Reply to Referee #1 (anonymous)

In comparison to the first Reply, we re-considered the terminology concerning haze and droplets.

We would like to thank the anonymous referee for the valuable comments which helped to identify weak points in the manuscript. We took into account the major and minor suggestions. Our results are discussed more in detail in the context of other articles. The terminology is now used and explained in a better way, and the text is easier to read as sentences were rephrased. The detailed replies to the reviewer’s comments in quotation marks are given below.

Major Comments

1) Conclusions

"A major conclusion of the paper appears to be that lidars are good for observing clouds. This is already well known. The case studies are interesting enough that other conclusions might be drawn. More attention to interpretation of the measurements in the context of earlier studies would make this a stronger paper."

We completely agree with the referee. The obvious conclusion that lidars are suitable for obtaining cloud properties has been removed. The focus of our conclusions is now related to the case studies. The observations were compared to earlier studies. New references have been added in the motivation, the discussion of the general cloud cover and cloud cases A and B:

Boers, R., Spinhirne, J.D., and Hart, W.D.: Lidar Observations of the Fine-Scale Variability of Marine Stratocumulus Clouds, J. Appl. Meteorol., 27, 797-810, 1988.

Fitzgerald, J.W: Approximation formulas for the equilibrium size of an aerosol particle as a function of its dry size and composition and the ambient relative humidity, J. Appl. Meteorol., 14, 6, 1044-1049, 1975.

Kay, J.E., L'Ecuyer, T., Gettelman, A., Stephens, G., and O'Dell, C.: The contribution of cloud and radiation anomalies to the 2007 Arctic sea ice extent, Geophys. Res. Lett., 35, L08503, doi:10.1029/2008GL033451, 2008.

Shaw, G. E.: Aerosol Chemical Components in Alaska Air Masses 2. Sea Salt and Marine Product, J. Geophys. Res., 96, D12, 22,369-22,372, 1991.

Wandinger, U, Müller, D, Böckmann, C, Althausen, D, Matthias, V, Bösenberg, J, Weiss, V, Fiebig, M, Wendisch, M, Stohl, A, and Ansmann, A: Optical and microphysical characterization of biomass-burning and industrial-pollution aerosols from multiwavelength lidar and aircraft measurements, J. Geophys. Res.,. D, Atmospheres, 107, D21, 2002.

Wyser, K., and Jones, C.G.: Modeled and observed clouds during Surface Heat Budget of the Arctic Ocean (SHEBA), J. Geophys. Res., 110, D09207, doi:10.1029/2004JD004751, 2005.

2) Terminology

a) optical density

"There are statements in a few places that “optically thick” clouds were observed. The fact that the lidar can see right through these clouds means that they are not optically thick, by definition. For Case B, the cloud optical thicknesses are said to be in the range of 11-17. However, the lidar sees right through this cloud, which is not possible for optical depths greater than 2 or 3 (depending on integration times)."

The referee is right - the lidar penetration of this cloud needs more explanation. We added in the text:

Assuming pure water clouds, the maximum cloud optical thickness estimated from albedometer data shows values around 13-17 for the more homogeneous cloud deck in the South. In the mixing zone starting at 09:00 UTC, the maximum optical thickness was lower (11-13 assuming pure ice). Despite this high maximum optical thickness, the lidar penetrated the clouds for most time steps due to cloud inhomogeneities and the long integration time of 15 s. For a shorter integration time of 1 s, about every 15th lidar profile reached the ground. This corresponds to "cloud gaps" with a distance of about 1 km. Similar variability of marine stratocumulus clouds with a scale of 1-5 km was reported by Boers et al. (1988).

b) pre-condensation / haze

"Pre-condensed versus haze: The measurements presented in Case A are interpreted to be evidence of “pre-condensed liquid droplets” rather than haze aerosols. However, unless this is a case of homogeneous nucleation (very unlikely), I cannot see the difference between aerosol-nucleated water droplets and aerosol haze. While it is appropriate to distinguish between hydrated and dry aerosols, I don’t think the distinction made in the paper is meaningful."

