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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 #2

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The paper by Li et al. reports on observations of aerosol parameters using ground-based MAX-DOAS measurements in the polluted environment of Pearl River Delta region 50 km north of Guangzhou. The study focuses on two major topics: 1) The concept for the retrieval of the aerosol extinction and the height of the boundary layer using O4 absorption measurements in the UV is described. 2) For a limited data set (nine days) the results of the aerosol retrieval are compared to ground-based nephelometer measurements of aerosol properties.

The usage of MAX-DOAS measurements to derive aerosol properties has already been shown in previous studies (e.g. Heckel et al., 2005, Wittrock et al., 2004, and

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in more detail in Irie et al., 2008, new paper in ACPD). But the technique is in general novel and validated data sets in particular for regions with high aerosol load are scarce. For that reason the study is clearly suitable for publication when the authors address for major revisions/corrections as detailed below. Since the paper is more of technical nature and the scientific outcome limited it might be worthwhile to publish the paper in the new journal "Atmospheric Measurement Techniques (AMT)".

General comments:

I agree with referee 1, that more information on the uncertainties of data presented here is needed, in particular, how the O4 error bars are considered in the retrieval. What's also missing is a more detailed statement on the errors of the nephelometer measurements.

I am quite critical about the argument on the maximum number of retrieval parameters (page 17669, last paragraph) for several reasons: On one hand measurements for different elevation angles are not independent from each other (depending on actual meteorological conditions), on the other hand each retrieval step comprises more than one scan and hence different azimuth and solar zenith angles (which increases the degrees of freedom). In principle the authors are right, that the information content from this type of measurements is limited. But the real number of possible parameters (degrees of freedom) to be retrieved changes dramatically with actual conditions. Therefore more advanced retrieval methods like optimal estimation are able to characterize the degrees of freedom for each data point. Friess et al. have made an excellent study on theoretical aspects of the aerosol retrieval from O4 measurements and came out with a number of less than 2 (degrees of freedom) for a retrieval taking into account only the O4 absorption in the UV. The authors should change their argumentation by addressing these issues.

Description of the extinction profile (Section 3): To my knowledge the extinction profile in polluted areas is highly variable with altitude and a well-mixed boundary layer unlikely

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(see e.g. Ansmann et al., 2005, High aerosol load over the Pearl River Delta, China, observed with Raman lidar and Sun photometer, GRL). The authors should consider this in their error discussion.

Why the authors introduce the scaling height for the aerosol in the free troposphere as an additional retrieval parameter? Several studies before (e.g. Friess et al. , Wittrock et al., 2004, and much more) have shown, that the sensitivity of MAX-DOAS observations to higher altitudes is very small. This is in particular the case for a high aerosol load and in the UV spectral range. If not applying a full profile retrieval like optimal estimation I would expect the best results by using a three-parameter model (A5 without scaling height in this study).

For similar reasons as referee 1 I am sceptical about the benefit of the two-parameter model A4. As also pointed out by the authors this model gives reasonable results for high aerosol load only.

Further corrections/comments:

Title: Since the information output of the presented data set is quite limited, I suggest to change the title to ... aerosol retrieval using ... . The word "profile" implies much more than a few values for the aerosol properties and only one of them being validated.

Abstract: The second sentence is misleading. The sampling for different elevation angles is done sequentially not simultaneously. Furthermore the O4 absorption is analysed between 352 and 390 nm which comprises two absorption bands at 360 and 380 nm, respectively.

Introduction: I recommend to redraft the whole section. In general, I would expect a more detailed discussion on other papers showing the practical possibilities to retrieve aerosol properties from MAX-DOAS O4 measurements. There is no need to spend time and space to explain the basic concept behind DOAS or MAX-DOAS since this

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has been done several times in other publications (in a more precise way, see below). It is also not clear to me, why the author describe in detail properties of the used radiative transfer model. A lot of the information given here is needless at this point and should be shifted to section 3.

Minor corrections in this section:

- line 11: (Platt and Stutz, 2008 and references therein)

- line 14: remove "profiles"

- line 16 and below: add for NO2 (Wittrock et al., 2004, Brinksma et al., 2008, Irie et al., 2008), for HCHO (Heckel et al., 2005), for CHOCHO (instead of glyoxal) (Wittrock et al., 2006)

- line 18: change "developed" to "described"

- page 17663: The "idea" of DSCDs is not only introduced for the description of measurements focusing on the troposphere. The DSCD is the standard outcome for DOAS observations from the ground, since there is no possibility to retrieve the SCD without any further assumptions. Please change all equations accordingly.

- page 17664: "Deutschmann": The authors should refer only to publications or thesis which are available to all potential readers (e.g. online with a fixed link).

- page 17666, line 6: at azimuth. ? word missing? In addition, the measured O4 towards the sun is not always lower than for the other directions. For higher solar zenith angles this is the other way round.

Experimental: What is the field of view of the instrumental setup? What's the impact on the results?

- page 17666, line 2: "contains" instead of "containes"

- page 17668, line 1: "with adjustments"

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-I0-effect: Is it really needed to take into account this effect? What's the impact on the results?

- section 2.3: please redraft to avoid too often "our";

Radiative transfer modelling: Not only RTM is described here but the whole retrieval method. Please change the title.

Page 17670, line 17: "extinction at ground level" ?, that's the same for the whole boundary layer, or not?

Page 17672, line 25: really right errors? 1.7 +/- 4.0?

Page 17674: Here the authors discuss several reasons on the large deviation between nephelometer and MAX-DOAS in the morning hours. One possible argument is missing: the telescope is pointed towards the east which means towards the sun in the morning. As shown in several studies before the radiative transfer in this case is quite difficult to model due to the forward peak of the aerosol scattering (e.g. Hendrick et al.), consequently the possible error in the modelled O4 column very high.

Page 17675: please change "aerosol profiles" to "aerosol properties", see arguments above.

Figure 1: not sure that this figure is needed.

Figure 7: definitely useless, all necessary information is given in Figure 8.

Add error bars in particular to figures 8 and 9.

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