Comment: The paper by Ojha et al., presents an analysis of the tropospheric composition and potential pollution sources over the lower troposphere over India. The authors use a combination of in-situ aircraft observations from CARIBIC, MOPITT-CO data and WRF-CHEM simulations. Since the model underestimates lower tropospheric CO observations of CARIBIC, but captures the free tropospheric data the authors perform sensitivity simulations with different source estimates. Based on these they conclude on a minor effect of the local emissions on the free troposphere over India. Using HYSPLIT backward trajectories they conclude on an underestimated transport path of CO from Africa in the lower troposphere. They test this by increasing CO at the according WRF domain boundary, from which the authors infer a better agreement with the in-situ observations.

The focus of the work is unclear to me: Currently the only conclusion, which can be drawn is, that WRF does not reproduce in-situ observations of CO, even with increased Indian emissions. The additional source from long range transport is not fully evaluated.

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 highlighted in red color. The objective of the study is to evaluate the performance of the online regional chemistry transport model (WRF-Chem) over India during summer monsoon. This is the first work evaluating WRF-Chem simulations with state-of-the-art aircraft-borne instrumentation for ozone and carbon monoxide over India. We use this evaluation to identify the weaknesses and strengths of the model setup in simulating the influences from regional emissions and transport. The effects of transport from within the Indian domain is shown to be well captured by the regional model (Figure 3 and Figure 11). The role of regional emissions is investigated using sensitivity simulation 1.5x_EM. Thanks to the suggestions of the other referees, we test it further using another regional inventory (INTEX-B) to find that our conclusions are consistent (independent of regional inventory used). Role of long-range transport is analyzed combining backward trajectories with sensitivity simulation (1.25x_BDY).

A comparison of vertical profiles of CO from driving global model (boundary conditions data to WRF Chem) with CARIBIC measurements is shown (Supplementary material- Figure 1). This clearly shows that CO levels are lower (by ~20 ppb or more) in transported air mass from west during June-August) at 800 hPa. Easterly air masses (influencing above 500 hPa) do not underestimate CO which is in agreement with our conclusions. In addition to evaluation of WRF-Chem simulations against in situ profiles, we also show a comparison of monthly average satellite data. We highlight that the evaluation results are different with satellite data as compared to in situ measurements. Therefore we suggested that follow up studies using more aircraft based observations are needed to validate the model as well as satellite retrievals to better understand budget of trace gases, especially due to transport as regional effects are reasonably represented in current setup .

Now we revise introduction and conclusion sections to clarify these objectives and implications of the study in the manuscript itself.

Comment 2: There's no discussion
- of the role of other emissions (SE China) and vertical transport there
- of transport outside the domain in the potential inflow region above 4 km (see trajectories Fig.7)

Response: In the easterly air masses, influencing the region above 4 km, CO mixing ratios are typically overestimated in driving global model (Supplementary material- Figure 1). Therefore the CO underestimation in WRF-Chem in lower troposphere could not be due to emissions over the SE China region.

- of the structure of the tropospheric CO profiles even after 'improvement' (Fig. 10, 1.25_BDY_case): CO is enhanced over the whole profile, but the model does not cap ture the vertical CO structure - the whole distribution seems to be wrong.

Response: The typical underestimation in the lower troposphere (westerly air mass) and overestimation in the upper troposphere (easterly air mass) does lead to a limitation of regional model in capturing the observational profiles. Global model (boundary conditions) also shows limitation in reproducing the observed structure. Here, We suggest that a regional model which better captures the local and regional effects can be combined with improved /adjusted boundary conditions data from global models. Improvement in global emission inventories and data assimilation to provide boundary conditions to regional model is recommended. However, in lack of in situ profiles in the surrounding marine regions, as of now such efforts are sparse. Our study partially fills this gap by combining sensitivity simulations with CARIBIC observations and backward trajectories.

Comment 3: Further:
- MOPITT and model disagree for the monthly means with WRF partly overestimating the observations, how does the hypothetical African source affect this?

