Interactive comment on Atmos. Chem. Phys. Discuss., 9, 18745, 2009.

AMALi – the Airborne Mobile Aerosol Lidar for Arctic research

Response to Anonymous Referee #3

by Stachlewska et al. 2009-10-29

We would like to thank the Anonymous Referee # 3 for all of his valuable suggestions and comments, which certainly helped us to improve this paper and make it more concise and better structured.

In the following we give detailed answers to the issues raised.

General comment

Referee: I am attaching my comments to the paper "AMALi - the Airborne Mobile Aerosol Lidar for Arctic research" by Stachlewska et al. (ACPD-2009-392). I have the feeling that the topic of this paper does not fit exactly the aereas usually published in ACP journals. It is a very technical paper which might be worth publishing in a more technical journal. Anyway I leave the decision ot the editor. If he thinks the paper is worth publishing in ACP, then I would apreciate if major revisions are required to the authors following the comments given in the attached file.

Authors: We agree that this is quite a technical paper. However, the publications based on data of the AMALi lidar were mainly acquired during the ASTAR 2004 and ASTAR 2007 campaigns. Therefore, we consider the paper fitting well into the ACP special edition 'ASTAR'. For future publications based on or related to the AMALi measurements, evaluation schemes or design, it would be advantageous to be able to quote a reference of the ASTAR special issue.

Specific comments

Referee: Section 2, "Instrument description", besides my non-objective point of view that this part is very interesting (I have a technical background), I think that the length of this Section as it is now is not suited for publication in ACP. This section has to be shortened and many (interesting) details have to be deleted.

Authors: The section has been revised and some of the technical parts of this section (e.g. eye-safety issue) were moved to the Appendix. However, due to a positive response of the Referee #1 and the Referee #2 on the technical part of this paper and moreover their request to further enlarge the amount of the technical issues discussed we propose keeping the technical details in the paper.
The main reason for this is the fact that so far the AMALi has been used in six airborne field campaigns and it will be used for future ones (e.g. during SORPIC - Solar Radiation and Phase Discrimination of Arctic Clouds in April 2010 and follow-up campaigns of PAM-ARCMIP Pan-Arctic Measurements and Arctic Regional Climate Model Simulations). The AMALi data were already used for aerosol and could studies by a large community of end-users, which are not only 'lidarists' and we reckon it will be the case also in the future. We have been often approached to answer technical questions regarding the design of the AMALi as well as the evaluation schemes (e.g. in this special issue three ACP and four ACPD papers refer to the AMALi lidar). It would be very good to be able to quote the ACP reference for any further publications which will use the AMALi data.

Referee: p. 18749, Fig. 3 is cited before Fig. 2

Authors: Accordingly to the suggestion of the Referee #2 Fig.2 is removed. The right hand side photo of Fig. 2 showing the integration of the AMALi on board the Polar 5 installed in a zenith-aiming configuration is now moved to Fig.1. The caption for Fig.1 is changed to: 'The AMALi in a nadir-aiming configuration on board the Polar 2 aircraft (left) and in a zenith-aiming configuration on board the Polar 5 (middle). The main AMALi elements are: optical assembly (1) with its interior (photo on the right), laptop (2), safety breaker box (3), laser control and cooling unit (4), and transient recorders (5).'

Referee: p. 18750, “The design of AMALi allows downward and upward measurements in vertical direction for the current configuration on board the Polar 2 and Polar 5 aircraft (Fig. 2)”. But Fig. 2 is only showing upward configuration, isn’t it?

Authors: Now we reference to the new Fig.1 (described above) where both configurations on board both aircrafts are shown.

Referee: p. 18750, The following sentence is not clear “The lidar potentially can be used in a scanning mode, if it is set on a platform allowing movement of the whole system in a vertical/horizontal direction.” It may be replaced by “The optical assembly is small and light enough to be installed in an elevation/azimuth moving platform to perform scans”.

Authors: This sentence is changed accordingly to the Referee's suggestion to: ‘The optical assembly is small and light enough to be installed in an elevation/azimuth moving platform to perform scans.’

Referee: p. 18754, “For all three criteria the sum of the quotients is less than 1” I guess the sum has to be calculated for each wavelengths. For 532 nm (0.5+0.63e-3+0.595) > 1, so?

