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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dual-wavelength aerosol vertical profile measurements by MAX-DOAS at Tsukuba, Japan" *by* H. Irie et al.

H. Irie et al.

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We thank the reviewer very much for reading our paper carefully and giving us valuable comments. Detailed responses to the comments are given below.

General comments:

Comment 1: I suggest to add the following information, which is important for a thorough quantification of the differences between the retrievals at 360 and 477 nm:

(1) Averaging kernels

(2) Error components (noise and smoothing error, model parameter errors)

(3) Information content, i.e. degrees of freedom for signal

Reply: We have added plots of the averaging kernels (Figs. 2a and 2b of the revised

manuscript) and several paragraphs (in section 2) for the quantification of the retrievals at both wavelengths, as the reviewer suggests.

Comment 2: How well can MAX-DOAS capture interesting dynamical features of the boundary layer, such as boundary layer break-ups? To address this, graphs of time series of retrieved aerosol extinction would be very illustrative.

Reply: We are also interested in the capability of MAX-DOAS for capturing dynamical features of the boundary layer. We think, however, that this is beyond the scope of this study, because temporal changes in aerosols at Tsukuba likely occur through more complicated dynamical and chemical processes.

Comment 3: Is the quality of the retrieval dependent on the SZA, i.e. are there any systematic errors at high SZA?

Reply: According to the reviewer's comment, we checked (1) the sum of the smoothing error and the retrieval noise error and (2) the degrees of freedom for signal, for different SZAs. However, we could not find clear correlations of these quantities with SZAs. This may indicate that the quality of the retrieval is dependent not only on SZA but also on other factors, such as the aerosol profile and the quality of the DOAS fit. It should be also noted that the aerosol retrievals have been made only at SZAs<83 degrees to minimize a potential influence of the treatment of the Earth's sphericity in radiative transfer model calculations. This is now stated in section 2 of the revised manuscript.

Comment 4: How do clouds affect the quality of the retrieval?

Reply: According to our previous publication (Irie et al., 2008) in which influences of clouds have been discussed, MAX-DOAS aerosol measurements would be very insensitive to high-altitude clouds (above $\sim$2 km) but that data for k below $\sim$2 km and τ could suffer from a non-negligible contribution of the cloud optical depth below $\sim$2 km. While a cloud screening method applicable to MAX-DOAS is highly desirable, the present work focuses on cloud-free cases, which have been identified by coincident

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lidar and sky radiometer measurements. These are now mentioned in section 2 of the revised manuscript.

Comment 5: Are parts of the discrepancies between DOAS, lidar and sky photometer caused by the sampling of different air masses?

Reply: Yes, parts of the discrepancies should have been caused by the sampling of different air masses. The revised manuscript now states this in sections 1 and 4.

Comment 6: A main conclusion drawn from the data presented here is that the observed systematic underestimation of the extinction at higher altitudes is caused by a small sensitivity at these altitudes. This is not correct since (1) the sensitivity with respect to aerosols is not described by the box airmass factor and (2) small sensitivity does not necessarily cause an underestimation of the retrieved quantities. O4 box airmass factors need to be replaced by weighting functions with respect to aerosol extinction (K = dSCD/dk), and the influence of the a priori on the retrieved profiles needs to be investigated.

Reply: We agree with the reviewer. In the revised manuscript, we have replaced O4 box airmass factors by d(ΔSCD)/dk, as the reviewer suggests. In addition, we now state in section 4 of the revised manuscript that the observed underestimation has occurred as a result of the retrieved quantity being closer to the a priori. Additional sensitivity tests for investigating the influence of the a priori on the retrieved profiles have been made, as suggested by reviewer. The results are now mentioned in section 2 of the revised manuscript.

Specific comments:

Comment 7: A comparison has not only been performed with Lidar, but also with sky radiometer measurements. This should be stated in the abstract.

Reply: This has been stated in the abstract.

Comment 8: Introduction: The statement '...MAX-DOAS measurements could con-

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tribute not only to monitoring of the atmospheric composition changes but also to the understanding of the Earth system' is very general. Atmospheric composition is already part of the Earth system. Can you be more specific?

Reply: We have rephrased this sentence to "... MAX-DOAS measurements would contribute to both the monitoring and a better understanding of atmospheric composition changes, in which aerosols play an important role."

Comment 9: Measurements: Please specify which a priori has been used for the retrieval.

