Review of “Reconciling the differences between OMI-based and EPA AQS in situ NO_2 trends” by Zhang et al.

This manuscript summarizes the sensitivity of OMI NO_2 trend to several factors such as a baseline trend (over the ocean), surface albedo, and lightning filter. I found the information in the manuscript is useful. The paper is well organized and presentations are neat. But I think that general conclusions (or contents in the abstract) are misleading and some important analyses are missing. I suggest to revise the manuscript before final publication based on the comments below.

First, I do not agree with the authors in the abstract line 14-15 (“how to improve OMI NO_2 retrievals for more reliable trend analysis”) and line 23-25 (“we recommend future studies to apply these procedures to ensure the quality of satellite based NO_2 trend analysis, especially in regions without reliable long-term in situ NO_2 measurements”). I think the agreement between the trends in surface monitored data (AQS) and those in standard OMI (Table 1) is already good, considering uncertainties in the satellite retrievals and the spatial coverage of the surface monitors. The authors need to clarify the spatial resolution of the OMI data (used in the trend analysis) and the spatial extent (or representativeness) of the ground-based observations. It is exciting to see better agreement between the trends from AQS and the final OMI (lightning filter) in Table 1. But I am not sure that these two should agree exactly. Figure 4 shows that the effects of different OMI retrievals are not clear except DJF in Midwest and Northeast. If summertime or typical ozone season satellite data are used for the trend study, it is not worth trying different retrievals or corrections suggested in this study.

In general, the manuscript reports the impact of uncertainties of satellite tropospheric NO_2 retrievals on the trend analysis. This is a sensitivity test study that provides useful information and can be a good reference to summarize the uncertainties in the OMI NO2 based trend analysis. However, I do not think it has broad and substantial impacts to change or shape future research.

One thing missing is a validation of *a priori* model NO_2 profile or near surface NO_2. According to this study, NO_2 columns (potentially NO_x emissions) decrease by ~40% for 10 years. How does satellite NO_2 column retrieval change if *a priori* profiles come from the model results incorporating this reduced NO_x emission (e.g., 40% reduction to 70% reduction considering a potential error in the emission).

Final comment is to elaborate the correction of NO_2 measurements by molybdenum converter. The plot in supplementary material (Figure S1) can include more details for the 7 sites. The diurnal variation in each season (if not month) and standard deviation in the plot will be helpful to characterize the ratios between surface NO_2 concentrations of chemiluminescence to photolytic instruments. The plots for each site (7 sites) will be useful for readers.