As stated in the manuscript, the very low depolarization values indicate the existence of spherical particles. In contrast, the typical Arctic haze observations have revealed a significantly higher depolarization of 2-5 %, as e.g. described in Hoffmann et al. (2009). For this reason, our first guess was that we observed liquid cloud droplets. However, the retrieved particle size is clearly below the characteristic size of cloud droplets (diameter around 10 micron). Further, the typical Arctic haze is usually observed within dry air (Ishii et al., 1999). Therefore, the high relative humidity measured by the radiosonde suggests a different situation. Our best explanation is that we observed spherical haze droplets. We exchanged the misleading expression "pre-condensation particles" against the more neutral "spherical haze droplets". Further, we added in the text observations of in situ measurements on the nearby Zeppelin mountain of that day, which also showed low values of aerosol, and an overview picture of the MPL lidar which shows the slow dissolution of the layer.

c) mixed-phase

"The term “mixed-phase clouds” in the Arctic usually refers to water clouds aloft with ice precipitation below. For Case B, the term is being used to describe a water cloud that later glaciates at the same altitude. It would be best to explain your use of the term to avoid confusion."

We added as explanation of the mixed-phase clouds in the introduction:

> Mixed-phase clouds are composed of both liquid water droplets and ice crystals. The characteristic vertical structure consists of an upper layer dominated by liquid water droplets and a layer of ice crystals below (Pinto, 1998, Shupe et al., 2008, Gayet et al., 2009 and Ehrlich et al., 2009, this issue).

At the intersection of two different air masses, local glaciation within the system of mixed-phase clouds was observed.

d) cirrus

"In Case C, clouds in the lower troposphere are referred to as “cirrus”. This
term is usually reserved for ice clouds in cold air near the tropopause. A more standard
term should be used to describe the clouds in Case C."

We now use the more general term "ice cloud" instead of "cirrus".

3) Measurement Techniques

a) Klett inversion

"The description of the Micro Pulser Lidar (Sec. 2.1) indicates that the Klett Inversion is used to obtain the backscatter ratio. However, that is not what the Klett Inversion does. It is used to obtain extinction coefficients or backscatter cross-sections, with an assumed lidar ratio (extinction to backscatter ratio). Perhaps the backscatter cross-section obtained using the Klett Inversion is used to determine backscatter ratios? If so, I would recommend using the more geophysically-relevant cross-sections the Klett Inversion provides. In any event, some clarification is needed."

We apologize for this error - of course the Klett Inversion provides the backscatter coefficients, which are then used to derive the backscatter ratio. We prefer the use of the backscatter ratio as it constitutes a number easy to understand also for non-lidar experts. We corrected the Klett inversion and added typical values for the backscatter ratio in Section 2.1:

> From the obtained profiles the total backscatter coefficient $\beta = \beta^{Ray} + \beta^{part}$ was calculated with the Klett algorithm (Klett, 1985). β^{Ray} and β^{part} are the molecular Rayleigh and the particle backscatter coefficients, respectively. The backscattering ratio (BSR) for a given wavelength λ at range z, defined as
>
> $$BSR(\lambda, z) = \frac{\beta^{Ray}(\lambda, z) + \beta^{part}(\lambda, z)}{\beta^{Ray}(\lambda, z)} , \qquad (1)$$
>
> was introduced to obtain values which can easily be compared to the clean atmosphere. In the case of pure Rayleigh scattering caused by the nitrogen and oxygen molecules of the air, the BSR has a value of 1. Typical values for enhanced aerosol load are around 2, for optically thin clouds up to around 10.

b) backscatter ratio

"Use of backscatter ratio: In Figures 5 and 7, the backscatter ratio is used to profile the clouds. Are these true backscatter ratios, or the "attenuated" backscatter ratios that lidars normally provide? For attenuated backscatter ratios, the manuscript should point out that non-zero values below the cloud may not be evidence of particles there, but are elevated due to extinction in the cloud. For this reason, use of extinction profiles would have been a better choice."

