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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of NO_x emissions from Delhi using car MAX-DOAS observations and comparison with OMI satellite data" *by* R. Shaiganfar et al.

Anonymous Referee #2

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The authors present first measurements in India by Car MAX-DOAS to assess the NOx emission inventory of New Dehli. The paper is generally well written, and meritorious of publication, though some doubts remain about the approach, and effect of aerosols on radiation fields. The paper is generally not placed well in context with recent literature from India, that in part seems to call for a need to reassess the sensitivity studies to bind uncertainty in radiation fields. Other then that, the paper is well written, and well within the scope of ACP.

Specific Comments:

1) In Northern India the vertical layering of the atmosphere is anything but simple (Babu

et al, 2011; Sateesh et al., 2011). These and other measurements have shown evidence for strong layering of aerosols. By March/April aerosols (and NO2?) reach well above altitudes currently assessed in the paper, and up to 9km. How valid is the approximation of geometric AMF under such radiation conditions?

2) Was the MAX-DOAS used to test AOD assumptions? If so, this is not clear. Is the S/N of the instrument sufficient to use O4 dSCDs to assess radiation fields in a meaningful way?

3) The authors measure other gases, i.e., O4, H2O, (Glyoxal?), but do not discuss these measurements further. Could the authors extend Figure 2 to show all the fitted cross-section results, and RMS. Also, some discussion about the variability of these gases in context of error bounds would be useful to illustrate the potential of the technique. What is known, what could be learned?

4) AOD at 550nm is given as in the range of 0.2-0.5 in one place, and as 0.1 'during afternoons' in another place. Where is which information coming from, and which information applies when, and where? Also, an AOD at 550nm (from MODIS, not available during mornings) is a lower limit for the AOD in the wavelength range used for the spectral retrievals. A higher AOD applies for the discussion of uncertainty, but it remains somewhat unclear what is the basis for estimating the error due to radiative transfer as 20%. For the wavelength dependence of AOD over another Indian City see Sateesh et al., 2011.

5) Can the authors add a literature search, or support from their own data, why the assumptions 'aerosols < 2km' and 'NO2 < 1km' are indeed meaningful assumptions for their radiation fields. Based on Figure 3, and taking AOD as 0.5 as the first point shown on this rather extended AOD scale (why up to 3? – this does not appear to connect with the paper), it seems that the error due to azimuth effects alone could be twice as large as the specified error for elevated NO2 and aerosol layers. In particular, with measurements as described here, the azimuth angle continually varies as the

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result of movements of the sun and the pointing of the sensor (inherent to the 'closed routes' that follow circles). Was any attempt made to account for effects due to the changing azimuth? In any event, an expansion of their sensitivity tests towards higher aerosol and NO2 distributions seems desirable.

6) Can the authors test the hypothesis whether aerosols and trace gas are collocated? If not, the sensitivity study indicates that an error of 20% could arise alone from the mismatch between aerosol and trace gas vertical profiles that has been tested here over a rather limited parameter space. Higher aerosol and NO2 distributions are likely to increase this error estimate. Some more interpretative discussion of Figure 3, applicable conditions, and resulting uncertainty would be useful.

7) In Figure 5, error bars to indicate absolute uncertainties are missing. The Figure as is creates the wrong impression that the last case study is actually more accurate than previous, while the opposite is the case. Maybe adding two error bars per case study could be a way to illustrate the (rather small) error due to 'data gaps', but it should not be shown without also showing explicit context in terms of the overall uncertainty.

8) Figure 11, Section 4: Uncertain aerosol optical properties, i.e., single scattering albedo, have been shown to strongly affect satellite retrievals at UV wavelengths (up to factor 3!), and add to underestimates of NO2 VCDs from space (Dix et al., 2009). The effect is likely not negligible also for NO2, yet discussion about aerosol optical properties is currently missing in this manuscript. Section 4 should include a brief summary of the assumptions made by OMI retrievals (NO2, aerosol vertical distributions, collocation, aerosol optical properties) and discussion should connect with sensitivity tests in Figure 3. A very relevant parameter in this context is single scattering albedo (Barnard et al., 2008; Dix et al., 2009), yet discussion is currently missing in justifying the geometric approximation.

Literature:

Babu S. Suresh; Moorthy K. Krishna; Manchanda Ravi K.; et al. Free tropospheric

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Dix, B.; Barnard, J. C.; Volkamer, R.; 'Implications of the In-Situ Measured Mass Absorption Cross Section of Organic Aerosols in Mexico City on the Atmospheric Energy Balance, Satellite Retrievals, and Photochemistry' in CURRENT PROBLEMS IN ATMOSPHERIC RADIATION (IRS 2008): Proceedings of the International Radiation Symposium (IRC/IAMAS). AIP Conference Proceedings, Volume 1100, pp. 161-164 (2009). doi:10.1063/1.3116938

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