Atmos. Chem. Phys. Discuss., 9, C5687–C5699, 2009
www.atmos-chem-phys-discuss.net/9/C5687/2009/

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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Received and published: 7 October 2009

The comments of the reviewer have been helpful to improve the manuscript. Especially the discussion on possible other vertical distributions of ice crystals did help to revise the argumentation of how to interpret our measurements. The think that the now included changes do make it easier for the reader to understand the key issues of the manuscript. 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

As mentioned in the previous paragraph, I don't believe the finding of the presence of ice up to cloud top to be a new one. Numerous recent papers discuss similar features ...

It is true that these papers clearly show the existence of ice crystals up to cloud top. As the papers show the ice concentrations decrease towards cloud top. That's why we have focus in our original manuscript on the uppermost cloud layer (< 50 m from cloud top) and did argue that this might be a new finding. Anyway, thanks to the review comments (see below), we found that the ice is not necessarily situated in the uppermost layer. Rather the reflectance measurements indicate that the ice is situated in the altitude where the measurements are most sensitive due to ice absorption. That is at altitudes about 25–75 m from cloud top.

For this reason we revised the statements in the manuscript in several parts and highlighted the agreement with recent literature. Although these findings are not new, we think that this was the first time this vertical structure of mixed-phase clouds was retrieved by remote sensing using spectral solar radiation. That is why we did not change the title of the manuscript which includes the restriction to airborne remote sensing technology.

In the introduction we added following sentences.

> From ground-based remote-sensing instruments it could be shown that even though the cloud top of ABM clouds is dominated by liquid water, ice crystals exist throughout the clouds with a maximum in lower cloud layers (Shupe et al., 2006, 2008a,b; de Boer et al., 2009). This

> was confirmed by McFarquhar et al. (2007) who investigated the vertical distribution of ice crystals by in situ measurements.

What worries me is that of these examples, two (McFarquhar and Shupe) are used in the conclusions section of the manuscript (p. 13822, lines 14-15) as examples of the "common vertical structure of ABM clouds" that this paper is trying to improve upon ...

With regard to the revised interpretation of our results, we removed this statement in our conclusions and amplify the agreement with recent publications by adding following sentences to the conclusion.

> However, similar clouds have been investigated by McFarquhar et al. (2007); Shupe et al. (2006, 2008a) who observed ice crystals throughout the entire clouds by in situ and ground-based measurements. With the airborne remote-sensing techniques presented in this study, these findings could be confirmed by a third independent method.

Since some of the work is justified by, and involves the use of in-situ ice measurements, I think it's relevant to include some discussion on the potential sources of error from these sensors. For example, the effects of shattering, icing (while within the super-cooled layer), etc.

The description of the in situ probes and their potential errors was kept short in the manuscript as there is a connected paper in the same special issue already published (Gayet et al., 2009), which discusses in more detail the in situ measurements obtained in mixed-phase clouds during the ASTAR 2007 campaign. We do not want to repeat this discussion in this manuscript, but now more clearly refer to Gayet et al. (2009). A short discussion on shattering

effects is included as well:

> A detailed discussion on potential sources of error of in situ measurements in mixed-phase conditions is given by Gayet et al. (2009) for observations similar those presented here. By comparison of Polar Nephelometer measurements with scattering phase functions calculated from FSSP-100 cloud droplet size distributions it was found that shattering of ice crystals is low in liquid-dominated cloud layers due to low concentrations of large ice crystals ($D > 100\,\mu$m). In ice-dominated layers with higher concentrations of large ice crystals the measured LWC could likely be overestimated due to shattering. Subsequently, the above given values of f_I and f_I^* might be biased, which will be addressed in Section 4.2.

The use of the "ice volume fraction (IWP/TWP)" on page 13808 confuses me a bit. This does not really provide any useful information on the vertical distribution of ice and liquid, and does not provide very much information in terms of how the two phases interact. I would think it would make more sense to compare the IWC/TWC fraction throughout the cloud layer. This is still a "volume" estimate, and more clearly defines the extent of the volume used. In addition, it provides vertically resolved information.