Authors: No, according to the regulations the sum has to be calculated for each criterion. To clarify we replace ‘For all three criteria the sum of the quotients is less than 1’ with ‘For each criterion the sum of the quotients from both wavelengths is less than 1’.

Referee: Section 2.1.2, “Eye-safety constraints”, is this section necessary for a scientific publication? Well, that's MY opinion. It's also ok for me to shorten the section.

Authors: As stated by all three Referees, this is quite a detailed technical article. Therefore, we consider the eye safety calculations an important detail which can be of interest for technically oriented persons as well as a good reference for present and future airborne lidar systems. However, we agree with the Referee that this calculations do not contain a purely scientific intent. We shortened this part as suggested and moved it into an Appendix of this paper.

Referee: Why talking about the old configuration of AMALI if the instrument is to be used now in its new configuration?

Authors: We recognize at least two main reasons. Firstly, due to a modular design of the AMALi the option to change to the old configuration is still open. Secondly, this ACP special issue ‘ASTAR’ includes publications based on measurements in the new configuration (e.g. ACP papers by Ehrlich et al., Lampert et al., Gayet et al. and ACPD by Ehrlich et al., Lampert et al.) and the old configuration (e.g. ACPD papers Dörnbrack et al., Stachlewska and Ritter). Therefore, both configurations are present in the AMALi paper.

Referee: I suggest the authors to make a drastic cut of this section and only give in a short Table the main numbers.

Authors: It is done as suggested.

Referee: For example it seems to me that the eye-safe altitude of the laser (2.5 km) is of importance and, however, it is not explained in the text how it was found.

Authors: This value of 2.5 km on p. 18753 should read 2.372 km. It is corrected now. Please note that on p. 18754 line 25 to p. 18755 line 4 we explained how the minimum altitude for eye safety considerations was calculated, i.e. ‘The third criterion was used to calculate the minimal permissible height of the eye-safe measurements performed in the nadir-aiming configuration. It is obtained by multiplying the maximum permissible operation height by the square root of the final quotient of the third criterion (0.625), as the exposure intensity is proportional to $1/r^2$. Hence, the permissible height of the AMALi eye-safe operation in the nadir aiming configuration is between 2372 m and 3000 m.’

Referee: At this stage (end of Section 2), I am coming across a fundamental question. There are continuously discussions alternating between the old configuration and the current configuration. Will the old configuration of AMALI be used again in the future? If not, then it should be avoided in the paper. If yes, then the authors should talk about configuration 1 (the current configuration) and configuration 2 (an alternative configuration which has been employed in the past).

Authors: Indeed, we do not want to exclude the possibility of changing back from the new configuration to the old configuration, as the use of the longer wavelength of 1064 nm has its advantages for particular studies (e.g. for observing large aerosol or cloud particles). Hence, we followed the Referee's suggestions and renamed the 'old' and 'new' configurations to the Configuration #1 (the current one) and the Configuration #2 (an alternative one which has been employed in the past). Additionally, we modified accordingly the existing Table 1. We are thankful for this suggestion as we had a difficulty to come up with a good idea on how to refer to each of these two configurations.

Referee: There is no lidar result shown with the old configuration.

Authors: No, results shown with this paper were obtained by the AMALi in the old configuration during measurements taken during the ASTAR 2004 campaign. The new configuration was used during ASTAR 2007 (e.g. Ehrlich et al., Lampert et al., Gayet et al. in this ACP special issue 'ASTAR').

Referee: p. 18758, The expression given for ξ corresponds to Rmin from Fig. 4? Please harmonize magnitudes!

Authors: Accordingly to the Referee #1 Fig. 4 brings nothing new to the scientific community and is now removed. Hence, the magnitudes need not to be harmonized.

Referee: p. 18763, The magnitude $\xi(\lambda)$ should be changed so as to avoid confusion with $\xi(h)$.

Authors: The magnitude $\xi(\lambda)$ is changed to $\varepsilon(\lambda)$.

Referee: p.18765, line 2-19 and p. 18767, line 13-24, In my opinion the introductions of Section 4.1 and 4.2 can be deleted. There is no new information here. The first type endproduct is basically what the quick-look display shows.

Authors: Both sections are omitted as suggested.

Referee: p. 18766, line 10, Rephrase this sentence!

