Atmos. Chem. Phys. Discuss., 7, S5753–S5758, 2007
www.atmos-chem-phys-discuss.net/7/S5753/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S5753–S5758, 2007

Interactive Comment

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.

Received and published: 8 October 2007

Resonse to General comments

Thank you very much for the kind words and useful commnets on the manuscript. We agree with the reviewer's comment that further cases should be examined to prove the technique. So, we have added two case studies on 31 March and 18 October 2006 in Fig. 3. Unfortunately, we have not measured the other dust events by use of the multiwavelength lidar.

We are now making the information content analysis of the multiwavelength and polarization lidar data to retrieve the particle size distribution of nonspherical particles using CFIE method (Mano, Y., Exact solution of electromagnetic scattering by a threedimensional hexagonal ice column obtained with the boundary-element method, Appl. Opt. 39, 5541-5546, 2000). The result is to be presented in the future paper.

Specific comments

1) There is no discussion of the impact of the different overlap factors for the various lidars on the lower boundary layer aerosol retrieval and, therefore, the optical depth comparisons.

Answer: We believe that the impact of the different overlap factors on the optical thickness is small because we have calculated it from the backscattering coefficients and depolarization ratio at 532 nm for which the lowest altitude of the full overlap is 100 m. However, the error can be caused by assuming that the mode radii of the aerosols were constant over the height. We have added this comment in the manuscript.

2) p10183, first paragraph: the derivation of the Rayleigh ratios at calibration height from Russell is quite old now. What impact would there be on these calibrations by using the background data from the GLOBE experiments by Cutten et al. that were derived both latitudinally and temporally closer to this study?

Answer: As the reviewer's suggestion, we have revised the backscattering ratios at calibration height based on the GLOBE experiments (Pueschel et al., Atmos. Environ., 28, 951-960, 1994). The ratios are 1.01, 1.02, 1.04, and 1.12 at 355, 532, 735, and 1064 nm. Accordingly, we have corrected the sentence and the reference in Sect. 2. The impact of using those values on the retrieved optical properties is small.

3) p 10183, last paragraph: much of this discussion is putting the cart before the horse. This discussion would best be left descriptive and the actual values used in the calculation discussed in Section 4 after you show what data (and method) is used to get k. Some of this reads like conclusion rather than introduction.

Answer: In accordance with the reviewer's suggestion, we have deleted the values of k and depolarization ratio in that paragraph.

4) p 10184, lines 18-20: Figure 1 doesn't show a continuous profile of k with height? Why not?

Answer: We don't show the continuous profile of k because we have eliminated the data of which measurement uncertainty in k at 355 and 532 nm were large (>1.5).

5) p 10187, equation 3: what is the justification for this mixing rule? Is there a reference? It is not clear to this reviewer that the wavelength exponent should scale with the fraction of fine and coarse particles. Similarly, equation 4 does not follow from equation 1. These ratios are dependent on both the fine and coarse fractions. Proof of the separability of these terms or a reference would help here. It may be true in the approximation of the dominance of one term in equation 1, but for equal mixtures, it doesn't appear obvious to me.

Answer: We have described the mixing rule of k, depolarization and the lidar ratio in Appendix A.

6) p. 10188, line 8: a value of r subg supN of 0.3 um for the coarse mode seems very small. The comparison with the sunphotometer measurements referred to in line 11 is premature since there is a section on it following (Section 5), but since you drew it out here, the comparisons of size distributions do not seem at all similar, especially for the October case.

Answer: As following the reviewers' criticism, we have deleted the comparison of the mode radii that are estimated from the lidar data with those obtained with sunphotometer in that section because the estimated value have large uncertainty.

7) p 10188, line 19: the comparison of retrieved Sa values here for the fine mode (presumably sulfate) make sense but the values of 50-60 for Sa for the coarse mode do not agree with Murayama's mean nor with the conclusions of Omar et al. which is currently being used in the CALIPSO retrieval. Table 2 gives no error on the Sa values. Since extinction retrieval and accurate backscatter retrieval

ACPD

7, S5753–S5758, 2007

Interactive Comment

is critically dependent on Sa and these changes in Sa are not trivial, it is not clear why the dust observed in this study is higher than the other studies. The recommendation to determine the backscatter to extinction value in a chamber is interesting, but since the dust is highly aged in this study, it is not clear how one would do that. The low mean radius in the coarse mode could be understood if significant removal in transport had depleted these plumes of the large particles, but it doesn't explain the high Sa values derived here. Was a Raman extinction or HSRL measurement not available for these plumes?

Answer: As the reviewer has criticized, the Sa value computed for the coarse mode (Sa=62) is larger than that reported by previous researchers (Sa=46-47 sr, Murayama et al., 2003, Liu et al., 2002, Cattrall et al., 2005). The reason for the large Sa value is currently unresolved but probably that our nonspherical particle model does not represent the real dust optical properties. The Sa value computed for the model has large variability depending on the particle shape. For example, Sa value ranges from 26 to 86 when the height-to-width (h/w) ratio of the spheroid varies from 0.9 to 0.2. Accordingly, Sa values would be smaller if the proportion of the particles having high h/w ratio was higher than that assumed in this study. Because we have no data about the particle shape of the long-transported Asian dust, it is difficult to conclude why our computed value was larger than the other studies. We have added this comment in Sect. 4.

8) pg 10189: Equation 5: again it is not at all clear to this reviewer that a mixing rule for the backscatter to extinction ratio is valid.

Answer: We have described the mixing rule for the backscatter to extinction ratio in Appendix A.

9) p 10190: lines 5-8: what mass does one derive from these number distributions?

Answer: We have deleted the discussion about deriving the aerosol concentrations because of the large uncertainty in estimating the aerosol size distributions.

ACPD

7, S5753–S5758, 2007

Interactive Comment

10) p 10191: lines 1-5: This discussion is particularly unsatisfying since it compares two essentially unvalidated methods to obtain the volume distributions of the dust aerosol without any way to resolve which is right or wrong. In my limited experience, the large particle modes from sunphotometry are often generated numerically (perhaps as a residual in the retrieval and the requirement to put something in the third mode, perhaps just from noise) and often there is no third mode. This may indicate that the lidar retrieval is better. This discussion is particularly inconclusive and leaves the reader with no ability to discern whether this method works or not.

Answer: We have deleted the discussion about the estimation of the size distributions because of the large uncertainty in the estimated values. Following the reviewer's comment, we have noticed that there is the possibility that the third mode obtained with the sky-radiometer was numerically generated by the measurement error in Sect. 5.

Technical corrections

11) p 10184, line 5: Mischenko spelled incorrectly

Answer: We have corrected it.

12) p 10188, line 24: space after "and" near the end of the line.

Answer: We have corrected it.

13) p 10199, Figure 1: enlarge the figure (relative to the caption) for readability.

Answer: We will ask the editor to enlarge the figure because the size of the figure has been reduced by editing.

14) p. 10200, Figure 2: the cluster of trajectories becomes totally unreadable. Can you plot a mean trajectory with bubble error bars going back in time to include the cluster (or plot only the mean and extreme trajectories)?

ACPD

7, S5753–S5758, 2007

Interactive Comment

Answer: We have redrawn the figure by filling the space inside the outermost trajectories to easily see the transport pathways of the air parcels.

15) p. 10201, Figure 3: the vertical error bars make the figure very busy. Since they are all about the same size, could you not eliminate some and give representative error bars? This is especially true in Figure 3 where they are all offscale anyway.

Answer: In accordance with the reviewer's suggestion, we have given only representative error bars in Fig. 3.

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