Atmos. Chem. Phys. Discuss., 8, S9490–S9492, 2008
www.atmos-chem-phys-discuss.net/8/S9490/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "MAX-DOAS measurements in southern China: 1. automated aerosol profile retrieval using oxygen dimers absorptions" *by* X. Li et al.

Anonymous Referee #1

Received and published: 26 November 2008

The manuscript by Li et al. describes MAX-DOAS measurements of oxygen-dimer differential slant column densities (DSCD) in China during the PRiDE-PRD2006 experiment and the retrieval of aerosol extinction profiles based on the observations and radiative transfer calculations. The method chosen to retrieve the aerosol profiles is the current state of the art in aerosol retrievals from MAX-DOAS and the manuscript presents one of the first applications of this approach. A particularly interesting aspect of the study is the intercomparison with ground-based aerosol extinction measurements, which provide one of the few validations of the MAX-DOAS technique thus far. The manuscript is clearly written, presents interesting and new results, and is clearly suitable for publication in ACP. However, I have a number of comments that the

authors need to address before the manuscript can be published in ACP:

It is currently difficult to understand how the measurement errors are considered in the retrieval. I recommend that the authors expand this topic to make it easier for the reader to follow their approach. For example, the manuscript does not present the typical statistical and systematic error of the O_4 DSCD. The authors should also explain the weighing in equation 9 in more detail, i.e. where do the weights come from, etc. In addition, Figure 3, 8, and 9 need error bars.

What is the reason for including the results of case A4 in the manuscript? It seems to me that using an approach that is based on an incorrect description of the atmosphere, i.e. omitting a variable boundary layer height, is destined to fail. A least squares fit based on a physically incorrect model will yield incorrect results, even if they seem statistically sound. The manuscript would not suffer from omitting case A4 completely.

The drying of the air analyzed by the nephelometer could cause a bias in the comparison of the in-situ vs. the MAX-DOAS extinction coefficients. This bias needs to be discussed in more detail. Showing the relative humidities during the experiment and presenting a back-of-the-envelope calculation of the change of the aerosol size and extinction coefficient during the drying process would allow an assessment of this bias and improve the comparison between nephelometer and MAX-DOAS data.

Figure 8 shows considerable variations of the boundary layer height in the afternoon, i.e. the retrieved BLH drops suddenly at 15:30 and at 17:30. I am not aware of a mechanism that could lead to a reduction of BLH on these short timescales. Are these results statistically significant or is there another cause for such a rapid change in BLH? Averaging the data over 9 days seems to smooth the BLH variation, but this approach seems somewhat arbitrary. It is unclear if the averaging just reduces statistical uncertainties or if there is a problem with the measurement/retrieval process for individual data points. These issues need to be addressed to give the reader confidence that the methods described in the manuscript are sound.

ACPD

8, S9490–S9492, 2008

Interactive Comment

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17661, 2008.

ACPD

8, S9490–S9492, 2008

Interactive Comment