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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of model spatial resolution on the interpretation of satellite NO_2 observations" *by* L. C. Valin et al.

Anonymous Referee #2

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In this manuscript, the authors addressed a point that the dilution of the model NO2 caused by the use of coarse horizontal resolutions can either increase or decrease the model OH radical mixing ratio depending on the chemical regime, which in turn can results in either negative or positive biases in the simulated NO2 columns, respectively. This important aspect of the model NO2 bias has not been accounted for in the past studies that have used both atmospheric chemistry models and satellite retrieval data. The manuscript is original and is well organized. Before the publication, however, the authors need to clarify several points.

It is clear in the manuscript that the impact of the model resolution on the simulated NO2 columns is important for intermediate and small sources: coarse model resolution can cause positive biases in NO2 columns. In these chemical regimes, the model

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dilution decreases NO2 and then decreases OH mixing ratios leading to the increase of the chemical lifetime of NO2 against OH.

In the manuscript, it is suggested that for large source of NO2, the dilution in coarse resolution model can increase the simulated OH leading to decrease of chemical lifetime of NO2 and therefore negative bias in the simulated NO2 columns. Because the model NO2 decreases due to dilution, it is not clear that the lifetime of NO2 can be decreased significantly. Note that the chemical lifetime of NO2 defined in the paper is a function of both NO2 and OH. In this sense, it is difficult to understand the reason why % deviation VCD in Figure 3 is so large for large source with 256 and 512 km resolutions. In addition to this, large differences in the magnitude of % deviation VCD between the 1-D and the 2-D model results (Figures 3 and 5) are not understandable.

I think the principle (Figure 1 in the manuscript) on which the paper is based is valid. But it is difficult for me to make connections among the 1-D, the 2-D, and the WRF-Chem model results quantitatively. I recommend the authors to provide clear linkage among 1-D, 2-D, and the WRF-Chem model results, based on the emission (kmol/hr) and the dilution factor (the area over which the emissions are released or model horizontal resolution). For the 1-D model, page 20247, lines 25-26, “emission rates of 30, 3.0, and 0.30 kmol h-1 at resolutions of 0.5 to 512 km” means the highest emission rate of 60 (=30/(0.5*1)) kmol h-1 km-2. In this case, the dimensions perpendicular to the flow were fixed at 1km. For the 2-D model, page 20249, lines 9-10, “a point source (2x2 km2) with emission rates of 300, 30, and 3 kmol h-1” means the highest emission rate of 75 (=300/(2*2)) kmol h-1 km-2. Why are the responses to various resolutions so different for the 1-D and the 2-D models although the emission rates are similar for the two models?

For WRF-Chem run for Four Corners and San Juan power plants, the finest resolution in the EPA National Emission Inventory is 4 km x 4 km. Please be specific if the dilutions in the emissions are the same for the two simulations with 4 km x 4km and 1 km x 1km resolutions. The direction of plume transport in 24 km x 24 km resolution run is quite

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different from those in other resolutions (Figure 7). I recommend the authors to check if the simulations are conducted correctly for 24 km x 24 km resolution run. Regarding the sampling box, are the results sensitive to the definition of the box? Looking at the columns in 12 km x 12km resolution run (Figure 7), the plumes extend out of the limits defined. I have the same question for the Los Angeles box. For San Joaquin Valley, many grids look like being affected by intermediate source of NO2 (green color), but the simulations with coarse resolutions do not show positive bias in the NO2 columns. More analyses of connecting the WRF-Chem model results to the 1-D or the 2-D model are necessary.

Comparison of the model NO2 columns with satellite NO2 columns over Four Corners and San Juan power plants can provide useful insights since the model simulations in this region are based on the emission inventory that included the measured NOx emissions from these power plants.

One missing part in the manuscript is the impact of volatile organic compounds (VOC) including isoprene on the level of OH and then NO2 columns. To interpret regional-model results covering various urban and power plant areas, insights on the role of VOC would be very helpful.

In general the basic ideas in the manuscript are excellent. However, the conclusions on the model resolutions required for minimizing the resolution-dependent NO2 bias should be made with caution because the model results can be sensitive to the chemical mechanisms used, the methods of OH calculation and the errors in the emission inventory and the model simulations cover only limited regions and periods. In practice, the conclusions in the manuscript may not be applied to other regions.

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