Authors: The sentence is changed to: 'Extinction profiles are obtained with the slope method from signals averaged over 1 min with 7.5 m range resolution (SNR of 15) and range from the overlap up to 4 km from the lidar.'

Referee: Section 4.1.2, It is the first time I hear about the backscatter ratio. What is the advantage of it in front of the usual Klett-Fernald-Sasano products, the backscatter and the extinction coefficients?

Authors: For non-lidar specialists, the backscatter ratio BSR defined by Eq.10 on p.18767 has the advantage to display numbers which are easy to understand as it is dimensionless and in the order of 1< BSR < 100. In a physical sense the backscatter ratio is similar to the mixing ratio and makes easier any direct comparison of data points from different altitudes. It is worth to notice that when the density profile of the atmosphere is known (in our case by the radiosonding at Ny Ålesund and the meteorology on board the aircraft) the backscatter ratio and the backscatter coefficient are equivalent.
The backscatter ratio nomenclature is used in the ACP special issue 'ASTAR' (e.g. Lampert et al., Dörnbrack et al.) as well as by other scientists (e.g. Stein et al., Optical classification, existence temperatures, and coexistence of different polar stratospheric cloud types, JGR, vol. 104, no. D19, p. 23.983–23.993, 1999). Also NASA offers backscatter ratio e.g. http://www.eol.ucar.edu/fadb/resource/show/1185.
To clarify this point we added a following explanation: 'The backscatter ratio of 1 corresponds to the pure Rayleigh scattering by molecules of the air. In the Arctic typical values of backscatter ratio range from 1.01-1.6 for the clear free troposphere. For the Arctic haze in the free troposphere they can reach values up to 5 and near the ground up to 10. For the sea-salt aerosols in the Arctic marine boundary layer values range from 2 to 4. For the Arctic subvisible clouds values reach up to 10 and for the optically thick water clouds they are exceeding 30.'

Referee: Section 4 should be also drastically shortened. I think the paper will interest mostly “lidar people” (since it is a technical paper about a specific lidar) and then should not go into such details: limit the discussion to the method used in each configuration and give the necessary hypothesis. Maybe a Table would be useful.

Authors: This paper was already used for a wide range of applications related not only to the ‘lidar users’ (over 10 papers) where co-authors each time demanded detailed information of the evaluation schemes applied with the particular configuration. This is why we did a detailed overview of the entire lidar evaluation chain. However, bearing in mind that all Referees insist on the shortening of Section 4 to the essential informations, we shortened and rewrote it according to the specific comments of all Referees.

Referee: p. 18769, line 11, hgc = 250 m. I read before _min = 155 m. Page 18776, line 8, 235 m is given. Explain the differences and harmonize!

Authors: The differences are due to the fact that we can adjust the overlap by tilting the laser beam into the field of view of the receiver and hence 155 m < hgc < 235 m, which is explained on page 18758, line 5-9. On page 18769, line 11 we took hgc = 250 m as a rough number and on p. 18776 in line 8 we took the maximum overlap value of 235 m. To harmonize now for all calculations hgc is taken at this maximum overlap of 235m.

Referee: p. 18769, the iterative method looks promising. My concern is about the knowledge of the lidar constant C. In practice it is extremely difficult to estimate and β(hgc) depends totally on C. In Section 3.1.1 it is not said how C is finally estimated. Is it with calculated theoretically with Eq. 5? Then I doubt it is correct.

Authors: With the Eq. 5 we wanted to remind that principally the lidar constant C can be calculated. It can also be measured in laboratory conditions as it is defined by basic lidar properties (e.g. frequently approach of monitoring the laser output by collecting a few percent of the photons emitted by the laser head onto a powermeter is used). However in the case of the AMALi the lidar constant C was not calculated from Eq. 5 neither measured using mentioned above approach.
To avoid confusion we added a following explanation: ‘The AMALi was designed and built in a way to assure a low variation of the lidar instrumental constant C by assuring a stable and environment independent lidar operation. Before the Arctic campaigns system was extensively tested in the laboratory and during several test flights to investigate its stability (we tested laser stability, temperature dependence, warmup time dependence, detector dependence, etc.). Results of these tests gave confidence to a low variability of the lidar constant. The constant itself was found experimentally for several atmospheric conditions according to the methodology described in Appendix C in Stachlewska and Ritter, 2009.

