

Interactive comment on “AMALi – the Airborne Mobile Aerosol Lidar for Arctic research” by I. S. Stachlewska et al.

Anonymous Referee #3

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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 appreciate if major revisions are required to the authors following the comments given in the attached file.

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.

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

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?

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”.

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?

Section 2.1.2, “Eye-safety constraints”, is this section necessary for a scientific publication? Why talking about the old configuration of AMALI if the instrument is to be used now in its new configuration? I suggest the authors to make a drastic cut of this section and only give in a short table the main numbers. 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.

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). There is no lidar result shown with the old configuration.

p. 18758, The expression given for ζ corresponds to R_{\min} from Fig. 4? Please harmonize magnitudes!

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

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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 end-product is basically what the quick-look display shows.

p. 18766, line 10, Rephrase this sentence!

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?

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.

p. 18769, line 11, $hgc = 250$ m. I read before $\zeta_{min} = 155$ m. Page 18776, line 8, 235 m is given. Explain the differences and harmonize!

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 $\beta(hgc)$ 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.

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?

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

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

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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.

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.

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)?

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.

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.

p. 18777, last paragraph, I don't see those striking features in Fig. 8. What are the white areas in Fig. 8: areas with too low signal-to-noise ratios?

Section 5.3, I have a few doubts here. What does t_1 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.

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

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). 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?

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.

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

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

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

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