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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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Reply to Editor

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

Comment 1: As a general comment, I think that the conclusions formulated by the authors in the last section of the paper are by far too optimistic. From this study, one may realistically conclude that only aerosol extinctions in the lowest atmospheric layer (0-1 km) can be reliably retrieved. At higher altitudes, validation results clearly demonstrate that sensitivity is lacking leading to systematic underestimation of the extinction, which is also and further demonstrated by the AOD comparisons. Without precluding possible major improvements in future versions of the instrument and retrieval algorithms,

the conclusions should be reformulated to give the adequate message.

Reply: Following this comment, we have added the sentence "For better performance, an improvement to this technique, for example, by utilizing the O4 absorption at longer wavelengths, is highly desirable." in the conclusions.

Comment 2: A second main comment concerns the lack of error analysis. Although the authors use the Optimal Estimation method for the inversion, they do not make use of the facilities provided by this method to properly assess the error budget of the measurement. Such an analysis would strengthen a lot the paper, and maybe also help for the discussion of the differences observed when comparing MAXDOAS to lidar and radiometer results.

Reply: We agree with this comment. 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 retrieval errors estimated by sensitivity tests, the sum of the smoothing error and the retrieved noise error, the degrees of freedom for signal, and the area.

Specific comments

Comment 3: P.2, L.15: the sentence ending by "understanding of the Earth System" is a little bit vague and unclear. Please reformulate and try to 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 4: P.3, L.3: why is the minimum angle limited to 3°? Is this due to on-site constraints? Adding measurements down to 1° (which is according to our experience generally easily achievable) would further increase your sensitivity to aerosols in the lowest layer.

Reply: I agree with this comment, but for our MAX-DOAS measurements the minimum

ACPD

8, S11654–S11658, 2009

Interactive Comment

elevation angle was determined to minimize a potential influence of the treatment of the Earth's sphericity in our radiative transfer model calculations.

Comment 5: P.3, L.12: the fitting range used for O4 retrieval at 354 nm extends to rather short wavelengths and therefore might be more easily affected by O3 misfit problems. Have you experimented other possible fitting ranges? How sensitive is the O4 slant column retrieval to the choice of the fitting interval? What are the sources of the absorption cross-sections used in the retrieval, in particular for O4?

Reply: We have tested fitting ranges 320-367 nm, 330-367 nm, 335-367 nm, and 350-370 nm. Strictly speaking, the retrieved O4 DSCDs were not identical, partly due to the difference between air mass factors at different wavelengths, while different fitting ranges give different O4-cross-section-weighted mean wavelengths. In the present work, this dependence has been taken into account by radiative transfer model calculations at the O4-cross-section-weighted mean wavelength (354 nm). On the other hand, our DOAS fit might be affected by O3 misfit problems. However, while the true DSCD is not known, we think that the comparisons with lidar and sky radiometer measurements support the accuracy of O4 DSCDs retrieved from 325-367 nm in the present study. The source of the O4 absorption cross-section is now mentioned as "We used the O4 absorption cross section data of Greenblatt et al. (1990) with manual adjustment (A. Richter, personal communication)."

Comment 6: P.3, end of second paragraph: the discussion on the reasons for the smaller errors obtained at 476 nm is somewhat unclear. My understanding is that this is due to 3 main reasons: (1) larger absorption cross-sections, (2) larger AMFs and (3) larger S/N ratio due to larger intensities

Reply: We agree that this is due to (1) and (2). This paragraph has been revised accordingly. Smaller errors were found at the longer wavelength even when a error comparison was made under the same intensity conditions, as mentioned in section 2.

Comment 7: P.4, last paragraph of section 2: please indicate in which way the other

ACPD

8, S11654–S11658, 2009

aerosol parameters are initialized in your retrievals (SSA, asymmetry factor, etc)

Reply: We have added information of SSA, asymmetry parameter, and surface albedo in section 2 of the revised manuscript.

Comment 8: P.6, first paragraph: to discuss the dependence of the measurement sensitivity to altitude and wavelength, why don't you show a plot of the Averaging Kernels which are commonly used to discuss information content issues in the Optimal Estimation framework?

Reply: In the revised manuscript, plots of the averaging kernels are now included.

Comment 9: P.7, L.7: I think your results show that the angstrom coefficient derived by MAXDOAS is underestimated. This is in fact consistent with your results of Fig. 2, which show that the retrieved extinctions are generally too low at 354 nm while the agreement with lidar data is better at 476 nm, so the angstrom coefficient which derived from the slope of the extinctions at 354 and 476 nm is also underestimated.

Reply: According to this comment, we have added the sentence "This may suggest that the α derived from MAX-DOAS was underestimated, but more work is needed to interpret the difference, with a consideration of the different altitude ranges measured by MAX-DOAS and the sky radiometer."

Comment 10: P.7, last paragraph of section 4: I think that the more compact relationship obtained when comparing MAXDOAS values at the two wavelengths can be due to several reasons: (1) part of the MAXDOAS might be systematic in nature, e.g. due to common approximations in the retrieval process at the two wavelengths, (2) when comparing with lidar data, the scatter also comes from uncertainties on the lidar measurements, (3) one part of the scatter comparing lidar to MAXDOAS probably comes from differences in the sampled air-masses (since the lidar and MAXDOAS instruments use different viewing modes)

Reply: We have replaced "individually" by "separately" in the first line of this paragraph.

ACPD

8, S11654–S11658, 2009

Interactive Comment

In addition, we have added the sentence "Also, it is suggested that part of the differences in comparison with lidar should have come from the uncertainty in the lidar data and the sampling of different air masses."

Editorial comments: Comment 11: P. 2, L. 2: replace "is" by "are" in the sentence "as well as its sign are highly uncertain"

Reply: According to a native speaker, the sentence has been unchanged.

Comment 12: P.2, L. 6: remove "suitable"

Reply: Done

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