Dear Handing editor Dr. Marie Dumont,

Thank you for handling our manuscript (ID: egusphere-2022-552). Based on comments from reviewers, we made further revision of our manuscript. Please see below point by point responses to the reviewers' comments (blue) The revised parts in the manuscript are marked in red.

Best regards,

MiaoYu and co-authors

Anonymous referee #3

The manuscript is well written though minor language edits are suggested.

R15. The first time the term Va is used it must be defined.

Defined accordingly (L15).

R17. Suggest to add over time or something instead of "in the study years" or even indicate which years are being addressed here.

Done (L17).

R60. detailly -> details

Corrected (L59).

R79. On R72 the time since melt onset was presented as 59 days, why wasn't this date used here instead of 30 days?

We have tested the effects of *D* on weighted mean values. The results revealed that the weighted mean values and standard deviations were nearly constant when the *D* was large enough. This can be seen from Figure A. 30 days was an appropriate value for most ice cores in the present study. We have added this information in the revised manuscript (L83-84).

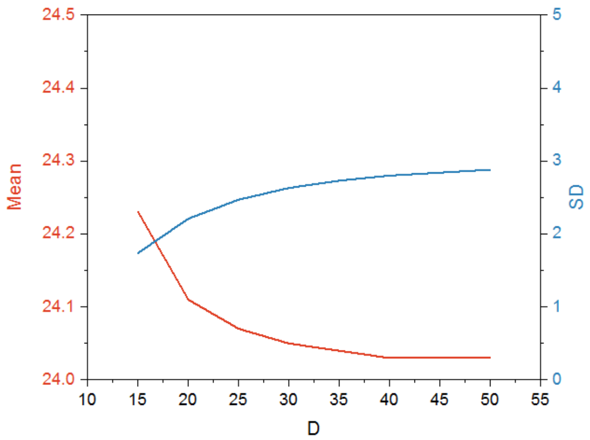

Figure A. Effects of D on the weighted mean values and standard deviations. Taking the TL-Va of ice cores in 2008 as an example.

R147-149. Perhaps you can elaborate a bit about the possibilities of different locations and in how this affects the results?

The decrease of V_a with the depth is a common feature in sea ice (e.g. Crabeck et al. 2016). This is controlled by the ice growth conditions (Some information was supplied in L90-93). So, it is expected that different locations can't affect this decreasing trend.

R155-156. The year of 2012 was one of the sea ice minimum record years, has this affected the results?

Yes, Figure 11a of the present manuscript also demonstrated that the number of melting days of ice cores was the largest in 2012 from 2008 to 2016. However, we didn't link the present result with this phenomenon directly, because the difference in the scale was so great. More ice cores would be needed to analyze the relationship between them.

Figure 2. the results are presented as a combination of FYI and MYI, would it be possible to see a separation between the different ice types instead in this graph?

As discussed in Section 4.2 (Fig. 11), we have shown the exact age of ice cores in each year but not sorted them into FYI and MYI classes. Then, we found that changing ice IOPs in each year was connected to the different combinations of FYI and MYI but we didn’t show information about FYI and MYI separately here. This was reported in Wang et al (2020) who used the same dataset.

R225-226. An ice thickness of 1m is rather thin sea ice. In the European Arctic I would perhaps pick an average of 2m instead. Could you provide some justification of the chosen ice thickness? Figure 6 seems to indicate that most ice is thicker than 1m also in the data used here.

In this section, we estimated the ice optical properties by setting a constant thickness reference (1m) to scale the ice microstructure relative between the surface and bottom. The actual mean thickness of all ice cores was 1.2 m. On the other hand, the reference thickness value doesn’t affect the trends in Fig 6. We have added this information to the revised manuscript. (L232-234)

R306-307. The data used in the study from 2008 likely contain ice of the type observed at the start of this study. Will the timing (2008 vs 2021) of the ice be important to consider for this comparison?

First, as discussed in Section 4.2, we didn’t find significant variations in the IOPs of the ice top layer (Fig. 10). So, it is expected that the timing of the ice sample can hardly affect the comparison. Second, the variations in IOPs in the present literature were still not clear, so it is difficult for us to consider the potential affecting factors. So, we pay more attention to the comparison of the IOPs range in Fig 9. We have added this statement to the revised manuscript. (L308-310).

R370-372. This sentence reads a bit strange the statistically significant trend is not what is being provided from ECMWF, consider rewriting it.

Revised accordingly (L378-380).

