Atmos. Chem. Phys. Discuss., 15, C6501–C6504, 2015
www.atmos-chem-phys-discuss.net/15/C6501/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone and carbon monoxide over India during the summer monsoon: regional emissions and transport" *by* N. Ojha et al.

Anonymous Referee #2

Received and published: 2 September 2015

This paper evaluates ozone and carbon monoxide predicted by the regional model, WRF-Chem, over India. There are limited data of atmospheric constituents above the surface. Thus, the focus on vertical profiles compared to CARIBIC data is a needed addition to understanding atmospheric chemistry predictions in South Asia. The authors find that the CO mixing ratios are typically underestimated in the lower troposphere and therefore investigate the cause of the underprediction. They conclude that direct emissions of CO are not responsible for the CO mixing ratio underprediction, but instead long-range transport is substantially underestimated. The paper is well written, but further discussion is needed about how this study's results compare with previous work and on the implications of the results. I recommend minor revision before publication

Major Comments:

1. There needs to be more discussion about what previous studies have found and how the results of this paper fits with those previous studies.

2. There needs to be discussion of the implications of the results (e.g. section 4.3.1). In addition, the Conclusions section should discuss the implications of the results.

Specific Comments:

1. For the model evaluation, it is clear that the WRF-Chem output is compared along the CARIBIC flight tracks. Then the model results are compared to MOPITT CO profiles over Chennai. The authors use MOPITT gridded data for this comparison. How big is the MOPITT grid? Is it similar in size to the WRF-Chem grid, or is it a larger (smaller) region? Is the MOPITT profile over a region similar in size to the CARIBIC vertical profiles?

2. For all model comparisons with observations, have the authors only compared the model interpolated (or nearest grid point) with the observation, leaving the possibility that a plume could be missed because the wind direction was slightly different than observations? In some cases, it helps to capture those plumes by using an average of the 9 surrounding grid cells in the model.

3. Page 21146, lines 1-6. The high ozone north of India is likely due to the high elevation of the region as well. There are other data sets that could and should be used by the authors to evaluate the model results, including ozonesondes and CAIPEEX data (as reviewer 1 mentions). For example for the northern part of the domain, Bian et al. ADVANCES IN ATMOSPHERIC SCIENCES Volume: 28 Issue: 6 Pages: 1318-1325 Published: NOV 2011 present ozonesonde data over Tibet, China. I recommend incorporating these data into the model evaluation.

4. Page 21146, lines 6-8. Would removal of ozone precursors by wet deposition possibly be important for the results shown here?

5. Page 21146, lines 13-16. This would be a good place to discuss how the model

results compare to previous modeling studies, e.g. Kumar et al. (2012) and Michael et al. (2014).

6. Section 4.3 Introduction. I know of two other papers discussing the importance of boundary conditions on regional-scale model results. Pfister et al. (2013) JGR discuss the contribution of the boundary conditions on ozone over California. Andersson et al. (2015) GMDD show the importance of boundary conditions on results in the regional MATCH model. These papers should be included in the introduction and/or discussion of long-range transport. Andersson, E., Kahnert, M., and Devasthale, A.: Evaluation of lateral boundary conditions in a regional chemical transport model, Geosci. Model Dev. Discuss., 8, 5763-5808, doi:10.5194/gmdd-8-5763-2015, 2015.

7. Page 21148, lines 12-14. In addition to introducing Figure 9, I suggest that the authors also present basic results, e.g. "for all days except one, the residence time in South India is less than a day". Otherwise I suggest introducing Figure 9 at the start of section 4.3.1.

8. Page 21149, end of section 4.3.1. I think section 4.3.1 omits the implications of the findings, which are that the high pollution event came from air masses that spent >3 days over South Asia and are influenced by boundary layer pollution. Please address these questions:

a) Why did WRF-Chem not reproduce this event? It could be the winds were in error (likely too high), or emissions of non-methane hydrocarbons or nitrogen oxides were too low (higher emissions of other ozone precursors were not tested in the high CO emissions sensitivity test).

b) Have other pollution events, like the one described, been observed before (i.e. cite previous studies)? At the least, the authors could point out that pollution events occur during stagnation events and this is an example for South India.

9. Section 4.3.2 would benefit by discussing previous studies on boundary conditions

for regional-scale model simulations.

10. Section 4.3.2. MOZART includes biomass-burning emissions, yet is shown to have an underestimate of CO mixing ratios in a region potentially affected by African biomass burning. Could the authors recommend improvements for the global chemistry transport model? I would think improving the fire emissions (which is an ongoing activity) and having data assimilation should help.

Technical Comments:

1. P. 21138, line 17, → data are

2. Page 21146, lines 6-8. What wet deposition scheme is used? It was not listed in Table 1.

3. Page 21146, line 8. → affected

4. Page 21147, line 9. Are the results presented in Figure 7 from one grid column in WRF-Chem or an average of a few grid columns that cover the Chennai region or interpolated to the latitude/longitude of Chennai?

5. Page 21148, line 22. → O3 and CO are found

6. Page 21148, line 22. Clarify whether O3 and CO are very high in concentration or whether their high concentrations reach up to "high" altitudes (805 hPa).

7. Page 21148, lines 23-24. I think it would help the discussion to refer to the vertical profiles in Figures 1-3.

8. Page 21149, line 10. → mixing ratios by 25%

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 21133, 2015.