Atmos. Meas. Tech. Discuss., 4, C174–C177, 2011
www.atmos-meas-tech-discuss.net/4/C174/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Eight-component retrievals from ground-based MAX-DOAS observations" *by* H. Irie et al.

Anonymous Referee #2

Received and published: 23 March 2011

The manuscript by Irie and co-workers on the retrieval of MAX-DOAS observations fits clearly into the scope of AMTD/AMT. It contains a very useful and new set of data, both from experiment and model. In the major parts of the manuscript the scientific methods are clearly outlined and the results support the interpretations except some issues noted below. In general, it is well written and the figures, tables, and references are adequate for this work. Also the abstract covers the main ideas and outcome of the study. However, some points are not fully clear to the referee, as outlined below. Therefore, I support the publication of a revised paper in AMT if the following issues are addressed.

General comments:

- HONO is a strong absorber ($\sigma \approx 5 \times 10^{-19}$ cm^2) in the near uv-range. In addition, HONO can reach 10% of the NO_2 at ground level. Therefore, it might influence the absorption spectra. Please comment on additional absorbers which are not considered here.
- The authors use 8 different spectral windows for their evaluation (Table 1). It is not clear to me why HCHO and AEC_{357} need different fitting windows. I also do not understand why NO_2 was not deduced from the strongest differential absorptions around 435 nm.
- In section 3.2.1 the aerosol extinction retrieval is described in detail. The authors use the OEM to retrieve the aerosol profile in the same way as in Irie et al. (2008). However, from the present text it is not fully clear why additional Monte Carlo calculation to create a lookup table are done. As far as I understand the LUT is used to calculate the $F(x_i)$ and the $\mathbf{K}_i$ as input for the OEM. Other studies (e.g Friess et al. 2006) use Optimal Estimation (with internal forward model) only, while Wagner et al. (2004, 2007) and Pikelnaya et al. (2007) and Li et al. (2010) use Monte Carlo radative transfer model to compare/fit the DSCDs. Instead of repeating part of Irie et al. (2008) it might be helpful to comment on the pro and cons of the method here compared to other approaches.
- In a paper by Zieger et al. (2011) on the same campaign the MAX-DOAS results do not well match the locally measured aerosol data. It might be useful here to make a not how this might affect the retrieval of the trace gases.
- The linear regression procedures are not explained. However, when dealing with data having statistical errors in both coordinates methods like *fitexy* (c.f. book by Press et al. 1993, Numerical recipes) should be considered. They also provide meaningful measures for the goodness of the fit and the errors of the deduced

parameters. This method *fitexy* was widely used in intercomparisons of instruments.

- Section 3.2.2, Page 652: The ΔA_{box} depends not only on the AOD but also on the absorption cross section of the considered absorber (Marquard et al. 2000, Rozanov and Rozanov, 2010). So, it is hard to understand how to re-calculate the ΔA_{box} LUT (in which the absorber is a specific trace gas, e.g. NO_2) based on an existing ΔA_{box} LUT (in which the aborber is O_4) which was created at different wavelength. In priciple, when switching from one absorber or wavelength to another, an accurate AMF can only be achieved by a new RTM calculation. Moreover, the LUT setup for the trace gas retrieval is not so clearly described. Please make a note how the AMF LUT during the trace gases retrieval was created and about its wavelength and cross section dependence.
- A weak part of the manuscript is the discussion on the HCHO and CHOCHO. In my opinion, an interpretation of the ratios of HCHO to CHOCHO requires more information (VOC levels, local sources, etc.) which is not in the focus of this manuscript. I suggest to focus on the the comparison of MAX-DOAS retrieved data with the locally measured data and, if measured data are not available, with model results. An interpretation in terms of chemistry (ratio of HCHO to CHOCHO) is outside the scope here.

Technical corrections:

- Page 644: Please cite Platt and Stutz (2008) instead of Platt (1994).
- Section "Conclusion" should be renamed to "Summary and conclusion"
- Figure 1 is a bit confusing since the different wavelength ranges are mixed around

the 8 panels. I suggest to have one wavelength scale and the different spectra stacked with fitting ranges (see also note on Table 1) marked.

- Figure 2: Why using K instread of AEC in the rest of the manuscript?
- Figure 5: Very dense figure. Please make a note in the caption why the CHIMERE results are scaled.
- Figure 6: Are error bars for CIMEL available?
- Figure 9/10: Are these regression lines meaningful? (see also note on regressions above)

References:

- *Friess et al. 2006, J. Geophys. Res. 111, D14203, doi:10.1029/2005JD006618*
- *Irie et al. 2008, Atmos. Chem. Phys. 8, 341–350, doi:10.5194/acp-8-341-2008*
- *Li et al. 2010, Atmos. Chem. Phys. 10, 2079-2089, doi:10.5194/acp-10-2079-2010*
- *Pikelnaya et al. 2007, J. Geophys. Res. 112, D10S01, doi:10.1029/2006JD007727*
- *Platt and Stutz 2008, DOAS, Springer, Heidelberg, ISBN 978-3540211938*
- *Press et al. 1993, Numerical recipes, Cambridge University, ISBN 0-521-43108-5*
- *Wagner et al. 2004, J. Geophys. Res. 109, D22205, doi:10.1029/2004JD004904*
- *Zieger et al. 2011, Atmos. Chem. Phys., 11, 2603-2624, doi:10.5194/acp-11-2603-2011*

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 639, 2011.