Referee: Section 4.3, I have not read the 3 references listed here and I am missing a short explanation on how the nadir-aiming and the zenith-aiming inversions are combined. It is not an easy task since you have a gap between both profiles [hflight-hgc, hflight+hgc]. Can this be added?

Authors: We do not combine the nadir-aiming and the zenith-aiming inversions from data collected by one airborne lidar. To clarify this we add sentence: ‘Signals colocated in time and space taken by the zenith-aiming ground based lidar are combined with signals from the overflying nadir-aiming lidar. The profiles of the extinction and backscatter coefficients can then be obtained directly by division or multiplication of these colocated signals (Stachlewska and Ritter, 2009).‘

Referee: p. 18772, line 25, Replace “in an experimental way” by “experimentally”.

Authors: It is done.

Referee: Section 5.1, Here again, if the readers are going to be mainly “lidar people” then this section needs to be shortened and its name changed probably. From the title “. . . and instrumental constant estimation” I was expecting a method for retrieving the lidar constant which is missing to apply the iterative method described in Section 4.2.2, and there is nothing said about it. Point 2) and 4) should be strongly shortened and moved to Section 4. Point 4) does not bring anything new. In Section 4) in each method presented a few lines should be added to say if the lidar ratio needs to be assumed or if it can be estimated by the method. Actually, reading back Section 4.1.2 almost everything from Point 2) and 4) is already said there.

Authors: It is substantially shortened and moved to a subsection titled ‘Lidar signal calibration’ in Section 4 according to the Referee's request. A short description on the lidar constant variability is added.

Referee: p. 18774, line 7, Maybe a more appropriate reference here (instead of Matthias et al., 2002) would be: V. Matthias, J. Bösenberg, V. Freudenthaler, A. Amodeo, D. Balis, A. Chaikovsky, G. Chourdakis, A. Comeron, A. Delaval, F. De Tomasi, R. Eximann, A. Hågård, L. Komguem, S. Kreipl, R. Matthey, I. Mattis, V. Rizi, J. A. Rodríguez, V. Simeonov and X. Wang, "Aerosol lidar intercomparison in the framework of the EARLINET project. 1. Instruments", Appl. Opt., 43, 961-976, 2004.

Authors: It is done.

Referee: p. 18774, If the AMALI can not measure as high as the KARL lidar why not calibrating the KARL profiles identically to AMALI (4.8 - 5 km and BSR = 1.06)?

Authors: There is a mistyping in the text. We calibrated the AMALi in the range 4.8 km – 5 km with KARL's value of 1.26 (not 1.06). This value is corrected in the final version. Further, to avoid confusion we added the explanation: 'Generally, the calibration value made at the far range from lidar and in the clear air atmosphere where the lidar does not sense aerosol particles is to foretake (issue discussed in Klett, 1984; stable backward solution). As the KARL system provides the opportunity to calibrate the signal at the tropopause or even in the stratosphere, we used KARL's signals for the initial calibration (with backscatter ratio value of 1.05) to assure higher accuracy of the KARL's retrievals. At the altitude 4.8 km – 5 km where the AMALi profiles had SNR >15 we obtained realistic value of the KARL's backscatter ratio of 1.26 and we used this value to calibrate the AMALi's retrievals. It is worth noticing that although the Arctic is generally considered clear and sparse in aerosol the backscatter ratio 1.05 on that day would be too low for the free troposphere at 5 km.'

Referee: Section 5.2, Nothing is said about the KARL lidar, just a reference is provided. I think a few lines are needed to describe this system.

Authors: According to the Referee's suggestion the following description of the KARL is added: 'The Koldewey Aerosol Raman Lidar (KARL) is a ground based system integrated at the Koldewey station in Ny Ålesund, Spitsbergen (78.9N, 11.9E). It is used for the detection of tropospheric aerosols and water vapur. During ASTAR 2004 the KARL system employed the Nd:YAG laser operating with 30 Hz repetition rate at 355 nm, 532 nm and 1064 nm, each with energy around 2 W. The receiving system is based on two mirrors. The smaller receiver (10.8 cm diameter with FOV of 2.25 mrad) was used for near range measurements from 650 m to 6 km. The larger receiver (30 cm diameter with FOV of 0.83 mrad) was used for far range measurements from 2 km to the lower stratosphere. The KARL detects the IR, VIS, UV elastic backscatter, VIS depolarization, and Raman-shifted wavelengths for nitrogen 387 nm and 607 nm and for water vapur 407 nm and 660 nm. In November 2006 KARL was modified. Hence, during ASTAR 2007 we measured with the new Nd:Yag laser (Spectra Pro 290-50) working at 50 Hz with 10W at 355nm and 532nm and 20 W at 1064 nm. Increase of the energy output by a factor of 5 significantly improved the KARL's data quality (Ritter et al., 2008). After beam widening the laser had an effective divergence of 0.5 mrad. With typically used 10 min integration time and 60m height resolution the elastic wavelengths in this new configuration can be analyzed up to 25 km altitude in a daylight conditions.'

