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Interactive comment on “Retrieval of tropospheric NO₂ using the MAX-DOAS method combined with relative intensity measurements for aerosol correction” by T. Vlemmix et al.

Anonymous Referee #2

Received and published: 14 July 2010

The paper by T. Vlemmix et al. "Retrieval of tropospheric NO₂ using the MAX-DOAS method combined with relative intensity measurements for aerosol correction" reports on a new two-step retrieval of the vertical column of NO₂. The retrieval is based on measured relative intensities and NO₂ differential slant column densities as well as radiative transfer calculations. It is applied to a couple of days reflecting the spectrum of atmospheric conditions at the measurement site during 362 days. The validity of the retrieval is substantiated by a comprehensive sensitivity study as well as comparisons with AERONET and OMI satellite measurements. The paper meets the focus of Atmospheric Measurement Techniques. It is well organized, clear and written in detail. I recommend the publication after the following minor revisions and technical

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corrections:

p. 2320, l. 11: "... is about 5 to 10 km" Please point out that this number is wavelength dependent and might only be true for the wavelengths used in this paper.

p 2323, l. 14: I could not find any description of the Mini MAX-DOAS instrument in Sinreich et al. (2005).

p. 2327 section 2.4: Are the intensities corrected for dark current and electronic offset before the ratio is taken? Are you applying the ratio also to the zenith spectrum which is closest in time? Please describe this in the manuscript.

p. 2330, l. 22-24: Why a simulation excluding NO₂ is needed? The differential air mass factor can be derived by simulations at the elevation angle and at zenith direction, both including NO₂, taking their difference.

p. 2339, l. 23-25: Since the errors of the retrieval and the analysis of the 30° elevation angle values can easily explain a difference of 20% there is no significant difference. They rather agree within their errors. Also, the conclusion p. 2340, l. 1-3, is not proven in Fig. 13. Thus, the next conclusion of p. 2340, l. 4-5, is invalid, too. Is the difference between the retrieval of this paper and the GA dependent on the AOT? Can you please comment on that?

p. 2340, l. 16: The 1251 data points are out of how many points total?

capture Table 1: The calculation ' $[P(\text{case2})-P(\text{case1})]/P(\text{case1}) \times 100\%$ ' instead of the given equation makes sense since only then you describe the deviation of case2 from case1 in %. Please change the table and the numbers in the text accordingly.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 2317, 2010.

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