Sure we agree, that the use of ice volume fraction f_I defined by (IWP/TWP) gives no vertical information. That was not our purpose. The intention to use IWP and TWP instead of IWC and TWC was to derive an ice fraction of the entire cloud similar to the ice optical fraction f_I^*. This is necessary for the sensitivity study in which f_I^* is varied. Due to the different vertical extends of the ice and liquid water layer in the modeled cloud the use of IWC and TWC would have been less useful to compare the entire clouds. Anyway, to provide also the vertical information of ice fraction we added the values of IWC, LWC and TWC in the tables of the revised manuscript.

Although the authors state that the selection of particle shape does not have large impacts on the results of the simulations, it would be nice to include a quantitative analysis of this. Particularly, since particle effective size is a predetermined value, and particle growth rate and fall speed are both strongly related to particle shape, I would think that by default the particle shape would significantly alter the vertical distribution of particle effective size, resulting in changes in the simulated optical properties. Are there any measurements/observations that provide information on particle shape (CPI for example?)

CPI identified most ice crystals in the clouds as irregulars. This does not perfectly fit to our assumption of column shaped ice crystals, but has no major impact on the results of the radiative transfer simulations as discussed in the revised version in more detail. We extended the discussion on the ice crystal shape in the revised manuscript (Section 4) by summarizing the results of Ehrlich et al. (2008) who investigated the impact of ice crystal shape on the radiative transfer in mixed-phase clouds.

> Columns do not perfectly suit the CPI measurements which mostly identified irregular crystals in the clouds. However, for the simulations presented here the choice of particle shape is less important than the ice crystal effective diameter. The impact of ice crystal shape on the radiative transfer in mixed-phase clouds has been evaluated by Ehrlich et al. (2008). They found that liquid water droplets dominate the cloud optical properties. Ice crystal shape effects are significant only for the presence of small ice crystals, $f_{\mathrm{I}} > 0.5$ and if the IWC is kept constant when changing the ice crystal shape. For ice crystals of a size similar to that observed in this study ($D^{\mathrm{I}}_{\mathrm{eff}}$=103 μm), shape effects are lower than

> 1% for cloud reflectivity and transmittivity. For cloud absorptivity which is relevant in this study the simulations by Ehrlich et al. (2008) showed no measurable shape effect at all.

I'm not an expert in spectral reflectance measurements, and I believe that it would be nice to have further discussion on how particle sizes and number concentrations may effect the simulated optical properties

Thanks for this hint. An understanding of the impact of cloud particle concentration (optical thickness) and particle size (effective diameter) and the cloud reflectance is inevitable to understand the study. As this seems not clear enough for all potential readers, we added following sentences to the description of the cloud reflectance measurements (Section 3.2).

> The properties of cloud particles like concentration and effective diameter strongly alter the cloud-top reflectivity. In general a higher particle concentration is linked to a higher cloud optical thickness and increases R almost independently of wavelength. The particle effective diameter is an indirect measure of the fraction of radiation which is absorbed by clouds. The larger the particles, the higher is the absorbed fraction and the lower is R.

In particular, the assumption that the layer from 1200-1600 m is liquid only (p. 13810, lines 22,23) does not totally seem to agree with the measurements. Yes, in an absolute sense, there is far more liquid mass than C3986 ice mass, but there is ice mass present, and depending on the instrument used (lidar vs. radar for example), you are going to be sensitive to one or the other because of the wavelength used. Further discussion on the relative sensitivity of the

SMARTAlbedometer to the concentration and mass of the different particles would help to justify the assumption that the top of the cloud is liquid only.

