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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Four years of ground-based MAX-DOAS observations of HONO and NO_2 in the Beijing area" *by* F. Hendrick et al.

Anonymous Referee #2

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In the manuscript "Four years of ground-based MAX-DOAS observations of HONO and NO2 in the Beijing area" written by Hendrick et al. long term observations of HONO and NO2 in Beijing and Xianghe are presented. Some interesting details about the MAX-DOAS retrieval are given and generally the paper is written in a clear and well structured manner. The variations of HONO, NO2 VCDs and ground concentrations, and their influence on OH production rates are analyzed on different time scales. Since the topic is relevant for ACP I recommend publications after some revisions.

General comments:

There are almost no validation studies presented in this paper. Especially for the novel HONO retrieval the results should be compared to other data sets. The only compari-

son mentioned is the comparison with the diurnal cycles of NO2 with similar measurements by Ma et al (2013) and they don't seem to agree well. A comparison of the NO2 column densities with satellite derived VCDs might be very useful. Regarding the HONO results a comparison with chemical transport model output could also share some light about the uncertainties.

The authors should try to find some independent data set to validate their results in order to show that the observed patterns and estimated uncertainties are reasonable.

Specific comments:

P10625, L15f: The sentence reads like you are saying that MAX-DOAS has a higher sensitivity to absorbers close to the ground than LP-DOAS, is that what you meant? Maybe you mean compared to other scattered sunlight DOAS techniques (e.g. ZSL-DOAS)? Please clarify

P10626, L21f and P10628, L15f: First you mention that the MAX-DOAS instrument is described in Clemer et al (2010), but in Clemer et al (2010) four wavelength regions (360, 477, 577, and 630 nm) are used for the O4 retrieval, whereas you are using only two wavelength regions (around 360 and 477). The uncertainties of the DOAS retrievals at larger wavelengths might increase but the results still contain useful information. Please explain why you are using a different retrieval algorithm (if you do). If your algorithm differs from the one described by Clemer et al., you should provide more details about the aerosol profile retrieval and show some examples.

P10627, L2f and L11: You write "Scattered light is collected at various elevation and azimuth angles. . .", but the azimuth direction is fixed (L11). Maybe you mean something else, so please clarify.

P10629, Eq.1: Did you check whether this profile shape is appropriate for an urban environment? Are the deviations shown in Fig. 2 and 3 typical or do they average out over time? If they do not average out, you could use an average profile shape scaled

by the VCD of the 30 deg scan as the a priori. Did you try alternatives? Another option would be to use the last accepted profile shape of the time series as the a priori for the next inversion. Please justify why you chose this profile shape.

P10630, L3: The geometric approximation is based solely on Rayleigh scattering, so if there are large amounts of aerosol present and hence a high degree of Mie scattering, this approximation becomes inaccurate. However, you have this information from the first part of the MAX-DOAS inversion, so why not use it here?

P10632, L3f: The rejection of contaminated retrievals is a very crucial issue, so some more information might be useful here. For example, did you check what is causing the rejection using independent data like cloud observations? How many scans are rejected based on each of the three criteria and overall? Why don't you use the color index as additional information as presented by one of your colleagues at the EGU meeting this year ("Development of a cloud-screening method for MAX-DOAS measurements", C. Gielen et al., 2013)?

P10635, Sec.3.2: Why are there no rush hour peaks in your NO2 signal? LP-DOAS measurements of NO2 ground concentrations e.g. in Hong Kong can see a significant increase during rush hour time (see Chan et al., NO2 measurements in Hong Kong using LED based long path differential optical absorption spectroscopy, AMT, 2012), so it should look similar for Beijing, at least to some extend in your surface concentration results.

Could you elaborate on what you think is causing the difference in diurnal variations compared to the results by Ma et al.(2013)? The spring and summer cycles don't agree at all and I wouldn't call a correlation coefficient of 0.6 for fall to "agree well". Did you compare only the average for the entire period or could you compare the seasonal averages on an annual basis? It would be interesting to see if the discrepancies change over time or not.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10621, 2013.