From the backscatter coefficients obtained by the Klett Inversion, we calculated the backscatter ratios. The values are not the same as the attenuated backscatter ratios. We pointed out in the caption of Fig. 5 that values behind the cloud are not reliable. We chose the representation of the backscatter profile instead of the extinction profile as the extinction coefficient is subject to higher errors introduced by the assumption of the lidar ratio.

c) multiple-wavelength inversion

i) validation of KARL retrieval

"For KARL, a multi-wavelength inversion scheme has been used to obtain particle size distributions, lidar ratios, and refractive indices. I have always been a little suspicious of the technique, given that the approach is not well-validated and will give results whether or not the input data is correct. In particular, I imagine that the mix of detector and signal acquisition techniques between the visible/UV and infrared measurements is very hard to deal with. Have you performed any validation of the KARL retrievals? Are there validation studies that demonstrate the technique actually works? For Case A, you could compare with the sun photometer which usually outputs these same parameters as standard data products (for AERONET, anyway). If the data are available, perhaps the aircraft measurements described in the paper could be used. In any event, references to where in the literature such comparisons can be found are needed."

Indeed, the retrieval of microphysical properties from remote sensing data is an ill-posed problem. Hence, small variations in the input data can greatly influence the result (but as the referee states a result is [almost] always obtained). The mathematical concept in finding a stable solution is called regularisation.

The theory was carried out by Böckmann (2001) and Veselovskii et al. (2002). A validation was for example given by Wandinger et al. (2002). This quote was added in the manuscript as it demonstrates that a successful inversion from lidar data can be done. Our code uses an improved version of Böckmann (2001). The mathematical approach for a more precise determination of the aerosol number concentration can be found at Böckmann et al. (2006). This information was emitted for brevity as the main idea remains unchanged. We suggest not to quote the latter paper in our manuscript as it is strictly mathematical and not mandatory for the general idea.

Extensive tests were performed to validate our code. Extinction and backscatter coefficients were calculated "forwards" from an arbitrary aerosol distribution, noise was added to the data and the inversion's ability to retrieve the aerosol distribution was analyzed. Moreover, for this work, several inversion runs with lidar data from different altitudes and times were performed from which the given error estimation was derived. So we are confident about our results.

Due to the additional cloud layers shown in Fig. 2, we did not perform a comparison with sun photometer data, which contain values integrated over the whole atmosphere.

ii) sensitivity of lidar inversion to large particles

"In Sec 3.2.2 (Analysis of Case A), it says "... the backscatter and extinction coefficients are clearly decreasing with wavelengths indicating that the main part of the particles was smaller than 1.25 [microns]." However, decreasing extinction and backscatter occurs even in the Mie regime, where the scattering efficiency is roughly constant. Given that the inversion technique requires there to be no large particles, this unsupported assumption poses a serious problem. What is the sensitivity of the analysis technique to large particles?"

The Mie efficiencies both for backscattering and extinction get smooth for large size parameters (> 40). This means that it becomes very difficult to provide information about large particles. If large particles had been present here, the code would simply have overlooked them. In the case presented here, the Angström coefficients for backscatter and extinction are around -1.2 which is typical for aerosol in the accumulation mode. If the scattering particles were overwhelmingly in the supermicron range we would have expected Angström exponents closer to zero.

d) depolarization

"Depolarization: Is the depolarization provided for particles only, or is it particles+ molecules? There is a substantial difference when the molecular contribution is not subtracted. I also wonder if the laser beam is tilted to avoid specular reflections from horizontally-aligned platelets? Finally, it should be specified as "linear depolarization ratio" to differentiate it from the circular depolarization measured by some lidars."

To clarify this point, we used the term "linear volume depolarization" in Sect. 2.3, indicating the total linear depolarization of particles and molecules. We further mentioned that the molecular depolarization alone provides values about 1.4 %. The laser beams of KARL and airborne AMALi are not tilted intentionally.

e) lidar ratio

"Measurements of the lidar ratio are given in several places, but there is no description of how these were measured."