The assumption to use a liquid only cloud top layer in the simulations results from the in situ measurements which did not measure noticeable amounts of ice in this part of the cloud. Between 1200–1600 m altitude CPI gives an optical thickness of ice crystals of about 0.03 ($IWC < 2\,\mathrm{mg\,l^{-1}}$). This is about a factor 20 less than measured in the ice dominated layer below. And this is also about a factor 20 less than the ice crystals added in simulated cases C–F. Such a low optical thickness is in the range of low aerosol concentration and changes the cloud reflectance by less than 0.2 %. This is far below the uncertainty range of the SMART-Albedometer and thus not detectable. We add following discussion in the revised manuscript (Section 4.1)

> The assumption of a pure liquid upper cloud layer is justified by the optical thickness of the ice crystal population measured by the CPI which was less than 0.03. Simulations not shown here reveal that this low ice concentration alters the cloud reflectivity by less than 0.2 % which is far below the measurement uncertainties. Thus ice crystals in the upper cloud layer are neglected in the following simulations.

I'm not sure that I believe that large ice crystals are present in the upper portions of the cloud (p. 13816, line 22). This seems to not only counter radar observations, which show decreasing dBZ as you approach cloud top, but also physics. What keeps these large ice crystals from quickly falling to lower altitudes within the cloud? How do they stay around cloud top and not demonstrate a presence at lower altitudes? I think that these are very important considerations that need to be addressed before making a claim such as this. Discussion on other factors that may lead to the albedometer measurements would be helpful

as well.

Considering the revised conclusions that the ice crystals occur in the altitude of maximum sensitivity of the SMART-Albedometer to ice absorption (1550 m) and not necessarily directly at cloud top, the large size of the ice crystals ($D^{\mathrm{I}}_{\mathrm{eff}}$=103 μm) is in agreement with measurements presented by McFarquhar et al. (2007). They showed that ice crystal effective diameter increases up to about 85 μm in altitudes close to cloud top, which is in the range we concluded here from the albedometer measurements.

The impact of particle size to the albedometer measurements is straightforward and thus we think the derived particle effective diameter reliable. Other factors leading to the spectral pattern of the cloud reflectance could not be found. In the revised manuscript we added a short description on how the reflectance is altered by cloud particle properties (see comment above). Here is a short short repetition in the context of large particles:

The size of the ice crystals define the amount of radiation absorbed by the crystals (wavelengths around 1510 nm). The larger the particles, the higher the absorption and the lower the cloud reflectance. The measurements showed a relative low reflectance (with low uncertainty) in this wavelength range and suggest large particles. Decreasing the ice crystal effective diameter in the simulations will reduce the absorption, consequently increase the number of scattering processes and enhance the cloud reflectance in the wavelength dominated by ice absorption. This does not suit the measurements.

The simulations with the additional ice near cloud top are interesting. The fact that placing the layer within the liquid or above it result in very similar solutions makes me wonder what would happen if the ice layer was distributed more evenly throughout the entire liquid layer. . .in other words, could different ver-

tical distributions of ice concentration and size (maybe some that more closely match in-situ and remote sensing observations) possibly result in similar cloud top reflectances that match observations?

Thanks for this comment. This helped us to more clearly interpret the vertical weighting functions (VWF) of the reflectance measurements we present in our study. As already mentioned above the ice crystals do not necessarily have to be placed at top of the cloud to match the simulations to the measurements. The only important factor is that the ice crystals are situated in the altitude where the measurements are most sensitive due to ice absorption. Our calculation of the vertical weighting functions showed that this is at altitudes about 25–75 m from cloud top.

In the revised manuscript we added two simulations with clouds of different geometry. In Case C the additional ice crystals are distributed between 1300–1600 m altitude. In Case D we place a thin ice layer between 1525–1575 m where the VWFs their self and the difference between the two VWFs is largest. The results of Case D are similar to Cases E and F (Case C and D in the original manuscript). Case C shows little deviations with less ice absorption simulated than observed. Thus the strict constrain to ice crystals at cloud top stated in the original manuscript was removed.

