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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence of ice crystals at cloud top of Arctic boundary-layer mixed-phase clouds derived from airborne remote sensing" *by* A. Ehrlich et al.

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The comments of the reviewer have been helpful to improve the manuscript. Several points of the manuscript were annotated where important information was lacking. Including this information in the revised manuscript made the manuscript much more understandable. The detailed replies on the reviewers comments are given below.

The reviewers comments are given italicized while our replies are written in roman letters. Citations from the revised manuscript are given as indented text.

Detailed Replies

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Interactive Discussion

Discussion Paper

I am not surprised that the in situ measurements could not be reconciled with the cloud reflectance measurements. I was a little surprised that the cloud optical thickness had to be more than doubled to match the visible reflectance, but I don't think it is necessarily unrealistic.

Sure a factor of more than 2 is a lot. But this counts only fitting the simulations to the mean measurements. Considering the uncertainty range of the reflectivity measurements the picture changes. We repeated the simulations with fitting the results to the lower boundary of our measurement uncertainty. Depending on which wavelength range we trust more an optical thickness of $\tau = 14$ fits the lower uncertainty range of our measurements between 500-900 nm wavelength and tau=12 between 1000-1300 nm. This means trusting the 1000-1300 nm range the differences between the in situ optical thickness and the simulations would be reduced to $12/8 = 1.5$. This seams more realistic.

Anyway, further simulations not shown in the manuscript using $\tau = 12$ did not change the findings on the vertical distribution of ice crystals as discussed in the manuscript. The wavelength range important for this study (1400–2100 nm) is less affected by τ.

In the revised manuscript we added following comment:

> It has to be pointed out that the large factor between τ derived from in situ measurements and τ derived from the reflectivity measurements is reduced when the uncertainties of the SMART-Albedometer are considered. Fitting the simulations to the lower boundary of the uncertainty range in wavelengths below 1400 nm (not shown here) yields $\tau = 12$. However, further simulations have shown that the spectral signature in the wavelength range of strong ice absorption and thus the investiga-

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tions shown below are not significantly affected by the optical thickness of the cloud.

What I found lacking was no description of the in situ and remote sensing sampling flight strategies. What kind of flight legs for each? Vertical spirals for in situ? How long? How closely matched in space and time were the in situ and remote sensing flight legs? What was the spatial footprint at cloud top of the reflectance measurement?

Thank for this hint. These information are absolutely necessary for such a closure study. Thus we added following part to Section 3 of the revised manuscript.

> In situ measurements are obtained from two subsequent ascents and descents each of about 6 min duration. The cloud-top reflectivity measurements have been conducted on the same flight track about 10 min later. The horizontal leg was performed at 1770 m altitude which was about 150 m above cloud top. For this short time delay, advection can be neglected considering the wide cloud field as shown by satellite images. The reflectivity data have been averaged over a time period of about 18 min. The temporal variation of the data did not show a significant change of the cloud characteristics.

And finally, what I really could not understand was the attributes of the single cloud reflectance spectrum applied to this study: was this really a single spectrum or was it averaged over a flight leg? What were the authors' motivations/ justifications for analyzing this single spectrum (rather than an average, etc.)? Was this representative of the entire domain under which the cloud was

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sampled?

As replied to the comment above we added this missing information in the revised manuscript.

Could there have been any chance that the underlying surface was not open ocean? My immediate concern was that variable surface albedo would impact the observed spectral reflectance over this low- to moderately thick cloud layer. However, the single instance in the text where surface albedo was discussed suggests that indeed the measurement was made over the Greenland Sea and that a measurement of the ocean albedo under the cloud layer occurred on that same flight.

Yes, we are sure that the ocean below the cloud was ice free. First, during the in situ sampling we did fly below the clouds. Second, the Greenland Sea is still influenced by warm surface water from the North Atlantic current and thus in large areas ice free over the entire year. Third, satellite pictures of the time period showed that the whole area was ice free.

What is less clear is how unique these results are; could other combinations of vertical ice/water distributions and cloud particle sizes produce similarly good matches? Unlike the authors, I include cloud particle size as a free parameter because it too could be biased by the same sampling issues that impacted other elements of this study.

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We performed a number of other simulations changing particle size, vertical distribution etc. which all did not result in a good fit to the measurements. We did not want to include all these results in the manuscript because this will blow up the study dramatically and draw of the attention of the reader from the major findings.

With regard to the particle size we can explain that increasing the size of ice crystals can reproduce the low reflectance at 1490 nm. But simultaneously the reflectance at 1700–1800 nm is reduced. This is similar to the simulations varying the ice fraction. In both cases the ice absorption is increased 'wavelengths independent' which means the change follows the refractive index if ice multiplied with a wavelength independent factor. Only if the vertical distribution of ice crystals and liquid droplets is changed it is possible to derive a wavelength dependent change of the reflectance. This is possible because the vertical sensitivity of the reflectance is wavelength dependent.

The conclusion from this study suggests that our model of Arctic mixed phase clouds dominated by liquid water at cloud top and ice at cloud base may need to be revised. This question of uniqueness of the results would need to be resolved before the full impact can be understood.

With help of reviewer #1 we came to the conclusion that this presence of ice crystals is necessary only in the vertical layer in which the reflectance measurements are most sensitive. This means ice not necessarily has to be directly at cloud top. Case D in the revised manuscript shows that ice close to cloud top also reproduces the measurements. This supports recent literature in which is stated that ice is found throughout all layers of Arctic mixed-phase clouds. In this regard we revised the manuscript (see replies to reviewer #1)

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Again, without reading the pervious paper the indices don't mean much. But to describe the physics than explain the behavior of indices that may not be understood by the reader

In section 3.2 we added a short description of the ice indices in the revised manuscript to give the reader some insight to the physics of these indices.

> The ice indices weight the ice and liquid water absorption present in the spectral reflectivity measurements. The spectral slope of R in the wavelength range between 1550 nm and 1700 nm is utilized in the spectral slope ice index I_S. Principle components related to ice and liquid water absorption are compared in the principle-components ice index I_P. For pure liquid water clouds, values of $I_S < 20$ and $I_P < 1$ are obtained. Strong ice absorption increases the spectral features in R and increases both ice indices.

Technical Corrections

Thanks to the reviewer for listing these technical errors. These are corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13801, 2009.