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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 #1

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The paper presents Multi-Axis-Differential Optical Absorption Spectroscopy (MAX-DOAS) measurements and derived NOx emission estimates in Delhi and nearby regions. The measurements were made on three days during April 2010 and one day in January 2011. Using simultaneous wind speed and direction data and assuming a constant ratio of NOx and NO2 in the polluted layer, the authors estimated the total NOx emission rate in the considered region. It is found that emission estimates on different days were similar within about 30 percent. The estimation was preceded by an attempt to justify the so called geometrical approximation of the air mass factor. The emission estimates for areas encircled by the measurements were up-scaled to the greater Delhi area and then compared with data of independent emission inventories. Additionally,

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Interactive Discussion
Discussion Paper

the paper presents the comparison of MAX-DOAS measurements with tropospheric NO2 vertical column densities (VCDs) retrieved from OMI measurements. The spatial correlation between these data was found to be rather high, but it was found also that OMI VCDs are systematically smaller than those from the MAX-DOAS measurements.

I noticed the two following potential major results of this study which could present substantial interest for the atmospheric scientific community and could deserve publication in ACP:

1. Measurement based estimates of NOx emissions in one of the world's largest urban agglomeration;

2. Justification of the geometrical approximation of the air mass factor.

Unfortunately, neither of these points is sufficiently elaborated, and further work is needed before the paper can be recommended for publication in ACP.

Major concerns:

1. The method to derive NOx emissions from the MAX DOAS NO2 VCDs involves a pair of factors (c_L and c_tau) defining the assumed NOx to NO2 ratio in the polluted layer and the ratio of the measured NOx and the originally emitted NOx. The authors assume a constant value of 1.32 for c_L with an uncertainty of about 10 percent. However, they note also (p.19188, l. 19-25) that "especially close to strong emission sources, part of the emitted NO might not be quickly converted to NO2 if the NO mixing ratios locally exceed those of O3". Thus the authors seem to recognize that the assumed uncertainty in c_L, and, consequently, the reported uncertainty in the NOx emission estimates may be wrong. It makes me wonder about actual usefulness of one of the major results of this paper. In my understanding, this study provided a rather credible estimate for the lowest limit of the NOx emissions from Delhi on the considered days, while the upper limit is essentially not constrained by the measurements. Therefore I believe that the authors should either clearly spec-

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ify that they report only the lowest limit of the NOx emissions, or provide additional facts (for example, air pollution monitoring data or urban-scale modeling results, see, e.g. http://urbanemissions.info/model-tools/sim-air/delhi-india.html) which would allow constraining their emission estimates from above.

2. The emission estimates derived from MAX DOAS measurements are compared with the estimates of annually averaged emissions from the EDGAR inventory and the study by Gurjar et al. The authors made a warning about inconsistency of the temporal scales, but unfortunately they did provide any hints about possible differences in emission estimates which may be associated with such inconsistency. I recommend the authors to review available information and literature about possible seasonal variability of NOx emissions in India or similar region (e.g., Mexico). In particular, regional emission inventories, as well as seasonal variations specified in different chemistry transport models could be considered and discussed.

3. The authors estimate possible deviations of true NO2 VCDs from those obtained with the geometrical approximation using a radiative transfer model for various scenarios. The results show that the uncertainties increase with increasing the NO2 layer height, and the authors assume that the NO2 layer height does not exceed 500 m. This is, in my opinion, not a credible assumption: I would expect that NO2 is well mixed within the whole boundary layer, and that the boundary layer height for the considered conditions (at, least, in April) probably exceeded 2000 m. The authors should provide the range of the boundary layer heights (e.g., from the ECMWF analyses used in the study) during the measurements and to show results of sensitivity tests covering this range. In addition, the deviations should be assessed for the range of possible values of parameters of particle size distribution and the single scattering albedo (SSA). The assumptions regarding SSA and the size distribution should be explained and justified.

4. The authors say (p. 19184, l.15-18) that “the retrieval result represents the difference of the SCDs ... and the Fraunhofer regference spectrum” (SCD_Fraunhofer) as defined by Eq. (1). This statement is confusing because just in a few lines below it is

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said that SCD_Fraunhofer “has to be added” (presumably to DSCD_alfa). Therefore, SCD_Fraunhofer is simply cancelled from Eq. (1), and it is puzzling that it was introduced in the discussion. Actually, the formulations in the preceding study by Wagner et al. (2010) look a bit different. In particular, Wagner et al. suggest deriving VCDs from DSCD_alfa (which is a difference of SCD measured at two angles), rather than directly from SCD_alfa as in this study, and these quantities are obviously not identical. This discrepancy should be explained.

5. The authors use wind speed data only for 3 lowest levels (0, 30 and 60 m above ground). I guess that the wind speed can be larger at upper altitudes within the boundary layer, and thus the pollution can be transported much more rapidly from the boundary layer over Dehli than it is assumed in the study, leading to an underestimation of emissions. This issue should be clarified, and corresponding uncertainties should be taken into account.

6. Validation of OMI data looks “off- topic” in regards to the main goal of the study (estimation of emissions). There are a number of publications on this subject, but the authors did not even made an attempt to compare their findings with results of other studies. I recommend that either the comparison with OMI data is completely removed from the paper (scientific contribution of this comparison is anyway not clear), or the OMI data are used to get independent NOx emission estimates in the same way as MAX-DOAS data. For me, the secon option is preferrable, and I expect that the second option will not require much of additional work.

Minor comments:

Abstract, l. 12, “the absolute values show a reasonably good agreement”. This statement contradicts to the next sentence and should be either revised or removed.

p. 19180: l. 24: Million -> million

p. 19181, l. 7-12: a logical link is missing between the two statements

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ibid, l. 9: to understand -> for understanding

p. 19182, l.3: confusing usage of the word “superior”

p. 19183, l. 2,3: the word “pointing” is used two times in the same sentence. Please consider revising this sentence.

p. 19185, l. 13: “tropsopheric” -> tropospheric

p. 19190, l. 14: Megacities -> megacities

p. 19191, l. 12, “similar patterns”: this statement is at least not evident.

p. 19191, l. 26, 27: please provide the average values of OMI and MAX DOAS VCDs together with their uncertainties.

p. 19192, l. 9-11: it is not exactly proven in this study that the unknown true VCDs are underestimated by OMI. I would modify the statement by saying “VCDs are LIKELY systematically underestimated by OMI”, or it would be even better to talk simply about differences.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19179, 2011.