We added the following information in Sect. 2.2.:

> To determine the lidar ratio, we proceeded as follows: After the calculation of the layer integrated optical depth, the particle backscatter coefficient was calculated via the density profile from the radiosonde according to Ansmann et al. (1992). The lidar ratio of the cloud finally is the layer integrated optical depth divided by the layer integrated particle backscatter coefficient. Hence it constitutes an average value for the whole cloud.

4) multiple scattering

"There is repeated reference to multiple scattering effects in the paper. This is surprising given that the clouds observed were, in essence, optically thin. I wouldn't expect multiple scattering to be a factor in these measurements at all. As you are aware, multiple scattering can be observed using multiple fields-ofview. You have referred to the work of Luc Bissonnette, who used a lidar specially designed to rotate through different fields-of-view many times per second, which is needed because of rapid changes in the cloud itself. The instruments used in this study are not designed to perform such observations. The attribution to multiple scattering is instead frequently made on the basis of variations in depolarization with altitude. However, there are very real variations in depolarization in clouds that aren't at all related to multiple scattering. In Section 4, "an afterglow effect behind the cloud" is also attributed to multiple scattering. I have never heard of such a thing from lidars before, and suggest that this may be due instead to photomultiplier tube ringing from over-exposure (highlighted elsewhere in the manuscript as a problem)."

As proposed by the referee, we removed the misleading discussion of multiple scattering.

6) Introduction

radiative impact of clouds

"There is a discussion of the radiative impact of clouds, and a reference to the paper of Ehrlich (2009) claiming a surface cooling of -160 W/mˆ2. This result seems pretty extraordinary, since a net surface warming from clouds would normally be expected at this time of year. I would be hesitant to refer to that result until it has been reviewed. In any event, the conditions under which such an unusual result was possible should be described."

The solar surface cooling of -160 W/m^2 mentioned in the introduction is a local and temporary estimate of the radiative forcing of low level stratus clouds. It was calculated for a solar zenith angle of 71°, not averaged over a whole day, and for a low surface albedo (open ocean with albedo of about 0.1). In this situation the cloud forcing indeed is a strong cooling. The clouds reflect a large amount of the solar radiation (cloud albedo of about 0.7) and the longwave infrared warming is weak because the cloud layer is situated in the boundary layer where the temperature difference between surface and cloud is low.
In the revised manuscript we pointed out more clearly that this estimate of cloud forcing corresponds to a single situation and cannot be used as a value for climate modeling.

Arctic haze, diamond dust, blowing snow

"Also, there is a lot of background on mixed-phase clouds, but none on Arctic haze, diamond dust or blowing snow. It seems to me that these are relevant to the discussion, particularly Case A which deals with boundary-layer observations."

We added a short paragraph about these phenomena in the introduction:

The Arctic troposphere is also subject to other phenomena such as Arctic haze (Quinn et al., 2007), diamond dust (Intrieri and Shupe, 2004), and blowing snow (which is confined to the lowest meters above ground). Arctic haze consists of anthropogenic aerosol transported into the Arctic region in spring time from polluting sources at southern latitudes. It can reach high optical depth values up to 0.3 at 532 nm wavelength (Herber et al., 2002) and thus significantly influences the radiation budget (e.g. Blanchet and List, 1983, Rinke et al., 2004, Quinn et al., 2007, and references therein). According to the IPCC report (IPCC 2007), it is still difficult to quantify precisely the radiative forcing of aerosol in the Arctic. Diamond dust is the ice precipitation out of "cloudless" sky, and is often observed in winter time. It is caused by optically subvisible clouds and was found to have a negligible radiative effect (Intrieri and Shupe, 2004).

Minor Comments

1) HYSPLIT analysis

"The trajectory analysis described on page 15141 is not very convincing. A more sophisticated model is needed to assess the impact of precipitation on the transported aerosol burden."

We agree with the Referee that the analysis of precipitation is not very convincing. However, our main point of using the trajectory analysis was to obtain information about the path of the air masses and the possible uptake of pollution. We reduced the information concerning precipitation as following:

> HYSPLIT analyses suggest that the probed air masses were confined to the boundary layer until 2 days before their arrival with only minimal precipitation (less than 1 mm). They reached the Siberian coast 6 days before the observation. Hence, a contamination with aerosol from the open sea or Eurasia cannot be ruled out.

