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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.

Anonymous Referee #2

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Review of Sakai et al. "Multiwavelength and polarization lidar measurements of Asian dust layers over Tsukuba, Japan: a case study"

General Comments:

This paper presents two dust transport events, one in the spring of 2006 and the other in the fall of the same year, and contrasts the differences in the microphysical properties between the two events. As such, its interest to the general reader is limited. For the more specialized reader (lidar researchers), the authors present a multiwavelenth approach to determining microphysical properties of dust plumes and means by which one can discriminate between dust and anthropogenic particles. This is not the first

time such techniques have been presented in the literature. Papers by Müller (2000) and Veselovskii (2005) have detailed the seven and five wavelength methods that can search microphysical aerosol parameter space to choose most likely combinations of those parameters fitting the aerosol measurements.

This paper uses four backscatter wavelengths and one depolarization channel to infer aerosol properties. The results were closed on aerosol optical depth using a sunphotometer. There is no information content analysis, as was done in the above two papers to show that this is sufficient to reliably retrieve aerosol size parameters as was presented in the paper. The abstract concludes that further case studies are necessary to prove the technique.

This is a weakness in the paper and given that the lidars have been running in Japan for many years (see for example the previous papers by the lead author) as part of the Asian Dust network, why were further cases not examined in this study? It would seem that once the technique has been decided upon, running many such cases would be of more value to the scientific community since they would give a better climatology of the dust events. It is understandable that each event might require significant analysis, but the value of the paper would be enhanced if a few more events were included and provided similar results.

The paper is well written, appropriate to the journal, and of sufficient interest to warrant open discussion. Several ACP questions to the reviewers are appropriate to state here:

2) Does the paper present novel concepts, ideas, tools, or data?

The attempt to use four wavelengths and one depolarization channel with a model to close optical parameters is novel and the data is original.

3) Are substantial conclusions reached?

The conclusions reached here are somewhat at odds with prior work (see specific comments below) and are not conclusive.

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4) Are the scientific methods and assumptions valid and clearly outlined?

The aerosol fine and coarse mixing rule is widely becoming used (c.f. in MODIS data processing). It is not clear to this reviewer that this mixing rule for N, Sa, and d is at all valid and needs substantiation before it propagates into the literature by reference rather than being proven.

Specific comments:

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.

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?

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.

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

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.

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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.

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 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?

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.

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

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

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with no ability to discern whether this method works or not.

Technical Comments: p 10184, line 5: Mischenko spelled incorrectly

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

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

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)?

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.

References: D. R. Cutten, R. F. Pueschel, D. A. Bowdle, V. Srivastava, A. D. Clarke, J. Rothermel, J. D. Spinhirne, and R. T. Menzies: Multiwavelength comparison of modeled and measured remote tropospheric aerosol backscatter over Pacific Ocean. J. Geophys. Res., 101(D5), 1996, 9375-9389.

Müller, D., Wagner, F., Wandinger, U., Ansmann, A., Wendisch, M., Althausen, D., and von Hoyningen-Huene, W.: Microphysical particle parameters from extinction and backscatter lidar data by inversion with regularization: Experiment, Appl. Optics 39, 1879-1892, 2000.

Omar, Ali H., Won, Jae-Gwang, Winker, David M., Yoon, Soon-Chang, Dubovik, Oleg, and McCormick, M. Patrick (2005). Development of global aerosol models using cluster analysis of Aerosol Robotic Network (AERONET) measurements. Journal of Geophysical Research, 110, D10S14, doi:10.1029/2004JD004874.

Veselovskii, I., Kolgotin, A., Müller, D., and Whiteman, D. N.: Information content of multiwavelength 5 lidar data with respect to microphysical particle properties derived from eigenvalue analysis, Appl. Optics, 44, 5292-5303, 2005.

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