Interactive comments on "Comparisons of ground-based tropospheric NO2 MAX-DOAS measurements to satellite observations with the aid of an air quality model over Thessaloniki area, Greece" by T. Drosoglou et al.

General comments:

This paper presents a comparison of satellite observations to ground based tropospheric NO2 MAX-DOAS measurements performed in 3 different types of conditions (urban, suburban and rural). A high resolution air quality model is used to try to assess the difference in spatial resolutions between the OMI satellite data and the MAXDOAS.

The paper is well written and the comparison is of interest of the community, however some of the data are not exploited/commented enough and I recommend the publication after the suggested revisions.

One of the major pity is that the air quality model is used only for the OMI results and not for the GOME-2 data. Moreover, the air quality model is used with the shortcoming of a typical OMI nadir pixels centered around each location (p.10, line 7 to 13) instead of the actual pixel size and location. If additional simulations are unavailable to improve the study, the variability of the NO2 content in space should be estimated in some way (maybe from the NOx emissions) and discussion of the spatial and temporal variability should be enhanced. For GOME-2 comparisons, the fact that the pixels would cover the whole domain of Fig. 1 (p. 7, line 33) can be used to test how the average variability seen by 3 typical stations would compare to the averaged variability seen from satellite. Moreover, the differences between GOME-2A and GOME-2B comparisons results are never mentioned, while Metop-A is in a reduced swath mode since 7/2013 with a pixel half the size of the nominal mode of Metop-B. This should be mentioned and commented (figure 7, table 3 and 4).

The OMI and GOME-2 tropospheric NO2 algorithms are only mentioned in Section 2.3, without any detailed description, and conclusions are thrown as if they were consistent among them, while we know that large differences in tropospheric NO2 exist between different product (applied on a same instrument) due to different algorithm hypothesis. The 2 algorithms should be described in Section 2.3 (maybe with a table of main differences) and impact of the algorithm hypothesis should be mentioned/quantified. Comparison e.g. of the model NO2 profiles at the 3 stations type wrt to the profiles used as a-priori in the satellites retrievals would be interesting.

Specific comments and technical corrections

Page 1, line 9: consider changing "the main" into "one of the"

Page 1, line 13 (and in the conclusions): why using DOAS/MAX-DOAS while in the rest of the document only "MAX-DOAS" is used?

Page 1, line 18: consider reformulating "mainly due to the higher spatial resolution of OMI" to also mention the different products.

Page 2, line 22: consider changing "is limited… respectively" to "are respectively limited by the …"

Page 3, line 10: "are applied to satellite data …" → Only applied to OMI data!

Page 4, line 18: inconsistency between "a fixed azimuth angle of 255º" and "at azimuth angles of 80° relative to the solar azimuth". Reformulate.

Page 4, line 21: add reference for “only at the elevation angles of 15º and 30º in order to avoid uncertainties introduced due to aerosol loadings at lower elevation angles” and explain what is done with the VCD retrieved at the 2 elevation angles. Is the average value used? Or is a filtering related to the difference of the 2 VCD values used (as in Brinksma et al., 2008)?

Page 5, line 7: illustrate the shape of the used “mean vertical profiles from the air quality modelling tool consisting of the photochemical grid model CAMx and the mesoscale weather prediction system WRF” (ideally also showing how they differ from the 2 satellite NO2 algorithms a-priori profiles)

Page 5, line 13: the 0.07 and 0.01 difference in AOD between the sites should be referred to the mean value. (0.07 wrt to 0.1 is large while it is small if compared to 1).
Why scaling the LIDAR profile to AOD values (line 14), if the “AOD loading and AOD profile between the three locations is very small” (line 12-13)? How much the choice of this aerosols a-priori profile affects the MAXDOAS NO2 VCD? Give an estimation of the MAXDOAS error.
Maybe add a subplot on figure 6 with the time-series of the Thessaloniki CIMEL AOD to have an idea of the variability?

Page 5, line 18: it’s Fig 4 and not 3. Line 21 it’s Fig 2 and not 4.

Page 5, line 24-25: I would put this sentence at the end of the section.

Section 2.3: add a description of the 2 NO2 algorithms maybe with an overview table) and references to papers (at least Valks et al., 2011 and Bucsela et al., 2013) and some other validation results (here on when discussing the results).