With regard to other parameters ice concentration, particle size,... we performed a number of other simulations 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 vary-

ing the ice fraction. In both cases the ice absorption is increased 'wavelengths independent' which means the change follows the refractive index 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.

What are the errors or possible nuances of using the polar nephelometer measurement? Could it be skewed towards liquid in regions where liquid mass dominates?

The polar nephelometer basically measures the scattering phase function of the particle population present in the sampling volume. The sampling volume is about 500 cm^3. The wavelength used is 804 nm for which absorption by cloud particles is negligible. In general, the optical properties derived by the polar nephelometer can be calculated from the single scattering properties of the individual particles inside the sampling volume using the standard mixing equation for volume scattering properties (*e.g.*, Wendisch et al., 2005). This implies that the contribution of liquid droplets and ice crystals to the measured scattering phase function (or asymmetry parameter g) depends on particle concentration and size. In this regard small liquid water droplets of high concentration compared to larger ice crystals with lower concentration will dominate the volumetric signal. But this does not mean that the polar nephelometer is skewed towards liquid droplets. The measurement should be considered as a property related to radiative transfer. Thus a asymmetry parameter derived by the polar nephelometer indicating liquid water droplets shows from a radiative transfer point of view that the sampling volume and all scattering processes are dominated by liquid water droplets. The distribution of liquid and ice mass might look different with a higher contribution of ice mass. That is why we always refer to two different ice fractions, ice volume fraction based on mass and ice optical fraction based on radiative transfer.

On line 4 of page 13822, a mention of "homogeneous mixed clouds" is made. What is meant by this? This requires clarification. By nature, the liquid water content will be higher at the top of the cloud. Do you mean that the ice water content increases equally? This seems unphysical. Or, do you mean that the ice stays the same throughout the cloud layer? Again, this does not seem likely. Please clarify this statement!

With 'homogeneous mixed clouds' we mean in this case clouds without horizontal and vertical variability of ice and liquid water content. This assumption is made by most radiative transfer simulations applied for retrieval algorithms using remote sensing. Sure this is far from reality, but it is common in order to reduce amount and complexity of such forward simulations. That is why we state that considering the vertical distribution of cloud particles especially of ice crystals will change the results of such simulations.

For clarification that 'homogeneous mixed clouds' is an assumption only, we changed the sentences in the following way:

> These findings implicate that the presence of ice crystals within the cloud top layer alter the radiative properties of ABM clouds compared to values derived with the assumption of homogeneously mixed clouds.

Also in this paragraph, the effects on the solar radiative cooling are discussed. How are these thought to compare to the longwave radiative effects? Are they important?

Longwave radiative cloud forcing is not significantly altered by particle phase or shape. Clouds investigated here with optical thickness larger 10 act approximately as black body and absorb all longwave radiation. Thus only the cloud temperature is determining the emitted longwave radiation and the longwave radiative cloud forcing. We added following sentence to the discussion:

> As the terrestrial infrared radiative forcing is determined by cloud temperature only and is almost unaffected by the vertical distribution of ice crystals, changes in the solar forcing propagate directly into the net (sum of solar and terrestrial) cloud forcing.

I found the discussion of simulation of different nucleation processes somewhat lacking. It appears to focus on only evaporation freezing, while there are other nucleation processes that could lead to ice formation at cloud top. For example, condensation and immersion freezing (see: Khvorostyanov and Curry, 2004; Diehl and Wurzler, 2004; de Boer, 2009) would also lead to ice formation in the regions of strongest supersaturation. Also, it is mentioned that simulations neglecting evaporation freezing show ice crystals to be dominant at lower cloud layers only. . . Isn't that what the observations show? Ice dominating at lower levels, with smaller amounts of ice extending to cloud top?

Here we intended to focus on nucleation processes causing the presence of ice crystals directly located at cloud top. As the conclusion of the revised manuscript changed stating that ice crystals are not necessarily situated direct to cloud top but in the upper cloud layers, this discussion is unjustified and therefore omitted.

Technical Corrections

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

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