Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20229, 2009.
On retrieval of lidar extinction profiles using Two-Stream and Raman techniques.

Response to Anonymous Referee #2

by Stachlewska and Ritter 2010-02-08

We would like to thank the Anonymous Referee # 2 for his valuable comments which certainly helped us to improve the final version of the manuscript. In the following we give explanations to the issues raised.

Referee: This paper compares range-resolved extinction and backscatter coefficients derived using a 'two stream' inversion to the same quantities derived using the well established Raman technique. The two stream method is applied to elastic backscatter lidar measurements made by a nadirpointing airborne lidar (AMALi) and a zenith-pointing ground-based system (KARL). Because KARL is equipped with a Raman detection channel, KARL also provides the measurements used in the Raman retrievals. The idea of validating the two stream retrieval methods with simultaneous Raman measurements is a good one, and one that has not, to my knowledge, been previously presented in the peerreviewed literature. The authors do a good job of explaining the mechanics of the two stream technique, and (equally important) of discussing its applicability and limitations. However, there are several substantial obstacles that should prevent the paper from being published in its current form. Below I provide lists of both general and specific remarks that I hope the authors might consider in crafting a revised manuscript.

General comments:

Referee: The paper contains numerous spelling and typographical errors; see for example pages 20231 ('commonly' on line 5, 'usually' on 20, 'severely' on line 21, and 'rarely' on line 23) and 20235 ('coefficient' on line 4, 'mathematically' on line 20, 'severely' on line 21, 'subtracted' on line 27, and 'subtracted' on line 28). An English language spell checking utility should be sufficient to repair most of these mistakes. However, there are also many other types of errors that cannot be cured by a spell checker alone (e.g., verb tense, subject-verb agreement, and missing articles). I encourage the authors to have a native English speaker thoroughly review their paper before the resubmit it.

Authors: We asked a native English speaker to review our paper and we hope that his review will satisfy the Referee.

Referee: The KARL calibration procedures seem unnecessarily complicated and/or are not clearly explained. The relevance of the sun photometer measurements described on page 20236 is not readily apparent without reviewing the material in Appendix C. I would suggest moving parts of Appendix C into the main body of the paper.

Authors: We shifted the whole Appendix C into the main body of the paper as a separate sub-section. We did not find a good way to cut this appendix into parts without losing the clarity.

Referee: Furthermore, with regards to the discussion of additional calibration constraints provided in Appendix C:

• Presumably the KARL data were always cloud-free above 10-km. In that case, the accuracy of C_K would depend on the accuracy of the sun photometer measurement τ_{sun}^{part}, the fraction of the total column optical depth that lies above 10-km, the aerosol scattering ratio in the calibration region, and the fidelity of the molecular model used. I do not understand the additional requirement that τ_{KFS}^{part} lie within 10% of τ_{sun}^{part}. Achieving this degree of correspondence will be influenced by the selection of lidar ratio (as pointed out by the authors), assumptions about extinction in the KARL overlap region, and the SNR of the profile between the ground and 10-km. While all of these are important in retrieving an extinction profile using the KFS method, none of them are germane to the actual instrument calibration.

Authors: Yes, as our measurements were taken at an Arctic site at an altitude of 10km we are already in the stratosphere, where the range corrected lidar signals normally follow the density profile measured by a co-located radiosonde very accurately. Hence, for our limited data set a stratospheric particle extinction can be neglected and the Eq. C1 should hold true if the particle extinction in the lidar signal can be expressed by the sunphotometer's particle optical depth τ_{sun}^{part} measured nearby (for this reason we need the constrain τ_{KFS}^{part} should match τ_{sun}^{part} as close as possible). Knowledge of backscatter and extinction in the overlap region does not influence the determination of the lidar constant C_K from the (elastic) lidar signal in Eq.C1 as long as the sunphotometer's particle optical depth τ_{sun}^{part} is representative for the lidar beam. This is because we express the integral from zero to boundary condition altitude of the extinction in the lidar equation by the optical depth of the photometer - so we do not depend on the altitude where the extinction really takes place. As the τ_{sun}^{part} for 532nm in the Arctic for our data is in the range of 0.1 (even in the case of a subvisible cloud) an error of the particle optical depth of 0.02 would lead to an error of exp(-0.04) in the lidar constant. So, our conditions for this kind of lidar calibration are more favorable if compared to a heavily polluted site. The knowledge of a reasonably chosen β(h) will practically limit the precision for the retrieval of the lidar constant.