Main references

Crabeck, O. and Galley, R., et al., 2016. Imaging air volume fraction in sea ice using non-destructive X-ray tomography. The Cryosphere, 10 (3): 1125-1145. doi:10.5194/tc-10-1125-2016

Wang, Q. and Lu, P., et al., 2020. Physical Properties of Summer Sea Ice in the Pacific Sector of the Arctic During 2008–2018. Journal of Geophysical Research: Oceans, 125 (9). doi:10.1029/2020JC016371

Anonymous referee #4

I realize that this is a revised manuscript. In this manuscript, the authors have tried to address the issues on the variations of the inherent optical properties (IOPs) of summer Arctic ice and their effects on the radiation budget. By using the observations obtained from the CHINARE during the summers of 2008 to 2016, they found that the changing microstructure of interior ice from 2008 to 2016 has led to the significant decrease in the scattering coefficient. They also reveal that such variations of microstructure and IOPs play an important role in the radiation budget of Arctic sea ice.

The paper is very well written and very informative. I think it well fits the scope of the journal, however, I still have some comments and minor suggestions as follows.

Abstract

Line 15: The full name of Va should be given here.

Revised accordingly (L15).

Data and method

Lines 71~75: Confusing about this statement. Do the authors want to express that the variability of IOPs during entire summer is very small? If yes, how should I understand that when the surface melting in August is only 1/10 of that in July, the state of surface ice microstructure in August is like that in July? If no, can observations concentrated in August represent the entire summer? I think it is necessary to clarify the representativeness of observations rather than just use ‘summer’.

According to the observation in MOSAiC, the surface scattering layer (SSL) of sea ice re-formed within a couple of days after removal (Smith et al., 2022). Macfarlane et al. (2023) found that there are no clear temporal changes in the microstructure of surface ice in the entire July. Furthermore, other observations have revealed that the ice surface melt rate in August was lower than in July (e.g. Perovich, 2003). So, it is expected that the microstructure of the ice surface develops rapidly in the early melting season, and is similar in the mid- and late-melting seasons but not the entire melting season (or the entire summer). We have revised these descriptions to make them clear (L74, L76-79).

Can you provide more information on ice in each year (e.g., thickness, age) in this section? Because it is important to explain your results.

The general description of the thickness and age of ice cores has been added here (L70-71). More detailed information has been supplied in Fig 6 (ice thickness) and Fig 11 (ice age). So, we didn't show the details here repeatedly.

Section 2.3 Arctic-wide up-scaling: My main concern on this part of analysis is whether your observational results is suitable for all the Arctic sea-ice types. I think more details should be concerned when you do up-scaling. For example, setting different values for the first-year and multi-year ice.

The different values for the first-year and multi-year ice have been considered in this section. As shown in Fig. 11, the age of ice cores in each year was different. That's to say, the changing ice IOPs resulted from different combinations of FYI and MYI in each year. L424-441 has in detail discussed the relationship between ice age, ice IOP, and ice microstructure.

Results

Line 203: 'there are' –> 'there were'.

Revised accordingly (L209).

Lines 209~210: 'Furthermore, the developments of κB in the three layers are similar (Figure 5b)'. Can you give correlation coefficients to support this statement?

Three fitting trends have been added here (L216).

Discussion

Figure 9: It is interesting that other studies present a noticeable smaller variation in σ for IL compared with your results? Can you explain this difference?

We have discussed the possible reason for this difference in Section 4.1 in detail. First, there isn't a unified method to get the ice IOPs to date. The result of IL from Light et al. (2015) and Frantz et al. (2019) were obtained by the same method, and they are smaller than the present result. Results from Mobley et al. (1998), Grenfell et al. (2006), and Perron et al. (2021) agree with our results. The method of these four studies was not the same. So, it was expected that the differences in the IL's σ partly resulted from the different methods used in the myriad studies (See details in L317-325).

Another possible reason comes from the brine loss during measurement and segmenting. Thus, our V_a values of the IL may be greater than the values derived from nondestructive methods. Taking the mean V_a and V_b of all ice cores as an example, these uncertainties overestimated the σ of the IL by 78 m^{-1} at most. This information has been supplied in detail (L326-333).

Main references

Smith, M. M. and Light, B., et al., 2022. Sensitivity of the Arctic Sea Ice Cover to the Summer Surface Scattering Layer. Geophysical Research Letters, 49 (9): e2022GL098349. doi:10.1029/2022GL098349

Perovich, D. K., 2003. Thin and thinner: Sea ice mass balance measurements during SHEBA. Journal of Geophysical Research, 108 (C3). doi:10.1029/2001JC001079

Macfarlane, A. R. and Dadic, R., et al., 2023. Evolution of the microstructure and reflectance of the surface scattering layer on melting, level Arctic sea ice. Elementa: Science of the Anthropocene, 11 (1). doi:10.1525/elementa.2022.00103