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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

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For technical reasons, the second reviewers comments to the original paper could not be entered in the usual way. They therefore will be repeated below. Please note, that some of the comments were already taken into account in the paper published in ACPD.

The anonymous reviewer wrote:

The manuscript strongly underlines the value of a lidar measurements to establish a quanitative climatology of the optical properties of aerosol.

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Although the text is clearly written a few improvements should be considered:

1. 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 Şresidual layerŤ instead of PBL; the influence of temperature and humidity on the day-time/night-time aerosol properties should be discussed.

2. Quite frequently the expression Şaerosol loadŤ 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.

3. The introduction looks much like a description of the method; a few more sentences about the scientific importance and background of the investigations should be added (e.g., aerosols and climate, optical properties of the aerosol needed, which are the ones which can be derived from lidar measurements?)

4. P. 2, line 2: ŞususallyŤ (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; Şmost frequentlyŤ is more adequate. A brief overview of the other methods is missing.

5. P. 3, line 20: k equals 0 not only for cirrus clouds; as mentioned later in the paper low values of k are associated with large particles.

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.

7. p. 5, line 9 from bottom: Ştoo lowŤ looks strange; usually signal-induced errors caused by PMTs produce excess signal.

8. P. 6, Fig. 6: The different traces are not explained.

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9. P. 6, lines 28-31: The observation of the highest backscater 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 concection alone is not true. There are more reasons such as higher humidity or enhanced photochemistry.

10. P. 6, Fig. 10: 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.

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

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