• The match between τ_{KFS}^{part} and τ_{TS}^{part} in the two-stream solution region will also depend on the lidar ratio chosen for the KFS solution. Why should the ability to guess the lidar ratio in this region have any influence on the computation of the calibration coefficient? The appropriate lidar ratio for the KFS solution should depend on the value of the calibration coefficient, not the other way around.

Authors: We demand that $\tau_{KFS}^{part} = \tau_{TS}^{part}$ initially and want to know β(h) in this two-stream solution. To make the backscatter coefficient profile calculated with the Two-stream technique independent from the backscatter coefficient profile calculated by the Raman technique the knowledge of the lidar constant C_{KARL} is needed. We estimate this lidar constant and its uncertainty from the elastic lidar equation with several Klett solutions, which match to all information we have. Now, in the region of altitudes where the Two-stream is applicable we know the extinction coefficient profile but the lidar ratio in this range depends on β_{KFS}, which depends on the assumptions above that Two-stream region.

• A lidar ratio of 12 sr for cirrus clouds seems unrealistically low. HSRL measurements reported by Eloranta et al. (2006) suggest that a value around 28 sr would be more appropriate for Arctic cirrus.

Authors: Indeed, the cirrus cloud show large spread in lidar ratios (Immler et al., 2008; Gayet et al. 2006, Sassen and Comstock, 2001; Chen, et al., 2002) which are, among others, related to their temperature and altitude of their occurrence, as these factors determine the form of the ice crystals in the cloud. Chen, et al., 2002 used the transmittance method (explained therein) to derive the Cirrus lidar ratio of 29sr ± 12sr and discussed their seasonal and physical variations over Taiwan. Reichardt et al., 2002 reported for a subarctic site the lidar ratios ranging between 7sr and 30sr at 355nm lidar wavelength. As the Referee mentioned the High Spectral Resolution Lidar measurements reported by Eloranta et al. , 2006 may suggest that a value around 28 sr would be more appropriate for Arctic cirrus. However, we do not intend to comment on general cirrus properties in the Arctic in our paper. For cases presented in this paper we used a lidar ratio of 12 sr if Cirrus cloud appeared in the 532nm signal, a value obtained according to the results of the transmittance method applied to data at our site in Ny Alesund. A higher lidar ratio would have led to a lower backscatter (even negative) below the cloud. But generally the Referee is right, not all Cirrus clouds over Ny Alesund can be described by 12 sr. The number given just holds true for the limited samples given here.
To clarify upon this issue we added the following: 'the altitudes where Cirrus or subvisible clouds were detected by KARL at about 9 km were treated with relatively low and uniform lidar ratio of B_{Ci}=12 sr, a value that was obtained by the transmittance method (described in Chen et al. 2002) applied to ~~a~~ the limited number of cases in this study.' Additionally, we removed here the quote of Ansmann at al.,1992, as it is misleading here (page 20249).

Immler, F., Treffeisen, R., Engelbart, D., Krüger, K., and Schrems, O. (2008), Cirrus, contrails, and ice supersaturated regions in high pressure systems at northern mid latitudes, Atmos. Chem. Phys., 8, 1689-1699.

Gayet, J. F., Shcherbakov, V., Mannstein, H., Minikin, A., Schumann, U., Str¨om, J., Petzold, A., Ovarlez, J., and Immler, F. (2006), Microphysical and optical properties of mid-latitude cirrus Cloud observed in the southern hemisphere during INCA, , Q. J. Roy. Meteor. Soc., 132, 2791–2748, doi:10.1256/qj.05.162.

Sassen, K., and J. M. Comstock (2001) A midlatitude cirrus cloud climatology from the facility for atmospheric remote sensing. Part III: Radiative properties, J. Atmos. Sci., 58 , 2113–2127.

Chen W., C. Chiang, and J. Nee (2002), Lidar Ratio and Depolarization Ratio for Cirrus Clouds," Appl. Opt. 41, 6470-6476.

