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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Three years of routine Raman lidar measurements of tropospheric aerosols: Planetary boundary layer heights, extinction and backscatter coefficients" *by* J. Schneider and R. Eixmann

J. Schneider and R. Eixmann

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Final Author Comment by J. Schneider (on behalf of both authors)

We received two referees' comments on our paper, but unfortunately, only one was published on the ACDP discussion web site. The published one (referee 1) was skeptical about our paper, while the other (referee 2) was positive. Referee 2 wrote: "The manuscript strongly underlines the value of a lidar measurements to establish a quantitative climatology of the optical properties of aerosol." Obviously, we agree with this opinion and therefore, we will submit a revised version of our paper taking into account the comments and critical remarks of both referees.

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We comment on the most important criticisms of the referees in the following:

Referee 1: "Substantial results presented in this paper are hard to find. Due to the high variability of the data no significant conclusions can be drawn from the data, possibly with the exception of the trajectory analysis presented in section 3.3."

The main results we think are of interest are:

- the annual cycle of mean aerosol backscatter coefficients, vertically resolved.

- the annual cycle of planetary boundary layer heights.

- mean extinction coefficients and lidar ratios for summer and winter.

- the above data obtained over three years.

- the variability of the optical properties is larger than the influence of the air mass origin.

Referee 1: "As the authors point out at the end of the paragraph the variability of the data does not allow to distinguish clearly between different aerosol types. Therefore a comparison with model data seems to be pointless."

To our opinion, the comparison clearly shows two things: 1) There is general agreement between our data and the OPAC dataset. 2) The difference between the individual aerosol types in the OPAC set are very small, meaning that one needs high quality measurements in order to infer aerosol types from lidar data.

Referee 1: "Furthermore I do not agree with the authors, that they can present a climatological dataset which might stand for itself. For this purpose much more data and/or a higher quality of data is needed. Unbiased data set? The authors have used only data from cloudfree days. The measurements were performed after sunset. In summer this is shortly before midnight, in winter it is late afternoon. There is no discussion in the paper concerning these conditions/restrictions."

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We agree that more data are needed. Therefore the measurements will be continued at Kühlungsborn and also at the other lidar stations of the network. Furthermore, we will include in the revised version a comparison between our data and published data from Hamburg and Munich which were also obtained in the German Lidar Network, showing excellent agreement in the boundary layer height between Hamburg, where measurements were taken in the afternoon, and Kühlungsborn. The difference between "late afternoon" and "shortly before midnight", in contrast, can be neglected, since the PBL remains steady during night-time due to the absence of convection. We will include this discussion this in the revised version.

Referee 1: "Abstract: the authors should mention, where and when they have measured"

This had already been included in the published ACPD version

Referee 1: "Introduction: it is not necessary to mention the manufacturers of the different instruments in the setup, this is a scientific journal"

I found it several times very useful to get information about manufactures of instrument parts also in a scientific journal, but if this is not wanted in ACP, we can easily skip it.

Referee 1: "figure 4 and 5:

- measurement days used for the figures should be indicated in the time axis (e.g. on the top of the figure)

- time axis is not readable

- figure 3: the overview of the measurement days should be shown in figures 4 and 5 table 1 and figures 1 and 3 are not necessary for the understanding of this paper (see comments above)"

We will indicate the measurement day in Figure 4 and 5 and therefore skip Figure 3. The time axis which was not readable had already been corrected for the published

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ACPD version.

Referee 1: "table 1 and figure 1: these technical details are out of the scope of the journal"

Are instrument descriptions out of the scope of ACP? How can we seriously discuss about measured data without mentioning at least some basic facts about the method?

Referee 1: "My suggestion is, that the authors try to shorten this paper in a way concentrating on the limited findings which are substantial, e.g. the trajectory analysis."

As we explained, we think that all our results are of interest and will therefore submit a revised version which will not be shortened substantially.

Referee 2: "Abstract (and elsewhere, e.g. p.6): The selection of night-time values is strange because the PBL is clearly defined only during day-time; therefore, it should be considered to use "residual layer" instead of PBL; the influence of temperature and humidity on the day-time/night-time aerosol properties should be discussed."

We will discuss these question, but we think that the boundary layer during the afternoon evening and the "residual layer" during night-time are very similar, due to the absence of convection. This is confirmed by the now included comparison with data from Hamburg where 3-year afternoon measurements of the PBL height show excellent agreement with our after-sunset-data.

Referee 2: "Quite frequently the expression "aerosol load" is used (in particular in the Abstract, Secs. 3.3., 4). However, the example in Fig. 12 just displays backscatter coefficients which are influenced not only by the aerosol load, but also by the reflectivity and other properties. Please, reformulate appropriately."

This has been changed.

Referee 2: "The introduction looks much like a description of the method; a few more sentences about the scientific importance and background of the investigations should

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be added (e.g., aerosols and climate, optical properties of the aerosol needed, which are the ones which can be derived from lidar measurements?)"

We added a short overview on these topics to the introduction.

Referee 2: " line 2: "ususally" (usually): the Raman method is just one approach and it is the worst one since it prohibits day-time measurements in a reasonable operating range; "most frequently" is more adequate. A brief overview of the other methods is missing."

This is a misunderstanding: "usually" refers to the usage of the nitrogen signal, not to the general use of the Raman method. We reformulated this part to make it clear and added an overview on other methods.

Referee 2: "k equals 0 not only for cirrus clouds; as mentioned later in the paper low values of k are associated with large particles."

This has been corrected.

Referee 2: "6. Page 4, final paragraph: It is rather dangerous to normalize 1064-nm profiles to the Rayleigh background since instrumental errors may be higher than the Rayleigh contributions. Please, comment."

As we tried to explain, we normalized the 1064 nm signal in cirrus clouds for those days when cirrus were present. From these days, we then estimated the "background" backscatter ratio for the altitude with the lowest aerosol backscatter signal at 532 nm. Then we used this backscatter ratio also for the days without cirrus. The Rayleigh signal between 6 and 8 km is still good enough, even at 1064 nm, to justify this method.

Referee 2: "8. P. 6, lines 28-31: The observation of the highest backscatter coefficients in winter is an interesting observation and deserves some discussion. Are there differences with respect to the other network stations (in general a comparison with the results at the other stations is missing)? The explanation of high summer-time values by stronger convection alone is not true. There are more reasons such as higher

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humidity or enhanced photochemistry."

We included a comparison with data from the stations at Hamburg and Munich that have already been published. For details see above.

Referee 2: "the vertical distributions of the extinction coefficient should be compared with those for the backscatter coefficient which is very difficult given the large variability. It should, therefore, be considered to discuss the seasonal cycle of the lidar ratio instead of that of the extinction coefficient."

Since the lidar ratio is calculated from the backscatter and extinction data, the variability is the same as for the extinction.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 75, 2002.

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