Referee: p. 18776, line 26, the estimation of C is crucial for the iterative method, and here a number is given for C with a reference to a former campaign. I am missing a paragraph on how to determine C from measurements from two systems.

Authors: We added the sentence: 'The lidar constant C was found experimentally for several atmospheric conditions according to the methodology described in Appendix C in Stachlewska and Ritter, 2009.'

Referee: p. 18777, last paragraph, I don't see those striking features in Fig. 8.

Authors: We agree that this is not clear on this plot. To avoid confusion we change this paragraph (p.18777 line 21-25) to: 'The obtained backscatter ratio profiles were studied in detail using the high resolution numerical model EULAG (Dörnbrak et al. 2009).'

Referee: What are the white areas in Fig. 8: areas with too low signal-to-noise ratios?

Authors: To clarify we added at the end of the caption of the Fig.8: 'The white areas indicate lack of signal due to overflying mountains (9:21 - 09:32 UTC) and signal saturation in clouds (remaining).'

Referee: Section 5.3, I have a few doubts here. What does t1 means in (Fig. 8)? How is calculated this error of the backscatter coefficient: it is a mean value? Over which range? Why are we talking of backscatter coefficient if before the authors talked the backscatter ratio (and Fig. 8 is showing backscatter ratios)? Which profile did you set as the "true" profile to calculate those errors: KARL profiles using its Raman capabilities? If so, it should be explicitly stated in the text.

Authors: If the Referee means here the 't1' which appears in the definition of the backscatter coefficient error in the Table 3 we would like to state that there should not be 't1' in this definition. Hence it is removed now. In the Fig.8 there is no 't1'. We guess that the Referee meant T=1. To clarify we added the explanation: 'The error of backscatter coefficient depends linearly on the transmission term, so that any difference between T=1 and T=0.99 can be neglected for the iterative method. Due to the iterative aproach the boundary condition can be estimated dependably on the accuracy of the determination of the lidar constant C. Hence, only three sources of errors remain:
a) error due to the choice of the molecular contribution – error proportional to the air density in our case known from radiosonding with a few percent, hence this error is negligible.
b) error due to the signal noise in the lidar data – this error for SNR > 15 is negligible.
c) error due to the assumption of the lidar ratio LR – this error dominates the accuracy of the solution, however, as the partial derivative $\partial\beta/\partial LR$ can be calculated this error can be estimated (results are given in Section 5.3).'

Referee: Fig. 6, Why don't you put the nadir-aiming at bottom and the zenith-aiming at top for a better understanding the figure?

Authors: It is done.

Referee: Fig. 5 and 6 seem to be there only for illustrating the online displays but I wonder if the legends and the titles should not be made readable (they are not right now).

Authors: It is done.

Referee: In both figures it also seems that the ground which should have a strong signature on the lidar signal is not always clearly visible. Is there any reason for that?

Authors: For the nadir-aiming configurations in Fig.5 and Fig.6 the ground return is not seen when the laser beam cannot penetrate the cloud which is below the aircraft.

Referee: Fig. 7, From this figure it makes no doubt that a smoothing technique (in the method itself or in the software which generated the figure) is employed. A short explanation should be given.

Authors: To clarify we add in the caption of Fig. 7 the sentence 'The original data of the color coded pictures have 10 min / 60 m resolution and are interpolated by the 'contour' routine in Matlab.'

Referee: All over the paper: stick to only one writing: online or on-line!

Authors: We changed all to 'online'.

Referee: Some 2-author references are referenced as (XX & YY, yyyy) and others as (XX et al.,yyyy). Please harmonize references!

Authors: References are harmonized to (XX & YY, yyyy).