Reichardt, J., S. Reichardt, A. Behrendt, and T. J. McGee (2002), Correlations among the optical properties of cirrus-cloud particles: Implications for spaceborne remote sensing, Geophys. Res. Lett., 29(14), 1668, doi:10.1029/2002GL014836.

Eloranta, E. W. , I. A. Razenkov, and J. P. Garcia (2006), Arctic Observations with the University of Wisconsin High Spectral Resolution Lidar, in Reviewed and Revised Papers Presented at the 23rd International Laser Radar Conference, C. Nagasawa and N. Sugimoto, Eds., pp. 399–402

• On page 20249, line 14: what does the flight altitude of AMALi have to do with the calibration of KARL? As it is necessary to calibrate the KARL data prior to retrieving the two-stream lidar ratio estimates, I wonder why the authors are only concerned with the specification of the lidar ratios above AMALi.

Authors: Please have in mind that the flight altitude does not necessarily take place in the region free of aerosol and, hence, it is difficult to estimate there the reference backscatter value. A reasonably well chosen boundary condition can be given for the groundbased system in the lower stratosphere where the clear sky condition holds with good precision. If the boundary condition of the groundbased lidar is fixed in the lower stratosphere, the value of the backscatter at the Two-stream range depends on the lidar ratio above the aircraft.

Referee: The authors state that “the applicability of the Two-Stream method depends critically on the constraint that both lidars probe into the same air masses”. What then is the RMS distance between the KARL site and the AMALi footprint for those profiles selected by the correlation test described on page 20235? Was the correlation coefficient a strong function of the distance between the two measurements?

Authors: The correlation coefficient was not really a very strong but a systematic function on position. The plane flew several (generally 3 legs over Ny Alesund along coastline / fjord. There always existed a fixed position in space where for the airborne system (with only weak time dependence to the groundbased system) the correlation was maximal. Hence, for our limited data set at a coastal site the place mattered stronger than the temporal evolution of the aerosol load. As our terrain is not homogeneous one cannot expect the best matching result for the Two-stream technique at the minimal distance between the footprints of the two lidars. An example of a correlation map can be found in Ritter et al., 2006. In the map’s structure systematically there are places (and times) where the data sets between AMALi and KARL match better or worse to each other. Hence, the correlation map and the comparability of the data do show some physical sense and not just casual fluctuations.

Ritter, C., Stachlewska, I. S., and Neuber, R. (2006), Application of the Two-Stream evaluation for a~case study of Arctic Haze over Spitsbergen, in: Proceedings of 23nd International Laser Radar Conference (ILRC 2006 in Nara, Japan), edited by: Nagasawa, C. and Sugimoto, N., 1, 507--510, ISBN 4-9902916-0-3.

Referee: In the Abstract and again in the Introduction, the authors assert their intention to investigate the feasibility of applying the two-stream method to CALIPSO data. It seems to me that understanding spatial correlations temporal variabilities will be critical to this effort, especially when considering the very poor SNR of the CALIPSO lidar.

Authors: Yes, this seems true. Meanwhile we rate both the SNR of CALIPSO and the possibility to employ a multiple scattering correction somehow skeptical as well. In any case for the Two-stream comparison it would be profitable to do it on a quasi-homogeneous terrain with as little topographical influence as possible (from a ship in a free ocean?) to allow more averaging for CALIPSO. On the other hand our site in Spitsbergen has the advantage of frequent overpasses of CALIPSO. Nevertheless, we plan work on this topic in the future.

Referee: The readers’ understanding of the source(s) of the disparities shown in Figure 1 would be improved by including color coded time-height plots of the attenuated backscatter measurements made by both KARL and AMALi. Such plots would be especially useful in interpreting the 15 May results, where the vertical extents of the two-stream and Raman layers are significantly different. Adding error bars to Figures 1–3 would also be helpful.

Authors: An idea of the temporal evolution of the troposphere for all days under consideration can be found under following link:

http://www.awi.de/de/infrastruktur/stationen/awipev_arktis_forschungsbasis/observatorien_und_infrastruktur/atmospheric_observatory/meteorological_measurements/index_of_tropospheric_aerosol_lidar_measurements_at_ny_alesund/

Regarding the error bars we used several approaches to expose them on the results but found out that the figures are more readable when the errors are drown separately. Now, heaving the error directly on the results and additionally to that on the two further figures would essentially give the same information twice.

