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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ground-based MAX-DOAS observations of tropospheric aerosols, NO_2, SO_2 and HCHO in Wuxi, China, from 2011 to 2014" *by* Y. Wang et al.

Anonymous Referee #1

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This paper presents long-term (May 2011-November 2014) MAX-DOAS observations of tropospheric aerosols, NO2, SO2 and HCHO in Wuxi, China. Vertical profiles of trace gas concentrations and aerosol extinctions are retrieved using a new inversion algorithm called PriAM. It is based on the Levenberg-Marquardt modified Gauss-Newton numerical procedure and uses the SCIATRAN radiative transfer model as forward model. In the first part of the paper, MAX-DOAS observations and the PriAM algorithm are described and the following issues are investigated: impact of the surface pressure and temperature seasonality regularly observed in Wuxi on O4 VCD and aerosol retrieval, observed differences between VCDs derived using the geometric approximation and the profiling algorithm, impact of the sky conditions on the aerosol and trace

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gases retrievals and on the agreement between MAX-DOAS and correlative measurements (AERONET, LP-DOAS). In the second part of the paper, MAX-DOAS data are used to characterize the seasonal, diurnal, and weekly variations of NO2, SO2, HCHO, and aerosols.

This is a very interesting study of high scientific quality which fits well with the scope of ACP. I recommend its final publication after addressing the following comments:

Major Comment:

Although clearly structured, the manuscript is difficult to read due to the large number of figures and panels in the manuscript itself (29) and in the supplement (28). The authors should improve the readability of their paper for the final publication in ACP by focussing on the main results only and asking themselves which figures are needed to best illustrate these results.

Specific Comments:

1) Sect. 2.2.3, page 6: One important result is the impact on the seasonal variation of the pressure and temperature profiles on the retrieved AODs and aerosol extinctions. So far most MAXDOAS groups were using US Standard Atmosphere in their aerosol extinction profile retrievals from O4 slant column densities. The authors show in their study that ignoring this systematic seasonal variation in Wuxi can cause a 20-30% bias on the retrieved AODs and near-surface aerosol extinctions, possibly yielding to unrealistic seasonal variations of these quantities. Is such a large effect only specific to the Wuxi region or can we expect similar features in other parts of China, especially the Beijing area where several MAXDOAS instruments are currently in operation ?

2) Sect. 2.2.4, page 6 and Sect. 2.2.5, page 8: The evaluation of the internal consistency of the inversion algorithm and the validation of the retrieval results are performed for favourable measurement conditions, i.e. 'clear-sky with relatively low aerosols (average AOD of about 0.6)'. 'average AOD of about 0.6' is for me very vague. Is it the

daily or hourly average ? Looking at Figure 9 showing the scatterplots of the MAX-DOAS versus AERONET AODs, MAXDOAS AOD values much larger than 0.6 (see scatterplot for Spring) are selected while this figure is supposed to illustrate the agreement between MAXDOAS and AERONET in low aerosols (and clear-sky) conditions. I think a clarification is needed here.

3) Sect. 2.2.5, page 8: MAXDOAS AODs are validated using AERONET data from the Taihu station, which is located at 18 km south west of the MAXDOAS instrument location. Based on the coordinates mentioned on the AERONET website, the sun photometer is located in the south of the Taihu mountains, at the edge the Taihu Lake (west of the lower left corner of the orange rectangle in Figure 1b), which seems to be a much more remote area than the one from where the MAXDOAS instrument is operating (see also figure 1b). The question is therefore how representative is the Taihu station compared to the location of the MAXDOAS instrument ? Did the authors consider this point for the interpretation of their comparison results ?

4) Sect. 2.2.5, pages 8-9: the validation results are discussed only in terms of absolute differences between MAXDOAS and correlative data. It would be useful for the reader to have also an idea about the corresponding relative difference values.

5a) Sect. 2.2.6, pages 9-10: Is it really useful to show the scatterplots of MAXDOAS versus correlative data and histograms of the absolute differences between MAXDOAS and correlative data for all sky conditions, seasons, and trace gas (TG) and aerosol variables (Figs 15-19 and S20-23) ? I think the manuscript could be simplified here. My suggestion for the final publication is to remove all the histograms from the manuscript and to present the linear regression and correlation results in a table. The latter could also be presented in a panel like the ones usually used in MAXDOAS intercomparison campaigns for summarizing the slant column density comparison results (see e.g. Figure 6 in Roscoe et al., Atmos. Meas. Tech., 3, 1629–1646, 2010; the authors would show for each TG or aerosol variable, the correlation coefficient, slope, and intercept values in three different subplots, replacing the x-axis by the sky conditions and the

elevation angles (colored circles) by the seasons).

5b) Based on these comparison results (Figs 15-19 and S20-23), the authors have developed recommendations for determining under which sky conditions which TG and aerosol data products can be used or not. This filter scheme is presented in Table 3. However, nothing is said on the criteria (e.g. which threshold values on correlation coefficients and/or slopes, etc) used in practice by the authors to develop these recommendations. Maybe a panel summarizing the linear regression results as suggested in comment 5a could also support the discussion here.

6) Sect. 2.2.7, page 12, lines 12-13: the total error budgets of aerosol retrievals are simply reported as error of TGs related to the errors of aerosols. This is not correct because the relationship between the aerosol extinction profiles and the TG retrievals is not linear, i.e. a 15% difference in the aerosol extinction profile used in the TG retrievals does not lead necessarily to a 15% difference in the retrieved TG profiles. This point should be further investigated and corrected in the revised manuscript.

7) Section 3.2: Interpreting the retrieved profile shapes should be done with caution given the fact that the average DFS is only around 2. This particularly the case for the HCHO profiles which show a secondary maximum around 1km. A possible explanation is of course the transport of longer-lived VOCs to higher altitudes but with a DFS around 2, one should not totally exclude the possibility of a retrieval artifact.

Suggestions for technical corrections:

*Page 1, line 16: ‘extinctions’ -> ‘extinction’

*Page 2, line 26: ‘ground based’ -> ‘ground-based’; this should be corrected throughout the manuscript.

*Page 3, line 8: ‘Clemer’ -> ‘Clémer’; this should be corrected throughout the manuscript

*Page 3, line 22: ‘humidity controlled’ -> ‘humidity-controlled’

*Page 3, line 34: ‘so called’ -> ‘so-called’

*Page 5, line 26: ‘long term’ -> ‘long-term’

*Page 6, line 11: ‘session’ -> ‘section’

*Page 12, line 21: ‘budges’ -> ‘budget’ or ‘budgets’

*Page 18, line 10: remove ‘.’ after ‘found:’

*Page 38, Figure 7: the ‘2’ of ‘NO2’ on the x-axis label of subplot (b) is cut.

*Page 39, Figure 9: remove the ‘,’ after ‘Figure 9:’

*Supplement, page 8, Figure S2: There is a problem with y-axis labels of subplots (a) and (b).

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