Reply: The a priori profile used is now specified in section 2 of the revised manuscript.

Comment 10: Results and discussion: P19362, L14: Please specify how the Lidar and MAX-DOAS data have been binned to generate the data shown if Fig. 2.

Reply: To specify it, we now state that "The mean MAX-DOAS k values for each 0.05-km^{-1} or 0.02 km^{-1} range of lidar data are plotted..."

Comment 11: The fact that MAX-DOAS extinction coefficients are systematically smaller than lidar is explained by the use of an inappropriate lidar ratio (P19362, L22). However, the same tendency is found when comparing aerosol optical depth from MAX-DOAS with sky radiometer measurements, which suggests that in fact MAX-DOAS retrievals are systematically too small, and not that the lidar extinction profiles are wrong. How do the aerosol optical depths from lidar compare to sky radiometer measurements?

Reply: We think that the tendency found in the comparisons with lidar is different from that in the comparisons with sky radiometer. As discussed in section 4, comparisons of MAX-DOAS with lidar k(0-1 km) data show a linear relationship, whereas the comparisons with sky radiometer rather show a non-linear relationship with significant smaller MAX-DOAS values at greater aerosol optical depths of sky radiometer measurements. On the other hand, comparisons between aerosol optical depths from lidar and sky ra-

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diometer measurements are difficult to interpret, since lidar measurements sometimes had the difficulty to derive aerosol extinction coefficients at higher altitudes with a sufficient accuracy. However, the comparisons showed that the aerosol optical depth from lidar was smaller by 20% on average. This might support our argument that part of the systematic differences between MAX-DOAS and lidar data are due to the use of an inappropriate lidar ratio.

Comment 12: An underestimation of the extinction above 1km is found. Since the sensitivity of the measurements to aerosols decrease with altitude, I wonder to what extend this is influenced by the choice of the a priori profile. This needs to be investigated in order to consolidate the conclusions drawn in section 5 (see below)

Reply: As the reviewer suggests, we have investigated influences of the choice of the a priori on the retrieval. The results are now mentioned in section 2 of the revised manuscript.

Comment 13: Fig. 5 shows O4 box airmass factors, i.e. the sensitivity of the SCD to the partial VCD at a given altitude. However, the important quantity for aerosol retrievals is not the box-AMF but the sensitivity of the measurement to aerosol extinction, i.e. the weighting function K = dSCD/dk. It is not specified for which elevation angle the box airmass factors are shown.

Reply: We agree with the reviewer. In the revised manuscript, discussions on the sensitivity is made using d(ΔSCD)/dk.

Comment 14: In Fig. 6, MAX-DOAS aerosol extinction between 0 and 1 km is compared to the aerosol optical depth from sky radiometer. It would be more useful to compare the integrated aerosol optical depth from MAX-DOAS with sky radiometer, instead.

Reply: We have made comparisons for aerosol optical depths, but the relationship between MAX-DOAS aerosol optical depths at 354 and 476 nm is not as good as that for

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the aerosol extinction between 0 and 1 km, due to the lack of the measurement sensitivities to aerosols at higher altitudes. We rather want to check the internal consistency for the MAX-DOAS k(0-1km) data, which should be most accurate in our aerosol products retrieved from MAX-DOAS measurements.

Comment 15: I cannot see how it can be concluded that '..MAX-DOAS is capable of deriving alpha vertical profile information' (P19364, L27) from this comparison.

Reply: We have rephrased this sentence to "...MAX-DOAS is capable of deriving $alpha below 1 km."

Comment 16 Conclusions: It is stated that O4 box airmass factors are a measure for the sensitivity of MAX-DOAS measurements for aerosols. This is not correct (as already stated above). Instead, the weighting function dSCD/dk describes this sensitivity. Also, it is not correct that a small sensitivity at certain altitudes necessarily leads to an underestimation of the retrieval. Instead, the retrieved extinction should be closer to the a priori at altitudes where the sensitivity is low. Thus it cannot be concluded that 'the cause leading to systematic errors was identified' (P10366, L4) without providing information on the a priori profile.

Reply: This has been addressed as mentioned above.

Technical comments:

Comment 17: P19358, L19: Insert a comma after 'change'

Reply: Done.

Comment 18: P19359, L12: Insert 'of' after 'wavelength'

Reply: Done.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19357, 2008.