Specific Remarks:

Referee: page 20230, line 19: the lidar satellite is spelled ‘CALIPSO’, not ‘CALYPSO’.

Authors: It is corrected.

Referee: page 20233, ~line 11: Is neglecting multiple scattering justified by the instrument and measurement geometries of the lidars used?

Authors: To clarify upon this we added in the discussion following text and references:
‘For all calculations the assumption of negligible multiple scattering effects was made. For the AMALi system, which has a large beam divergence and hence a large FOV, the distance from lidar flight altitude and the ground is so short for nadir-aiming configuration (<2.7 km), that instrumental effect is negligible. Hence, only multiple scattering due to extremely dense aerosol load or clouds and fog could actually matter. However, calculations of multiple scattering effect performed for the case of the mixed-phase cloud system (Stachlewska et al., 2006 ; Gayet et al., 2007)

accordingly to the multiple scattering model (Eloranta, 1998) showed, that on such a short distance effect of multiple scattering was relevant for particle extinction threshold of 0.65 x 10-3 m-1 above which multiple scattering is non negligible. In the retrieved here profiles extinction is of at least 2 orders of magnitude lower and neglecting multiple scattering for AMALi observations discussed here is justified. For the KARL system neglecting multiple scattering is justified by the instrument and measurement geometries of the lidar, some technical details are depicted in Hoffmann et al., 2009. The optical depth for midlevel clouds where multiple scattering sets in is about 0.5, far above the values considered in our study. Hence, we reckon that any influence of multiple scattering can be neglected.'

Stachlewska, I. S., Gayet, J.-F., Duroure, C., Schwarzenboeck, A., Jourdan, O., Shcherbakov, V., and Neuber, R. (2006), Observations of mixed-phase clouds using airborne lidar and in-situ instrumentation, in: Reviewed and Revised Papers Presented at the 23rd International Laser Radar Conference (ILRC 2006), 1, 325–328.

Gayet, J.-F., Stachlewska, I. S., Jourdan, O., Shcherbakov, V., Schwarzenboeck, A., and Neuber, R. (2007), Microphysical and optical properties of precipitating drizzle and ice particles obtained from alternated lidar and in situ measurements, Ann. Geophys., 25, 1487–1497, 2007, http://www.ann-geophys.net/25/1487/2007/.

E. W. Eloranta (1998), Practical Model for the Calculation of Multiply Scattered Lidar Returns, Appl. Opt. 37, 2464-2472.

A. Hoffmann, C. Ritter, M. Stock, M. Shiobara, A. Lampert, M. Maturilli, T. Orgis, R. Neuber, and A. Herber (2009), Ground-based lidar measurements from Ny-A° lesund during ASTAR 2007, Atmos. Chem. Phys., 9, 9059–9081.

Referee: page 20234, line 16: Since the variability of lidar ratio distributions reported in the literature is in the neighborhood of 40% or larger (e.g., Sassen & Comstock, 2001; Chen et al., 2002), I wonder how the lidar ratio of any specific cirrus cloud could be sufficiently well-known to use a priori in deriving a profile of backscatter coefficients. An uncertainty of 40% would seem to be a huge impediment to retrieving a high quality backscatter solution.

Authors: As mentioned already altitudes where Cirrus or subvisible clouds were detected by KARL at about 9 km were treated with lidar ratio of 12 sr, according to the results obtained with the transmittance method (described in Chen et al. 2002) applied to a limited number of cases in this study. We clarify in the manuscript: '(known B_{Ci} for the optically thin Cirrus cloud in this study)'.

Chen W., C. Chiang, and J. Nee (2002),Lidar Ratio and Depolarization Ratio for Cirrus Clouds, Appl. Opt. 41, 6470-6476 (2002)

Referee: page 20235, line 11: Does that phrase "easily detected as a shift in the data sets" mean only that the correlation coefficients were unusually low? Or is something additional implied?

Authors: Indeed, the correlation is very low in these cases.

