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Interactive Comment

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

Anonymous Referee #1

Received and published: 5 March 2002

A) General comments

In this manuscript the authors present a 3-years time series of lidar measurements at Kühlungsborn, Germany. They have calculated aerosol backscatter coefficients for three different wavelengths (355, 532 and 1064 nm) as well as the extinction coefficient for one wavelength (532 nm). In addition the authors have performed a trajectory analysis to get information on the origin and the type of the aerosols measured above Kühlungsborn during this time.

The material presented in the paper is potentially of interest to the scientific community,



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but for a number of reasons (see comments below), the manuscript is not acceptable in its current form. I therefore do not reccomend it for publication.

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.

In detail:

- 3.1: the authors have calculated from their data set the phase shift between the height of the planetary boundary layer and the sun solstice. However, it remains quite unclear, how to get a shift of 25 days (error bar?) from a data set, where in 12 out of 36 months no measurements are available (e.g. in July and August 1999). For the same reason it is not clear to me, how the authors are able to make meaningful comparisons between the different years.

- 3.2: Lidar ratios given in this section are not the same as those shown in table 2. In general the distinction between summer and winter seems to be very questionable.

- 3.4: 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.

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.

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.

B) Specific comments

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

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

figure 4 and 5:

- plotted are monthly means, but there are only 1.4 measurement days each month on average!

- 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

table 1 and figures 1 and 3 are not necessary for the understanding of this paper (see comments above):

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

- figure 3: the overview of the measurement days should be shown in figures 4 and 5

table 3: what happens to the 46th trajectory in the lower level?

C) Technical corrections

p.6 last row: ŞofŤ instead of ŞifŤ

p.7 third row: ŞwithinŤ instead of ŞwithŤ

figure 6: there is no explanation of the different lines in the figure

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