Review of “Reconciling the differences between OMI-based and EPA AQS in situ NO2 trends”

After reviewing the responses by the authors and the revised manuscript, I do not recommend the manuscript for a publication to AMT in the present form. The responses to the reviews are not serious. There is no single analysis to support their responses.

The NO_2 concentrations measured at the surface monitors can be substantially different for the sites very close (e.g., 500 m). The size of OMI swath is 24 km x 13 km at the finest resolution and is often larger than this. In addition to differences in the spatial resolution, there are uncertainties in the satellite NO_2 retrievals and surface measurements. The trends of OMI tropospheric NO_2 columns and those of NO_2 measured at surface monitor can be similar as shown in the previous publications. But reconciling the differences between OMI and EPA AQS NO_2 trends for large regions can not be a measure for improvement of OMI NO2 retrievals and their trends.

Overall, the manuscript needs a major revision if the authors would like to publish it at AMT. I list my suggestions below.

(1) Authors need to make their focus clear. OMI data co-located with the AQS are mainly discussed through the manuscript, but all of sudden the trends of OMI NO_2 for large regions are emphasized (e.g., abstract line 27-31). The comparison of the trends of OMI NO_2 for large regions with the trends from the AQS does not make sense. It is confusing if the authors mention the OMI trends at the AQS or the OMI trends for large regions such as West, Midwest etc for all the figures and the text.

(2) Please make careful statements based on clear or enough proofs. The authors added “However, the current OMI tropospheric NO2 retrievals are not designed for analyzing multi-year tropospheric NO2 trends”. I do not understand what it actually means. Does it mean the previous publications on OMI NO_2 trends are not correct?

(3) The ocean trends do not look significant as another colleague mentioned. Albedo correction based on MODIS data looks promising as Russell et al. (2011) demonstrated. Lightning filtering also gives new insights for southern US. It is important to show spatial variability of the trends or NO_2 columns from adopting MODIS albedo and lightning filtering similar to Figure 8. Detailed spatial distribution rather than the simple values for 4 large districts would be useful. Add explanations of why the impact of lightning filtering is large for the Northeast US (not only the South US, see Figure 1).

(4) Referring to Lamsal et al. (2015), the authors only mentioned average values. Lamsal et al. (2015) also stated that the impact of changing anthropogenic emissions in calculating a priori profiles can be large up to 15% more reductions in the declining trend depending on the location. Lamsal et al. used 1 degree x 1.25 degree GMI model grid resolution to produce trace gas profiles. The authors have the REAM model setting with the 36 km resolution and are capable of producing own profiles rather than fully depending on discussions in the previous publication. I am not convinced with lines 28-29, page 4, “The NO_2 VCD trend analysis is particularly sensitive to the first two factors and we will discuss these in the following sections”.

(5) Discrepancies between NO_2 chemiluminescence to photolytic converter measurement are small in the morning (7-9 am) and become larger in the afternoon. The plots of diurnal variations of the ratios at each station would give a confidence in the quality of the measurements.

(6) I do not understand the meaning of Figure 9. Is this for the apple-orange comparison of the trends from the AQS and those from OMI data for the large areas?