Atmos. Chem. Phys. Discuss., 9, S2284–S2288, 2009
www.atmos-chem-phys-discuss.net/9/S2284/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of urban NO_2 concentrations and their diurnal and seasonal variations observed from space (SCIAMACHY and OMI sensors) using in situ measurements in Israeli cities" *by* K. F. Boersma et al.

K. F. Boersma et al.

Received and published: 26 May 2009

We thank Reviewer 2 for his/her thorough comments that we address below:

GENERAL COMMENTS: The authors present a concise study of NO2 variability in Israel, using ground and satellite measurements. As such it is useful for validation of the SCIAMACHY and OMI instruments. It also provides qualitative support for model predictions about tropospheric NO2 amounts over a region with a unique weekly cycle.

The paper is well-organized and well written. However, I have reservations about the derivation of the column measurements from the ground data, along with a few lesser

concerns. If these can be fully addressed, I'd recommend publication in ACP.

SPECIFIC COMMENTS: (1) The biggest issue I have regards the conversion of surface measurements to columns. Martin et al [2002] do correctly point out that most of NO2 over land is in the BL, and the assumption of a well mixed BL is probably reasonable. However, the amount of NO2 above the BL is not negligible. A quick calculation with a model (eg GEOS-Chem) NO2 profile for Israel, using the BL depths given in your paper indicate that on the order of 1/3 (sometimes more) of the tropospheric NO2 column lies above the BL. Neglect of this may or may not qualitatively change the surface-derived seasonal and diurnal effects in this study, but it certainly would affect the comparisons to satellite data.

We have addressed this point by calculating the above-boundary layer NO2 column over Israel with GEOS-Chem. As also stated in response to Reviewer 1, These above-boundary layer columns range from 0.1x1015 molecules cm-2 in winter months to 0.4x1015 molecules cm-2 in summer, presumably from the stronger lightning NOx source in that season. For the urban regions studied here, this implies on average a <8% free tropospheric fraction in Summer and >2% free tropospheric fraction in Winter, small enough to be neglected. If we account for GEOS-Chem free tropospheric columns in our validation study, the correlation and slope remain unchanged, and the intercept decreases slightly: from -0.4x1015 molecules cm-2 to -0.7x1015 molecules cm-2. We have updated the third and fourth paragraphs of section 3 accordingly.

(2) The method used to derive the corrections to the surface measurements is different than that of Lamsal et al. [2008], and the amount of NOz interference inferred in the present study seems smaller than that of Lamsal et al., although it is hard to estimate from the information given. It would be useful to know the magnitudes of your corrections, any seasonal dependence they might have and why they might be different from those of earlier studies.

Our numbers are indeed smaller than in Lamsal et al. [2008], who present observed

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and simulated correction factors for the rural site of Taenikon, Switzerland. For our comparison, using only measurements in urban region, we follow Dunlea et al. [2007] who focused on an urban area and found little evidence for interference at mid-morning and inferred a correction based on observed ozone levels. We evaluated this approach with CHIMERE simulations of NO2:NOz rations (or correction factors) over Europe, and found (1) correction factors in the 0.9-1.0 range for all seasons (CHIMERE, CHIMERE with Dunlea-approach, and observed with Dunlea-approach in Israeli cities, and (2) good agreement between simulations of correction factors with CHIMERE and observed correction factors at Taenikon, providing confidence in the ability of CHIMERE to simulate NOx-chemistry. We now include this discussion and provide the magnitudes of the corrections in the first paragraph of section 3. Please also see our response to Reviewer 1. Detailed visualizations of the CHIMERE results can be found at www.knmi.nl/ boersma/publications/papers/interference.pdf.

(3) In the Ensemble Validation section, does a data point (ie, one of the n=542) consist of exactly one satellite pixel and one surface measurement? Are any pixel centers ever within the threshold distance of more than one city?

All 542 OMI pixels uniquely fulfil the criterion that they were observed within 0.1 deg of a station location. Sometimes two different OMI pixels matched up with the same surface measurement, as both fulfilled the 0.1 deg coincidence criterion. An OMI pixel never coincided with more than one surface station.

(4) Satellite data for a large (off-nadir) pixel would not be representative of a surface measurement at a point. Since you imply this as a reason to exclude the cities of Haifa and Ashod (near strong sources), please be explicit about what sizes of satellite pixels are being excluded and discuss the implications.

OMI pixels with viewing angles <35 deg (at 32 degrees N) have pixel sizes smaller than 34 x 14 km2, and any pixels larger than that are excluded. We have now added this information in section 3 and in the caption of Table 1, and state explicitly that our

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ensemble validation holds for small pixels. An additional advantage of using viewing zenith angles <35 deg in the remainder of this study (diurnal cycle) is that SCIAMACHY and OMI pixels have been observed under similar close-to-nadir conditions.

(5) The caption for Table 1 states that pixels were restricted to viewing angles within 35 deg of nadir.I don't understand how this ensures satellite and surface measurements coincide within 15 min, as stated in the table caption.

Pixels with a viewing zenith angle <35 deg have centers within 4.5 degrees longitude from nadir pixel centers. Pixels with vza<35 deg are thus within (4.5/15)x90 = 27 minutes solar time from the nadir pixels that have been observed at approximately 13:40 hrs local time. We have corrected our statement in the caption of Table 1 accordingly.

TECHNICAL CORRECTIONS: P 4308, L 11 reword: "(0.30 x 1015 and 0.59 x 1015 molecules cm-2, respectively)"

Corrected.

P 4309, L 11 reword: "Figure 2 compares tropospheric NO2 columns..."

Corrected.

P 4309, L 13 reword: "As seen in the left panel, BL NO2 columns show similar correlation..."

Done.

P 4309, L 18 reword: "...a slightly lower negative intercept..."

This has been removed in line with addressing some earlier comments.

P 4309, L 21-22 "r=0.53" does not match "r=0.54" in the Fig 2 caption

Corrected.

P 4309, L 22-25 Please be more specific in identifying the five stations e.g. you could add in parenthesis "(all except Haifa, Ashod and Afula)"

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Done.

p 4311, L 5 By "Israeli stations" do you mean all except Afula?

Except Ashdod, Haifa, and Afula. Corrected.

P 4311, L 18-19 reword: for example, "...did not show the same diurnal cycle..." This seems a less ambiguous way to phrase it, since you later present evidence for a slight increase from SCIA to OMI in winter.

Done.

P 4312, L 10 reword: "The top panel of Fig. 7 shows..."

Done.

P 4312, L 14 reword: "...as shown in the bottom panel of Fig. 7..."

Done.

p 4314, L 9 slope=0.93 0.07 does not match 0.06 on p 4309 L 15.

Corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4301, 2009.

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