Referee: page 20235, line 14: I believe the term 'overlap function' is much more widely used – and hence much more readily understandable – than 'geometrical compression'. (Searching Google Scholar for {lidar AND (cloud OR aerosol) AND "geometric compression"} yields 12 hits; {lidar AND (cloud OR aerosol) AND "overlap factor"} returns 312 hits.)

Authors: 'Geometrical compression' is replaced with 'overlap function'.

Referee: page 20235, line 18: What constitutes "sufficient SNR"? Later on in the Discussion section, the authors state that an "SNR of at least 100 is required". This limit should be specified earlier in the manuscript. If this is the SNR specification for a single 60 m range bin, that point should be clarified as well.

Authors: Thank you, in fact it is a typing error. The SNR above 10 for a single 60m range beam is required. As suggested we now specify this earlier in the manuscript.

Referee: page 20237, lines 1–4: I'm afraid I do not understand the intended meaning of this paragraph.

Authors: We wanted to comment on a comparison of the optical depth measured by sunphotometer and lidar for the determination of the lidar constant. The comparison is more appropriate to be done at times with as little as possible cloud contamination. As the sunphotometer and the zenith aiming lidar are looking into different directions in the Arctic atmosphere (low sun elevations) we cannot assume, that there is a time interval where lidar and sunphotometer simultaneously see no cloud. Hence, we did a cloud screening for both instruments separately (in the case of the sunphotometer the data with low particle optical depth and high Angstrom exponent were chosen) and the comparison was done for the clearest time intervals.

Referee: page 20237, lines 5–14: The authors appear to be saying that the scattering ratio in the "aerosol-free calibration range between 10 [and] 12 km" is $R = (\beta_m + \beta_a) / \beta_m \approx 1.30 \pm 0.05$. This hardly seems like an "aerosol-free" region.

Authors: Indeed, there is a mistake in the text. There should be
β_{ref}^{part} = (**0,03** ± 0.05) x β_{mol} = 0,015 x 10^{-6} ± 0.025 m^{-1} sr^{-1} (i.e. R=1.03) for 15 May and 19 May
β_{ref}^{part} = (**0,02** ± 0.05) x β_{mol} = 0,01 x 10^{-6} ± 0.025 m^{-1} sr^{-1} (i.e. R=1.02) for 14 April

Referee: page 20237, line 15: It's not clear whether the boundary condition for obtaining β_{TS}^{part} (h) was C_K or β_{ref}^{part} (though perhaps it doesn't matter, since presumably these are mutually consistent values).

Authors: We wanted to say that we used C_K, but yes C_K and β_{ref}^{part} depend on each other via Eq.1 and Eq.2.

Referee: page 20237, line 21: Please add a reference for "[the] standard for KARL's Raman returns".

Authors: We make additional reference to Ansmann et al., 1990 and Ansmann et al., 1992 in this place.

Referee: page 20237, line 25: Was the 10-minute attenuated backscatter measurement centered within the 20-minute Raman data acquisition sequence?

Authors: Indeed, the 10-minute attenuated backscatter measurements are centered within the 20-minute Raman data acquisition sequence for 19 May 2004 and 14 April 2005. However, due to a warm up of the KARL lidar we used a shifted mean (9:55 - 10:15) for Raman evaluation on 15 May 2004 (Fig.1) compared to a center of 10:00 for the Two-stream. We wondered whether the altitude shift in the extinction layer at 1800m might be due to this delay in time but our 2 minutes 60 m raw profiles did not support this view. Hence, we took the best matching 20min interval available for the Raman comparison.

Referee: pages 20238–39 : This section could probably be shortened a bit. While the authors provide a fine description of the meteorological context in which their measurements were acquired, it seems to me that, for a paper that intends to validate a specific measurement technique, it would be much more useful to describe the differences seen in the two sets of retrievals.

Authors: We see Referee's point of view and partially agree as the demonstration of the method with real data should be the main idea of this paper. However, as some astonishing results came up (large variations in lidar ratio and extinction) we feel that a description of the meteorological situation should be given to understand these results.

Referee: page 20238, line 23: Please add a reference for the assertion that lidar ratios of ~20 sr and extinction coefficients of ~1.5 x 10–5 m–1 are characteristic of 'clean Arctic air'.

