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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multiwavelength and polarization lidar measurements of Asian dust layers over Tsukuba, Japan: a case study" *by* T. Sakai et al.

T. Sakai et al.

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I appreciate it very much for the reviewer's critical comments on our manuscript. I accept the reviewer's criticism that the part of the retrieval of microphysical properties of the aerosol is weak based on the simplified assumptions. Thus, we have removed the part of the discussion about retrieval of the size distribution in the revised manuscript. We have only suggest that the observed optical properties can be explained semi-quantitatively using our simplified model. The responses to the details comments are given below.

Details:

1) P10179, Abstract: elastic backscatter lidar

Answer: We have corrected it.

2) P10183, Ismail et al. 1998, in the references 2000

Answer: We have corrected it.

3) P10183, backscatter calibration in the stratosphere (10-15km height) after Russell et al, 1979, 1982 is no longer appropriate for the years after 2000 (see Jager publications after 2000 in GRL and JGR). Use of continental aerosol model (lower troposphere after Ackermann) is not appropriate for the stratosphere.

Answer: In accordance with the reviewers' suggestion, we have recalibrated the backscatter signals using the backscattering coefficients obtained from the recent in-situ observations. We have used the backscattering coefficient that were obtained from the airborne measurements in the upper troposphere over the Pacific Ocean by Pueschel et al. (1994), They computed the mean values for a range of wavelengths $0.385 < \lambda < 1.64$ nm based on the results of the aircraft measurement. We believe that Pueshel's values are more appropriate for calibrating our lidar data than those reported by Jager et al. (GRL, 2002) because the altitude range of the calibration was mostly in the upper troposphere (10-15 km in altitude).

4) P10183, 6is surprizing. How can you obtain such a high accuracy at that comparably long wavelength (difficult Rayleigh calibration when compared to 355 and 532nm)?

Answer: We have miscalculated the uncertainty at those wavelengths because we have used the molecular backscattering coefficient at 532 nm for calculating the uncertainty at the other wavelengths. We have corrected there error in the revised manuscript.

5) P10184, depolarization calibration is a rather important point when discussing desert dust/urban haze/maritime aerosol mixtures, but nothing (zero) is mentioned regarding a careful, quality assured retrieval and calibration of the de-

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polarization ratio. As I understand, the authors use different telescopes for the parallel and the cross-polarized signal components. This sounds rather strange, and I think, calibration is a very difficult task. Did you use lambda/2 plates or polarizing filters to check cross talk in the different polarization channels?

Answer: As the reviewer has mentioned, the depolarization calibration is important when discussing the aerosol mixtures. Therefore, we have calibrated it experimentally using the method described by Adachi et al. (Appl. Opt., 40, 6587-6595) and Sakai et al. (Appl. Opt., 42, 7103-7116). We have cited these papers in the manuscript.

I am afraid that the reviewer has misunderstood that we have used different telescopes for the parallel and the cross-polarized components. We have used one telescope for measuring the two components in the same altitude range. A telescope with 20 cm in diameter was used for the lower troposphere and 40 cm was used for the middle and upper troposphere). Each telescope has a polarizing beam splitter and the detectors for the two components.

Results in sections 4 and 5:

6) P10184, I do not see any correlation between k and depol. Error bars are missing in the plots for the Angstrom values.

Answer: As the reviewer has commented, it might be difficult to see the correlation between k and depolarization in Fig. 1, although the correlation can be seen in Fig, 3. Accordingly, we have deleted the sentences that mention the correlation in Fig. 1 from the manuscript.

7) P10185, differences in the aerosol optical properties may also depend on the dust source and thus on chemical composition, aspect ratio, degree of non sphericity.

Answer: In accordance with the reviewer's suggestion, we have corrected the text by mentioning that the aerosol optical properties may also depend on the dust source and

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thus on chemical composition, aspect ratio, degree of nonsphericity.

8) P10185, Ansmann et al. 2002, I cannot find what you mention..., wrong paper?

