Dear Reviewer:

Thank you for your comments concerning our manuscript (ID: egusphere-2022-552). Those comments were very helpful for revising and improving this manuscript, as well as for providing important guidance to our study. We have considered the comments carefully and will make enough changes to the manuscript. The responses to the reviewer's comments are provided in blue, as follows.

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.

We have supplied more statistical treatment to the ice core data. Then, the effects of spatial and temporal variability on the results can be reduced. The conclusion has been revised accordingly. First, to reduce the impact of temporal variations in the ice cores on the ice microstructure, we preprocessed the ice core data. The ice cores in each year were allocated different weights according to their sampling date. Subsequently, Figures 2–8 have been updated. It is a big challenge to make clear the effects of shortwave radiation, flushing, rain events, etc. on ice properties according to the available literature. Whereas, the mean sampling date of ice cores was Aug. 18 ± 9 days (described in Section 4.2). According to the previous observations, ice surface melt was most intensive in July, and relatively stable in August (Perovich et al. 2003; Nicolaus et al. 2021). Meanwhile, it can be seen from the melt onset data that the melting days of the sampling sites were similar (58 ± 7 days). In other words, although the radiation, flushing process, etc., affected the seasonal variations in the microstructure of ice, their combined effects did not introduce many variations to the melting days of ice. It could be expected that the brine and gas volumes in sea ice will not change obviously over the course of several days.

As for the spatial variations in the ice cores, more discussion has been supplied in Section 4.2. We have quantitatively analyzed the effects of spatial variations on the interannual variations through the propagation law of error. Some conclusions have also been amended accordingly. As we know, it is difficult for field observations to avoid the effects of spatial variations. Therefore, related studies have generally ignored the effects of sampling locations on the statistics (e.g. Carnat et al. 2013; Katlein et al. 2019; Frantz et al. 2019). In the present study, Fig. 10 shows the inherent optical properties of the ice cores in different latitude zones. It can be seen that there were indeed spatial variations in the ice properties. However, we noted that the spatial variations were relatively smaller than the interannual variations. In other words, interannual factors played a more important role in the ice properties than spatial factors. This was what we have sought to express clearly in Section 4.2.

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 abstract has been rewritten. Information about the effects of the spatial and temporal variations on the interannual variations has been supplied. We have quantitatively shown the roles of the interannual and spatial variations on the changing microstructure and optical properties of ice.

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.

As stated in the reply to another comment, we preprocessed the ice core data to reduce the effect of the temporal variations in the ice cores on the statistical results. The weights of the ice cores sampled at early or later dates have been reduced. Meanwhile, according to the melt data from NASA, the ice cores in the present manuscript were all sampled during the late melting season. As such, the sea ice had been melting for a while (~58 days) and had not yet begun to freeze (it needed another ~15 days). According to previous observations, the ice surface melt rate in August was only ~1/10 of that in July (Perovich et al. 2003; Nicolaus et al. 2021). Therefore, short-term temporal variability was expected not to affect ice obviously. This information has been supplied in the revised manuscript.

Line 143 “There were clear increases in the Vb 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.

Yes, the permeability of ice was not the main target of the present manuscript. This sentence has been rewritten in the revised manuscript.

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

Statistical descriptions of variations has been supplied in the revised manuscript.

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.

For now, we have little quantitative knowledge of the seasonal progression of the sea ice scattering coefficient or microstructure and its influencing factors. In 2008, Light et al. first showed some evolution of IOPs during the course of the summer for the multiyear ice observed at SHEBA, but did not discuss their influencing factors much. The reason we analyzed the relationships between temperature and the ice scattering coefficient was because the former was an important parameter in sea ice models. Furthermore, temperature was easy to measure. If empirical or semi-empirical relationships between temperature and the scattering coefficient could be determined, it would be useful for related studies and models to set ice parameters.

Lines 156 – 158 “2). Although the Vb 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.

What we wished to express was that the effects of the changing brine pocket volume was covered by the changing gas bubble volume. This sentence has been rewritten to reduce the previous ambiguity.

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.

Yes, they were estimated. Some descriptions will be supplied here. Similar parameterizations have been widely used to link ice microstructure with optical properties and have been verified by extensive observations (e.g. Taskjelle et al., 2017). Radiative transfer models are also commonly used, whose accuracies are widely accepted.

Line 231 – 233 asserts: “Meanwhile,Ea 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.

Similar results have been observed in a related study. Section 4.2 of the present manuscript showed that the ice ages of these ice cores were different. Light et al. (2015) showed that the thickness of first-year ice was less by 13.3% than multiyear ice (1.3 m vs. 1.5 m, respectively). Whereas, the radiation absorbed by the former was less by 2% than the latter. In other words, the solar energy absorbed by a unit volume of first-year ice was greater than multiyear ice by 12.5%.

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.

Yes, we agree with you. We have supplied more information about the spatial and temporal variations of the ice properties. To reduce the temporal variations, the weights of the ice cores sampled at early and late dates have been reduced. Quantifying the effects from spatial variations is a big challenge because little quantitative knowledge is known about them. Instead, we have quantitatively analyzed their effects on the interannual changes. Although the present ice core data set did not form a strict time series in the classical sense, it could be used to derive a qualitative picture of the changing ice microstructure. After making the temporal and spatial variations clear, the interannual variations then become clear, and we have sought to unambiguously convey our results in the revised manuscript.

Lines 316 – 317: "For σ, there were no clear changes in the TL. This demonstrated that the variations of σ 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?

In the following part of Section 4.2, we discussed the interannual variations in the melting days, temperature, surface radiation, and ice age. They were all important factors related to the development of ice properties. We found that melt days and radiation in the study years were relatively stable, while the air temperature and ice age clearly varied. Thus, Figure 11 mainly discusses the effects of these two latter parameters on the ice optical properties. More discussion has been supplied in this section in the revised manuscript.

Lines 317 – 318: "With an increase of latitude, the σ 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.

Yes, what we wished to express in this paragraph is there were, inevitably, spatial variations in the ice cores. Furthermore, the spatial variations were relatively less than the interannual variations. Following this, a discussion regarding the reason for the interannual variations has been provided in the text. We have also reorganized this part to make it clearer than before.

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.

The mean surface radiation in the study years was 99.4 ± 6 W, which was determined from the reanalysis data (ECMWF). Definitely, short-time scale variations in radiation affected the ice properties. In this section, we have paid attention to the interannual variations of ice optical properties and their reasons. After checking the relationships between the ice optical properties and potential affecting factors, we found that the interannual variations in the surface radiation could hardly explain the changing ice optical properties. Indeed, there were some more important factors resulting in these variations. We have supplied more descriptions of the variations in other climate factors in this section, and have discussed the relationship between these factors and the ice properties.

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.

Yes, there were temporal and spatial variations in air temperature. We would like to emphasize that the increasing temperature in Figure 11(a) was not an exception but a general circumstance in the Arctic during 2008–2016 (Collow et al., 2020). This could also be seen in the reanalysis data, where the mean air temperature in the summer of the study area has been increasing gradually (0.12 ℃/year, correlation coefficient $r = 0.84$). This trend agreed well with our observed temperature measurements (0.14 ℃/year, $r = 0.84$). However, there did not appear to be a clear trend in the solar radiation ($r < 0.5$), and the correlations between radiation and the ice optical properties were not clear. So, although solar radiation and vapor deposition could affect the seasonal variations in ice clearly, we thought that the air temperature played a more important role in the interannual variation in ice. We have supplied more discussion about these factors to make our view clear.