Authors: We added the references to measurements taken at the site in Ny Alesund: Stachlewska, 2006 (discussion of data obtained during May 2004 and April 2004) and Hoffmann et al., 2009 (data for April 2007).

Stachlewska, I. S. (2006) Investigation of tropospheric arctic aerosol and mixed-phase clouds using airborne lidar technique, PhD Thesis, University of Potsdam, opus.kobv.de/ubp/volltexte/2006/698/.

A. Hoffmann, C. Ritter, M. Stock, M. Shiobara, A. Lampert, M. Maturilli, T. Orgis, R. Neuber, and A. Herber (2009), Ground-based lidar measurements from Ny-Alesund during ASTAR 2007, Atmos. Chem. Phys., 9, 9059–9081

Referee: page 20239, line 21: Should "uniform strait transport form" actually be "uniform transport straight from"?

Authors: Indeed, it is corrected now.

Referee: page 20240, line 3: Liu et al., 2006 describes useful procedures for estimating λ.

Authors: we add this sentence and the following reference:

Liu, Z., W. Hunt, M. Vaughan, C. Hostetler, M. McGill, K. Powell, D. Winker, and Y. Hu (2006), Estimating random errors due to shot noise in backscatter lidar observations, Appl. Opt., 45, 4437–4447.

Referee: page 20241, line 6: Here and elsewhere, change 'insecurity' to 'uncertainty'. In the context of data analysis, the two words are not synonymous.

Authors: It is corrected.

Referee: page 20243, line 9: Instrument noise seems a highly unlikely culprit for the sorts of differences seen in Figure 1.

Authors: Thank you, we replaced this sentence by following 'Therefore, we address these deviations to real variations of the atmosphere during the longer integration of the Raman-shifted lidar profiles'.

Referee: page 20245, line 9: I think the authors mean 'accurately', not 'precisely'.

Authors: It is corrected.

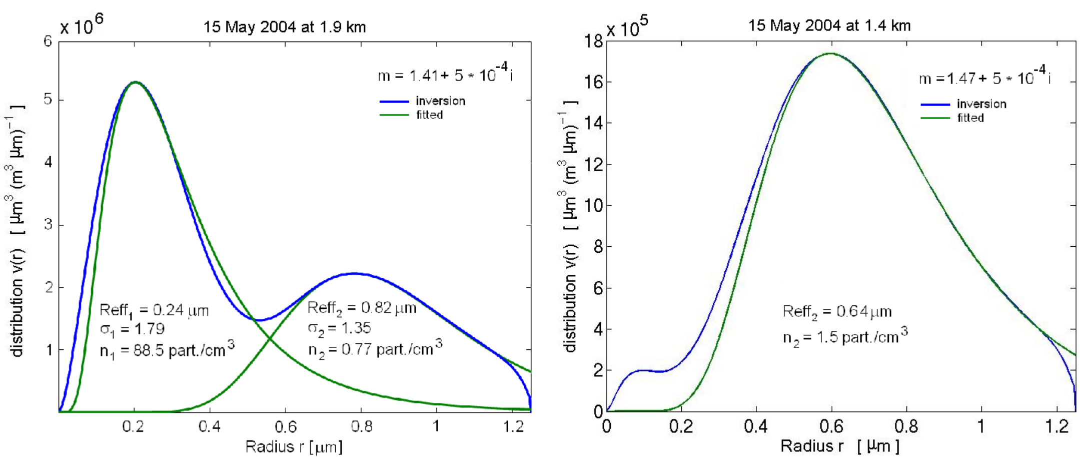
Referee: page 20245, line 16: I am skeptical of the authors' claim that "small spherical water droplets" could produce lidar ratios of ~80 sr. Using a Mie calculator, I have to posit a modal droplet radius of less than half a micron to begin to approach a lidar ratio of 80 sr for gammadistributed spherical water droplets. However, a cursory literature search suggests that the smallest measured droplet sizes are a good deal larger (e.g., Garret et al., 2004).

