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# Comment on egusphere-2022-552

Anonymous Referee #1

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Referee comment on "Modeled variations in the inherent optical properties of summer Arctic ice and their effects on the radiation budget: a case based on ice cores from 2008 to 2016" by Miao Yu et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-552-RC1, 2022

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This manuscript describes a study using sea ice microstructural property observations recorded over a broad region in the Arctic Pacific sector during the interval 2008 – 2016 to compute changes in the inherent optical properties of the observed ice. This paper takes air volume and brine volume observed by Wang et al. (2020) as the basis for computing inherent optical properties (scattering coefficient, absorption coefficient, and scattering phase function asymmetry parameter) and apparent optical properties (albedo and transmittance) for sea ice.

The text and figures are clear as presented. I do have major concerns with the method and the conclusions that were reached. There is a general lack of rigorous statistical treatment applied to this dataset. To do this study of interannual variability correctly, it is necessary to first establish the (regional) spatial and temporal variability in a single year. The variability in microstructure properties is affected by temperature and number of melt days, but also potentially by absorption of shortwave radiation, melt water flushing, synoptic weather (e.g., rain events), surface vapor condensation, surface melt pooling, and other factors. Many of these processes would be expected to drive significant spatial and temporal variability in the brine and gas volumes in sea ice, especially in the uppermost portions of the ice cover. The spatial and temporal sampling are not adequate to draw the conclusion that the brine and gas volumes have changed in response to spatially large and temporally long changes in climate.

Lines 15 -17 (abstract) illustrate my point: "Compared with 2008, the volume fraction of gas bubbles in the top layer of sea ice in 2016 increased by 7.5%, and decreased by 50.3% in the interior layer. Meanwhile, the volume fraction of brine pockets increased clearly in the study years." With no knowledge of the spatial or temporal variability of these properties within a single region / year, attribution of their interannual variability is unfounded.

The temporal variability question here may be tied to the sampling period. Line 59-60 reads "Almost all cores were sampled in August, when the ice had started to melt." I would argue that data taken in August likely exhibit very strong short-term temporal variability. By August, the ice surface has likely been melting (losing mass) for at least a month. It is also possible that by August the surface melt has ceased. The brine and gas volumes may thus be changing quickly, and not monotonically at this summer/autumn transition time. It is possible that the sampling was carried out without spatial or temporal biases, but the authors have not presented a convincing  statement that this is true.

Line 143 "There were clear increases in the *V*b of all three ice layers (Figure 3b), which implied dramatic variations in the permeability of summer sea ice." There is no discussion of how permeability is measured or modeled.

Line 145: "From 2008 to 2016, the increase in the IL was clearest." This is a qualitative statement and contains no robust statistical assessment.

What physics drive changes in sea ice scattering coefficient? Temperature is certainly a primary driver, at least initially. But it is by no means the only driver. Once the ice surface is melting its temperature changes little.

Lines 156 – 158 "2). Although the *V*b values of the ice cores increased clearly with depth, they did not enhance the scattering capacity of ice. The reason for this was that the refractive indices of brine pockets and pure ice are close (Smith and Baker, 1981; Grenfell and Perovich, 1981)." It is certainly true that the refractive indices of brine and ice are close, but even small changes affect scattering.

Section 3.3. Are the reported AOPs observations? Or are they calculated with a radiative transfer model? Caption for Fig. 6 says "estimated", so I am left to infer these are calculated, not observed. It would be interesting if there were a comparison between these calculated values and observed values.

Line 231 – 233 asserts: "Meanwhile, *E*a decreased from 15 W m-2 in 2008 to 13.8 W m-2 in 2016. As the

decrease in ice volume from 2008 to 2016 was 32.2%, the solar energy absorbed by a unit volume of sea ice increased by 35.7% on the Arctic scale." This would be an interesting result if it was based on rigorous assessment. It is difficult to discern however whether it is rather based on propagated error.

Lines 300 – 305: "Extensive measurements of the IOPs of Arctic sea ice have been carried out, and some authors have noticed the seasonal variations of the ice microstructure and IOPs (e.g., Light et al., 2008; Frantz et al., 2019; Katlein et al., 2021). However, interannual variations in sea ice IOPs are still not clear, although such changes in sea ice extent, thickness, and age are evident. A lack of continuous IOP measurements is the primary reason. Compared with previous observations, the ice core data in the

present study were more appropriate for interannual analyses of the IOPs of ice because of their long time span and consistencies in the sampling method, seasons, and sea areas." Yes, I agree with this statement. I also agree this data set is "more appropriate". But, "more appropriate" still needs to be handled carefully. I don't find it appropriate to assume that because it is "more appropriate" that it is appropriate enough.

Lines 316 – 317: "For $\sigma$, there were no clear changes in the TL. This demonstrated that the variations of $\sigma$ in the TL largely resulted from interannual factors." I completely agree. But there is no elaboration on what these interannual factors could be. Rain/snow? Ice dynamics? Length and intensity of melt season?

Lines 317 – 318: "With an increase of latitude, the $\sigma$ of the IL tended to increase." Yes, it would be expected that the ice at lower latitude is generally warmer earlier in the season. This internal warming would be expected to lead to increased brine inclusion size and connectivity. This connectivity would naturally lead to brine drainage, and reduced scattering coefficient. This seems like a useful, justifiable result, but I don't believe this is the point being made here.

Lines 325 – 326: "The amount of surface radiation during the study years was also similar (Laliberté et al., 2021)." This is a very sweeping generalization. I would expect the details of this study to be quite sensitive to short time scale variations within this generalized picture, and for the ice state to respond to these variations.

Figure 11(a): I would expect T_air to have synoptic (temporal and spatial) variability. I would expect TL scattering coefficient to be sensitive to integrated solar radiation and surface vapor deposition. I think the correlation implied by this figure (as stated in Lines 348 – 349 "In summary, the differences in the IOPs of the ice cores were related to interannual variations in the air temperature and ice age" is misleading.