2) remove case D

"I don’t think there is very much of interest in this Case. I recommend removing it."

We would prefer to keep case D in the manuscript. First, it represents a cloud structure in the free troposphere which has rarely be reported in the literature. Second, we consider the finding that local orography may influence cloud structures at these altitudes interesting in itself.

3) figure 1

"The top panel isn’t very useful, and I recommend eliminating it. The bottom panels should have the height intervals labeled rather than numbered, and the measurement dates printed on the plot."

We changed the figure according to the reviewer's suggestions.

4) one-sentence paragraphs

"There are several one-sentence paragraphs, and these should be expanded, joined to the surrounding paragraphs, or removed."

The one-sentence paragraphs have been joined to the other paragraphs or removed.

5) complicated paragraphs

"AMALi description (Sec 2.3): The first paragraph is very hard to read. It needs to be broken up into multiple paragraphs and clarified. Similarly, the first paragraph in Sec 3.5.1 (Case D) should be broken up into multiple paragraphs and clarified."

The mentioned paragraphs have been broken up and clarified. Section 2.3 has been changed to the following:

> The Airborne Mobile Aerosol Lidar (AMALi) is an airborne backscatter lidar system operating at two wavelengths (532 nm and 355 nm). Additionally, it measures the linear volume depolarization of molecules and particles at 532 nm. AMALi has been developed and operated by the Alfred Wegener Institute for Polar and Marine Research (Stachlewska et al., 2004, Lampert et al., 2009, Stachlewska et al., 2009, this issue).

Section 3.5.1 has been changed to:

> On 14 April 2007, the Polar-2 aircraft went from Longyearbyen towards the South along the West coast of Svalbard, in the direction of an approaching high pressure system. A two-layer cloud structure was observed by the zenith pointing AMALi (case D). The system had a horizontal extent of around 30 km (8 flight minutes from 16:18 to 16:26 UTC).

6) MPL cloud detection:

"The paragraph beginning with “Using different thresholds...” is really unclear."

The paragraph has been re-phrased:

> The algorithm for cloud detection compares the BSR values of adjacent height intervals. Different thresholds for BSR were used in order to categorise the clouds. E.g. the difference of adjacent BSR values had to show high values above 0.1 increasing for at least 3 height steps or a single peak difference of minimal 0.2 to 0.3 if no lower clouds were detected. The BSR was analyzed for cloud peak structures in five distinct altitude intervals: 0-300 m (snow on the window), 300-1200 m (boundary layer clouds), 1200-2500 m (low clouds), 2500-5500 m (midlevel clouds) and 5500-10000 m (high clouds). If none of these were detected the profile was set to ‘cloud free’.

7) Spelling Errors

The mentioned spelling errors were corrected.

References used in the answer to Referee #1:

Böckmann, C., Kirsche, A., and Ritter, C.: Methods for the retrieval of microphysical aerosol parameters from optical data, http://www.gi.alaska.edu/ftp/foch/ILRC23_Proc/ILRC23/2P-60.pdf, ILRC 23, 2006.

IPCC, Intergovernmental Panel on Climate Change: Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, Z.M., Averyt, K.B., Tignor M., and Miller, H.L. (eds.), Cambridge University Press, 996 pp., 2007.

Veselovskii, I., A. Kolgotin, A.,V. Griaznov, V., D. Müller, D., U. Wandinger, U., and D. N. Whiteman, D.N.: Inversion With Regularization for the Retrieval of Tropospheric Aerosol Parameters From Multiwavelength Lidar Sounding, Appl. Opt., 41, 3685-3699, 2002.

Wandinger, U, Müller, D, Böckmann, C, Althausen, D, Matthias, V, Bösenberg, J, Weiss, V, Fiebig, M, Wendisch, M, Stohl, A, and Ansmann, A: Optical and microphysical characterization of biomass-burning and industrial-pollution aerosols from multiwavelength lidar and aircraft measurements, J. Geophys. Res.,. D, Atmospheres, 107, D21, 2002.