Comment: 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 under-prediction. They conclude that direct emissions of CO are not responsible for the CO mixing ratio under-prediction, 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.

Response: We thank the referee for constructive comments and suggestions. The point-by-point response to referee comments are given below and changes made in the manuscript are in red color.

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.

Response: More discussion about previous studies has been added in the revised manuscript (Page:4, Lines:1-6). We in particular fill a gap of WRF-Chem evaluation in free troposphere against aircraft data of ozone and CO during monsoon (Page:5, Lines:1-3). We additionally show that the evaluation results are different as compared to satellite retrievals.

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.

Response: Thanks for the suggestion. Implications of the results and recommendations are added (Page:21, Lines:8-13). Conclusions section has also been revised to include new analyses and implications (Page 23,lines:8-13).

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?

Response: MOPITT grid size is bigger (1°x1°) as compared to WRF-Chem (30 km x 30 km). We agree that the best comparison between model simulations and satellite observations can be made when the two datasets have same spatial and temporal resolutions. Here we suggest that such effects could be minimal as "monthly average" distribution from model and satellite has been taken for the comparison, which could minimize the effects of day to day variations in the comparison. A synchronization of the spatial resolution using interpolations could induce further errors, especially in the cloudy conditions affecting many grid points during monsoon.

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.

Response: The suggested method of averaging gives the similar results as shown in the manuscript, since we have interpolated the model output "bi-linearly in space" which accounts for the possible spatial variability. The differences in model output time and observations have been accounted for by taking temporally weighted average (more weight to the model output which is closer in time to observational time).

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.
Response: CAIPEEX data was requested, however is not yet received and therefore will be analyzed as a follow up work. Bian et al paper uses primarily satellite data. As suggested, we made a comparison with ozonesonde data, which shows a good agreement between model and ozonesonde climatology during summer monsoon in the lower and middle troposphere over Delhi and Pune. However, model overestimated ozone in the upper troposphere. Over southern Indian site Thiruvanathapuram, model showed a negative bias in the troposphere, as also seen with the CARIBIC observations. This information has been added in the manuscript (new Figure 5, Section-3.2, and Section-4.1.2).

4. Page 21146, lines 6-8. Would removal of ozone precursors by wet deposition possibly be important for the results shown here?
Response: Yes, removal of ozone precursors by wet deposition would affect ozone, especially near surface, where in situ chemistry is more important. The sentences are suitably modified in the manuscript. However above surface, O_3 and CO profiles are little affected by wet deposition (e. g. Bela et al., 2013; http://www2.mmm.ucar.edu/wrf/users/workshops/WS2013/ppts/7A.5.pdf).

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).
Response: Suggestion is incorporated.

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.

Response: Suggestion is incorporated (Page:5, Lines:2-7)

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.
Response: Suggestion is incorporated.

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).
Response: We thank the referee for the suggestion. We have analyzed the wind fields over Chennai based on radiosonde measurements (Supplementary material, Figure 3, 4). We found that the model systematically overestimated wind speed during the July period, in particular when observations show lower wind speeds (2-3 m/s) causing favorable conditions for stagnation. Therefore we expect that the air parcel do not collect enough pollutants from the boundary layer. Additionally, no indication of underestimation of emissions is seen as model performance did not improve in reproducing the event, when emissions were increased by 50 %. This discussion has been added in the manuscript (Page:19, Lines:12-21).

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.

Response: As mentioned before, analysis with radiosondes measurements, this is exactly the case: the pollution events are due to stagnation episodes which are not captured in the model due to higher lateral wind speeds. We have discussed this now in the manuscript (Page:19, Lines:12-21; Supplementary Material: Figure 3, 4).

9. Section 4.3.2 would benefit by discussing previous studies on boundary condition for regional-scale model simulations.

Response: Suggestion is incorporated (Page:19, Lines:23-26).

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.
Response: Suggestion is incorporated (Page:21, Lines:8-13).

Technical Comments:
1. P. 21138, line 17,→ data are
Response: Corrected.

2. Page 21146, lines 6-8. What wet deposition scheme is used? It was not listed in Table 1.
Response: A simple parameterization of wet scavenging in convective updrafts was included. Simulation with full wet deposition (including aquous-phase reactions) was also conducted (Supplementary material- Figure 5) showing very similar results. Newly developed wet deposition (Neu-Prather scheme) was not available with the employed model chemistry (RADM2). Evaluation results using Neu-Prather scheme (Bela et al., 2013) have also shown insignificant effects of wet deposition on O3 and CO profiles above surface.

3. Page 21146, line 8.→ affected
Response: Corrected

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?

Response: This figure shows model simulation bi-linearly interpolated to the site.

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

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).
Response: We referred to the mixing ratios and not to the altitude. Sentence corrected in the manuscript.

7. Page 21148, lines 23-24. I think it would help the discussion to refer to the vertical profiles in Figures 1-3.
Response: Figures of vertical profiles are now referred (Page: 19, lines: 1-2).

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