Page 6, line 17: “a particular direction and path” – how long is the representativeness area of the MAXDOAS? Give references (at least irie et al., 2011) and estimation ranges. In page 7, line 32, an area of 2x2km² is considered representative of the MAXDOAS data, but when using high elevation angles as 15° and 30°, and considering no aerosols, longer distances are obtained. How much the choice of using only one model cell of 2x2km² is affecting the adjustment factor?

Page 6, line 19: “the satellite data are adjusted” → only OMI data are adjusted.

Page 6, line 25 to page 7 line 24: a lot of details are given for the model data used here, which is too much compared to the lack of description of the satellite data in Sect 2.3. The goal of the papier is focusing on the satellite data!

Page 7, line 25: again, only OMI data are adjusted.

Page 7, line 32: see comment above about the impact of using only one model cell of 2x2km² is affecting the adjustment factor.

Page 7, line 33: “the method was applied only to OMI data because for GOME-2 the sub-satellite pixel is very large, covering typically the entire domain of Fig. 1” – see general comment: how the GOME-2 data would compare to an average of the 3 stations?

Page 8, line 14 and line 19: again, what columns are used in this study (in the figures, tables, etcc)? VCD at 15° or at 30°? Or something else?

Page 8, line 26: the “upper limit for the distance”: does this mean that only the closest pixel within this distance is used for the satellite data? Or an average of the pixels values within this distance is used? How much choosing 50km for both instruments would affect the comparison?

Page 8, line 29: typo: “table 3 and 4”

Page 9, line 6: “can be attributed mainly to its smaller pixel size” and smaller distance criterium?
“to its higher sensitivity in the boundary layer compared to GOME-2”: give a reference.
The fact that the 2 algorithms are different should also be mentioned and commented.

Page 9, line 11: “The use of actual satellite geometry for each day is complex and would require a much larger domain for the air-quality simulations, more than double the currently used (120x120 km2) in order to include all possible pixel sizes and positions for each location. However, such simulations were not available for this study.” → This is really a pity. The study would be much more realist…

Page 9, line 19: again, it would be nice to have a figure with the NO2 model profile at the 3 sites.

Section 4: no discussion to previous validation results is included, neither in Section 3 or 4. This should be added. Discussion of the difference in GOME2A and GOME2B results should also be added. Again importance of the OMI and GOME2 algorithmic differences should be mentioned.

Page 18, table 2: mention that the statistics are for tropospheric NO2. From which angle (12 or 30°)?

Page 18, table 3: discuss the larger differences in GOME2A wrt GOME2B mean values in RC sites and smaller in SC and UC (not consistent with the pixel size influence, as GOME2A is twice as small than GOME2B). A figure with location of the satellite pixels position/size could be useful.

Page 19, table 4: add slope and intercept values to this table. Again, discuss GOME2A wrt GOME2B differences.

Page 19, table 5: considering adding a line with values over 2 or more cells in the MAXDOAS pointing direction (related to comment on the MAXDOAS representativeness).

Page 20, figure 1: add a ruler to estimate distances.

Page 23, caption of figure 4: “and azimuth angle 100º relative to the sun for “ → in the text (page 4, line 19) it’s 80° !

Page 24, figure 5: it would be good to add the locations of the 3 stations also on the 1rst panel, and add a ruler to estimate distances.

Page 25, figure 6: why not including the period where the 3 instruments measured at the same site on the figure?

Page 26, figure 7: use the same axis limit for the urban scatter plot for the 3 instruments.

References:

Irie, H., Takashima, H., Kanaya, Y., Boersma, K. F., Gast, L., Wittrock, F., Brunner, D., Zhou, Y. and Van Roozendael, M.: Eight-component retrievals from ground-based MAX-DOAS observations, Atmos. Meas. Tech., 4(1), 1027–1044, doi:10.5194/amtd-4-639-2011, 2011.

Bucsela, E. J., Krotkov, N. A., Celarier, E. A., Lamsal, L. N., Swartz, W. H., Bhartia, P. K., Boersma, K. F., Veefkind, J. P., Gleason, J. F., and Pickering, K. E.: A new stratospheric and tropospheric NO2 retrieval algorithm for nadir-viewing satellite instruments: applications to OMI, Atmos. Meas. Tech., 6, 2607–2626.

Valks, P., Pinardi, G., Richter, A., Lambert, J.-C., Hao, N., Loyola, D., Van Roozendael, M. and Emmadi, S.: Operational total and tropospheric NO2 column retrieval for GOME-2, Atmos. Meas. Tech., 4, 1491–1514, doi:doi:10.5194/amt-4-1491-2011, 2011.