Answer: As the reviewer has mentioned, we have referenced the wrong paper. Ansmann et al., J. Geophys. Res., 108, 4783, doi:10.1029/2003JD003757, 2003 is correct. We have referenced it in the revised manuscript.

9) P10186, depolarization ratios >10% do not indicate spherical particles.

Answer: We have corrected the sentence that spherical and/or fine particles were present but not predominant.

10) P10186, again, negative correlation? seems to be somehow speculative (error bars are missing).

Answer: We have deleted the sentence mentioning the negative correlation found in Fig. 1. We have added the error bars in Fig. 1.

11) P10186, imaginary part of 0i is unrealistic.

Answer: As the reviewer has criticized, the imaginary part of 0i is unrealistic for the aerosol in the real atmosphere. However, we have no data about the imaginary part of the fine aerosol. We have assumed that the fine aerosols are predominantly ammonium bisulfate based on the previous airborne measurement (Sakai et al., 2003; Matsuki et al., 2003) of which imaginary part of the refractive index is negligibly small as shown by the experimental result by Tang Munkelitz (1994). We do not discuss the quantity that critically depends on the imaginary part of the refractive index in the revised manuscript

12) P10186, only one (and fixed) value for sigma_g of 1.66 for sulfate particles is unrealistic, may vary, thus simulations with different values are required (at least as part of an uncertainty analysis).

Answer: As the reviewer has criticized, the fixed value for sigma_g might be unrealistic

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for the real tropospheric aerosol. So, we have computed the optical properties for sulfate with different values of sigma_g. For example, the mean radius of the fine mode ranges from 0.07 to 0.17 μm when sigma_g varies from 1.6 to 2.0 when k=1.36. The mean radius of the coarse mode ranges from 0.2 to 0.4 μm when sigma_g varies from 1.8 to 2.2 when k=0. These results suggest that it is difficult to estimate the mode radius from our data if no data is available about the sigma_g. Thus, we abandoned estimating the size distribution and used the model to interpret the observed optical properties and the relation between k and depolarization ratio.

13) P10186, again only one (and fixed) value for sigma_g of 2.00 for dust particles is unrealistic, may vary.

Answer: Please see the above response.

14) P10187, with all the fixed input parameters the computations are trivial and rather questionable.

Answer: Please see the above response.

15) P10187, the derivation (from Eq.(2) to Eq.(3)) should be provided, same for Eq.(4), at least references should be provided.

Answer: We have described the derivation of Eqs. (2) and (3) in the Appendix.

16) P10187 and following pages, the discussion is nothing else as speculation based on questionable results. The lidar wavelengths do not cover the coarse mode (>1micron particles), thus the coarse mode cannot be retrieved from lidar data. The Sun photometer does not allow a trustworthy retrieval of the column size distribution in the presence of maritime and urban particles (in the PBL and even higher up) and dust. Nothing is mentioned to the Sun photometer wavelengths and the wavelengths at which the scattering phase function is measured (is that measured? is this information considered in the retrieval?), what procedure is used to retrieve the size distribution (including all the artefacts, three

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mode distribution), do you use the Dubovik code (see papers from 2002 and 2006)? The model computations of the aerosol microphysics, on the other hand, are based on these simple assumptions (mentioned above) and many fixed input parameters. The impact of the omnipresent maritime aerosol is completely ignored. This is not tolerable. So, the results in sections 4 and 5 are at all questionable, errors are certainly larger than 100The paper must be rejected.

Answer: As the reviewer has criticized, there are many uncertainties in the estimation of the aerosol size distribution. Accordingly, we have changed the title of Sect. 4 to "interpretation of the observed aerosol optical properties". In addition, we have noticed that the many values of the aerosol properties (e.g. refractive index, size distribution shape, and particle shape) were fixed in the model because the properties obtained from our lidar data is very limited. Thus, we have noticed that the model should be just an example that can explain the observed optical properties in the qualitative manner. We have noticed the wavelengths and scattering angle of the sky-radiometer measurement and the method of the retrieval in Sect. 5.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10179, 2007.