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Atmospheric Measurement Techniques Discussions

Interactive comment on "Observation of slant column NO_2 using the super-zoom mode of AURA-OMI" *by* L. C. Valin et al.

Anonymous Referee #2

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In their manuscript "Observation of slant column NO2 using the super-zoom mode of AURA-OMI", L. Valin et al report on NO2 slant column retrievals on super zoom measurements of the OMI instrument. The spatial resolution of the measurements is assessed by comparison with MODIS observations and good agreement is found with results from on-ground calibration of the OMI instrument. The NO2 data are then compared for several scenes with the results obtained using the standard resolution of OMI data and an average over several standard overpasses of the same location. Higher spatial resolution is evident in the super-zoom NO2 fields as expected; both when compared to the individual overpass standard resolution and the average picture. As an example application, the NO2 lifetime in a plume is estimated using data from one scene taken over Dubai and reasonable values are found.

The topic of the paper – high spatial resolution satellite observations of tropospheric pollution – is interesting and in view of current plans for future satellite instruments very relevant and within the scope of AMT. The paper is well written but lacks details in some places (see below).

My main problem with the paper is that while (at least to my knowledge) it reports on the first analysis of OMI super-zoom measurements, there is not much a reader can learn from this study. The retrieval technique is standard DOAS, the determination of spatial resolution confirms known instrument parameters, and the reported gain in spatial resolution is what one would expect if averaging is disabled. Probably the most interesting question is the uncertainty of the super-zoom observations, and this point needs to be discussed in more detail to be useful.

Major Comment

The error discussion in the paper is not yet sufficient.

So far, a fitting error is quoted but it is not clear, how it was computed – does it include the uncertainties from the radiances and irradiances or is it based on the residuals only?

Then, a detection limit is mentioned (page 1994, l 23) and again it is not clear how it was determined – in my opinion, this should be linked to the random noise of the slant columns as for example determined over a clean oceanic region as shown in Boersma et al., (2004).

In the uncertainty discussion (page 1995), the change in fitting error from super-zoom to standard resolution fits is reported and the deviation from the expected square root dependence is attributed to the temperature dependence of the NO2 cross-section. I don't think this can explain the observations. If it were the case, values in unpolluted regions where the temperature of the NO2 x-section used is appropriate should show the expected dependence on the number of measurements averaged. I assume that this is not the case and that the systematic residuals which cannot be reduced by averaging have another origin. It would be very interesting to see an example of an NO2 fit in both, super-zoom and normal mode.

In my opinion the paper needs to give good estimates of what the random noise of the NO2 values in super-zoom mode are for background and polluted situations in order to give the reader an idea of what type of signals one can detect at this resolution.

Minor Comments

1) The paragraph on the retrieval needs rewording and more details – it is not clear, whether the fit is done on the ratio of radiance and irradiance or on the logarithm and it is not clear what a "linear" term for the correction of rotational Raman scattering is. Also, some of the wording is strange – why is a third order polynomial "varying slowly", and why are the reflectance spectra fitted to the cross-sections and not the other way round?

2) Also, it should be noted that water vapour has considerable absorption structures in the wavelength region selected for analysis, and therefore the H2O x-section is included in the current operational OMI NO2 data (see http://toms.gsfc.nasa.gov/omi/no2/OMNO2_readme.pdf). While this might not be a problem over deserts, it certainly has to be corrected for applications to a global data set.

3) Where does the estimate for the stratospheric column (page 1996, line 9) come from?

4) It might be worthwhile to mention that measurements with even higher spatial resolution have been performed from aircrafts as reported in Heue et al., Atmos. Chem. Phys., 8, 6707–6717, 2008. These data also can give an indication on the spatial scales needed to fully resolve NO2 plumes from point sources.

5) For individual overpasses, striping is an important problem in OMI data. The authors

correct this for their Dubai case but not for the other figures – why?

6) The application shown for the estimation of the NO2 lifetime is in my opinion questionable as it is not clear which of the NO2 is coming from the source, what the role of dilution is and how the uncertainty on the measurements translates to uncertainty of the NO2 lifetime. At the very least, it has to be shown which OMI pixels were used in the estimation by indicating them in Fig. 4, and an x-y plot should illustrate the NO2 decay as function of distance (time).

7) In Fig. 1, please use the same region in the plots of MODIS and OMI reflectivity. Also, please use a colour scale that does not saturate so that the reader can better compare the two figures. In panel c) of this figure, it would be good to indicate the individual points. How exactly have the MODIS data been gridded to the OMI data?

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 1989, 2011.