Atmos. Chem. Phys. Discuss., 8, S11479–S11482, 2009
www.atmos-chem-phys-discuss.net/8/S11479/2009/

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.

M. VAN ROOZENDAEL (Editor)

michelv@oma.be

Received and published: 2 March 2009

Referee’s report

Manuscript no. ACPD-2008-0448 Authors: H. Irie, Y. Kanaya, H. Akimoto, H. Ibawuchi, and K. Aoki Title: Dual-wavelength aerosol vertical profile measurements by MAX-DOAS at Tsukuba, Japan

General comments

This is a concise manuscript which addresses the validation of aerosol extinction coefficient measurements obtained from MAXDOAS O4 observations at two wavelengths 354 and 476 nm. The MAXDOAS technique is a new method for aerosol measurements, and therefore the assessment of its capabilities and limitations is an essen-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

tial step in the process of firmly establishing the technique. This is addressed in the present study by means of comparisons with coincident lidar and sky radiometer measurements, gathered during 4 months in winter-spring at the mid-latitude urban site of Tsukuba, Japan. Such comparisons involving two wavelengths have not been reported so far and are therefore of great interest for the remote-sensing community and the ACP readership in general.

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.

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.

Specific comments

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. 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. 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

ACPD

8, S11479–S11482, 2009

Interactive Comment

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? 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. P.4, last paragraph of section 2: please indicate in which way the other aerosol parameters are initialized in your retrievals (SSA, asymmetry factor, etc) 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? 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. 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)

Editorial comments:

P. 2, L. 2: replace “is” by “are” in the sentence “… as well as its sign are highly uncertain” P.2, L. 6: remove “suitable”

ACPD

8, S11479–S11482, 2009

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

ACPD

8, S11479–S11482, 2009

Interactive Comment