Authors: Indeed, accordingly to Garret et al., 2004 the smallest measured droplet sizes are much larger than 500 nm. We should have rather talked about particles than droplets here. We corrected this and replaced the sentence: 'The enhanced extinction, together with the high lidar ratios, the high relative humidity and the low volume depolarization recorded by both instruments on both days suggest that these layers were composed of a very small spherical supercooled water droplets, not unusual in the pristine conditions at the Arctic region (Pinto et al., 2001; Treffeisen et al., 2007).' with following explanation:

'The enhanced relative humidity indicates existence of more wet particles in the layers than outside of it. The temperature below -8 Celsius suggests supercooled conditions rather than ice formation, which is supported by the low depolarization ratios indicating spherical particles. The calculations of backward trajectories give no evidence of possible anthropogenic pollution, which could contain absorbing particles. If the layers were dominated by spherical particles which are comparable or much larger than the interacting wavelength size (532 nm) it should show as an enhancement in both extinction and backscatter profile. We hypothesize, that only an increased number of rather small spherical particles (~200 nm) could give an enhanced particle extinction accompanied by a small particle backscatter, i.e. as in the layers at 900 m and 1800 m in Fig.1 and at 1800 m in Fig.2. Hence, we argue that these layers were mainly composed of very small spherical supercooled water particles, not unusual in the pristine conditions at the Arctic region (Pinto et al., 2001; Treffeisen et al., 2007). However, heaving no in-situ measurements within nor outside these layers it is hard for us to discriminate between water and non absorbing cloud activated aerosol, which might consist of a sulfate core with a water shell (similar index of refraction). Hence, the high lidar ratios reported here may also indicate the existence of submicron aerosol particles activated in the humid environment.'

Garrett, T. J., C. Zhao, X. Dong, G. G. Mace, and P. V. Hobbs (2004), Effects of varying aerosol regimes on low-level Arctic stratus, Geophys. Res. Lett., 31, L17105, doi:10.1029/2004GL019928.

For the Referees interest we include in this Authors Comment the result of a microphysical parameters retrieval based on the KARL measurements on 15 May 2004 in the range of layer and below it. For both cases the refractive index is typical for a water particle and its low imaginary part confirm that no absorbing particles are present. In the layer results are dominated by the small particle mode with effective radius ~ 200 nm.



For this retrieval channels 355nm, 532nm and 1064nm as well as the Nitrogen Raman channels at 387nm and 607nm were used. The inversion code assumes Mie theory and was adopted from Böckmann, 2001. Due to higher numerical stability the volume (not size) distribution function is inverted (blue lines). As the particle number is the least secure parameter of such an inversion this parameter is calculated by a log-normal fit (green lines) to the volume distribution.

Böckmann, C. (2001), Hybrid regularization method for the ill-posed inversion of multiwavelength lidar data to determine aerosol size distribution, Applied Optics 40, pp 1329-1342

Referee: page 20245, Fig. 3: What explains the difference of ~50% seen below ~1 km in the extinction and backscatter profiles?

Authors: To comment on this we add on the page 20244 in line 20: 'The difference of about 50% between the Two-stream and Raman retrievals of the extinction and backscatter profiles seen below ~1 km cannot be caused by an overlap misalignment of the groundbased KARL seems as it sees more scattering. It cannot be a result of multiple scattering in the airborne AMALi both because the optical depth is too low for multiple scattering to set in and backscatter and extinction are affected in the same way. Both methods retrieve below 1 km a lidar ratio close to 40 sr which is close to the value measured at our station for Arctic Haze (Ritter et al., 2004), a value lower than the one found for Arctic Haze at lower latitudes (Mueller et al., 2007). Below 1 km altitude we are in Ny Alesund in the regime which is directly influenced by the local orography. Indeed our daily radiosonde (launched earlier at 11:00 UTC) recorded a pronounced temperature inversion around 0.93 km altitude. Hence, it seems that below 1 km altitude more local and variable conditions have been probed by the lidars whereas above 1 km altitude the conditions were more stable. In both regimes the lidar ratio was similar however.'

Müller, D., A. Ansmann, I. Mattis, M. Tesche, U. Wandinger, D. Althausen, and G. Pisani (2007), Aerosol-type-dependent lidar ratios observed with Raman lidar, J. Geophys. Res., 112, D16202, doi:10.1029/2006JD008292