal2021,Maus2025 for a more recent analysis of micro-CT data. Spatial resolution may also be an issue that limits the determination of thin pore featues. The voxel size is only 30 μm in this study and gives a nominal spatial resolution (Nyquist Theorem) of 2 x 30 = 60 μm. In view of typical similar median pores sizes at low temperatures discussed by Maus et al. (2021) this is likely too small to retrieve connected pore distributions properly.

L305-307. As noted above, under general points, the autocorrelation function is, for variable grain size directions, not suitable to derive the anisotropy properly. The data on pore aspects rations based on which Nghiem et al. (1995b) had based their model were obtained from pore analysis and may represent a more realistic anisotropy (though these values must be viewed as tentative as they were based on qualitative inespection of 2D thin sections rather vrthan statistics). However, as noted in the last paragraph, small aspect ratios may also be the the consequence of temperature-dependent splitting of long pores into strings of inclusions.

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