Response: Here we are convinced that the MOPITT data have strong discrepancies with the in-situ observations. We now make more analysis to find that the model profiles after applying the MOPITT operator (Std (AK)) converge towards the satellite a priori profiles, especially in the lower troposphere. The averaging kernel values are found to be much smaller (less than 0.1 in LT) during this period (summer monsoon), as compared to the values reported in literature (e. g. Kar et al., 2008). This indicates biases in the satellite retrievals towards a priori from model (similar to what is used as boundary conditions in WRF-Chem) (also see response to the comment 10).

Manuscript has been suitably revised by adding this discussion (Figure 6, Page: 15, Lines:3-7).

Comment 4: the simulation fails to reproduce the C-shape seen in MOPITT which could indicate, that vertical transport in the model is incorrect (which would be in agreement with the 1.5_EM experiment)

Response: We do not agree. The limitation in reproducing the C-shape is not due to incorrect vertical transport in WRF—Chem and the observed shape is

not captured in the model fields used for boundary conditions (Supplementary material-Figure 1). Regarding the comparison with MOPITT, the complete CO profile from WRF-Chem is in very good agreement with MOPITT in June, indicating reasonable vertical transport in the model. During July-September also, model has significantly higher CO than MOPITT in the altitude range where we are reporting an underestimation as compared to measurements. New analysis comparing the vertical distribution of water vapor from WRF-Chem with in situ measurements from CARIBIC (Figure 4 in the revised manuscript) also indicates adequate vertical transport in the model. The contrasting evaluation results with in situ profile and satellite data clearly indicates the uncertainties in retrievals in cloudy-rainy conditions of monsoon period.

Comment 5: Is it appropriate to use CO (with its relatively long lifetime) to evaluate a regional model over several months, without carefully evaluating the driving model, which delivers the chemical boundary conditions? I highly recommend to either include an analysis of the pollution sources of the driving model and a larger area, if CO is taken as evaluation species. Alternatively the authors should use the full capability of CARIBIC data (short lived compounds), which are suitable to evaluate sources in a regional domain. Given the above points, the paper requires major revisions.
Response: We agree with the referee that the manuscript can be largely improved in this sense. Therefore the chemical field from the driving model providing boundary conditions has also been evaluated same way by bi-linearly interpolating along the CARIBIC flight and comparing with the observations (Supplementary material Figure 1). Boundary conditions data does show an underestimation near 800 hPa. Ground based observations at the site were not available but in the nearby region, CO is not underestimated at surface. Also any increase in surface emissions has large impact on surface (Figure 10), therefore we suggested that CO in the boundary conditions need to be increased in westerly air. We do agree and wanted to convey that this study will be followed more by future air craft campaigns on the oceanic regions around south Asia to provide further insight on fire influences. A recent study (Osman et al., ACP Discussions, 2015) also indicated higher CO mixing ratios on the western boundary of our domain using trajectory-mapping of global aircraft observations. The manuscript has been revised suitably to include these information (Page:21, Lines:8-13).

Comment 6: Details: The hypothesis that the uncertainties of local emissions alone cannot explain the discrepancies to in-situ observations are build on very few in-situ profiles. On the other hand the authors show, that MOPITT profiles and WRF simulations agree or even the observations are overestimated. Nonetheless, they conclude on a south westerly CO source by increasing part of the model boundary CO by 25%. The authors infer, that transport of CO from the south west is underestimated and they increase the CO over a specific part of the domain boundary by 25% (but the whole altitude range) and compare this with individual local profiles. How is it possible, that the increase at the domain boundary over the

entire altitude range affects CO over Chennai, since the authors show, that HYSPLITT back trajectories indicate a totally different air mass origin above 4 km ? Given this behavior: did the authors check, how an increase at e.g. the opposite model boundary would affect the comparisons (not the flow at higher altitudes in Fig.7)?
Response: Easterly air masses have higher CO in MOZART than observations indicating that sources of the underestimation in LT are in the west. Our explanation of westerly influence will be consistent with WRF-Chem and observation comparison as well as MOZART-observation comparison.

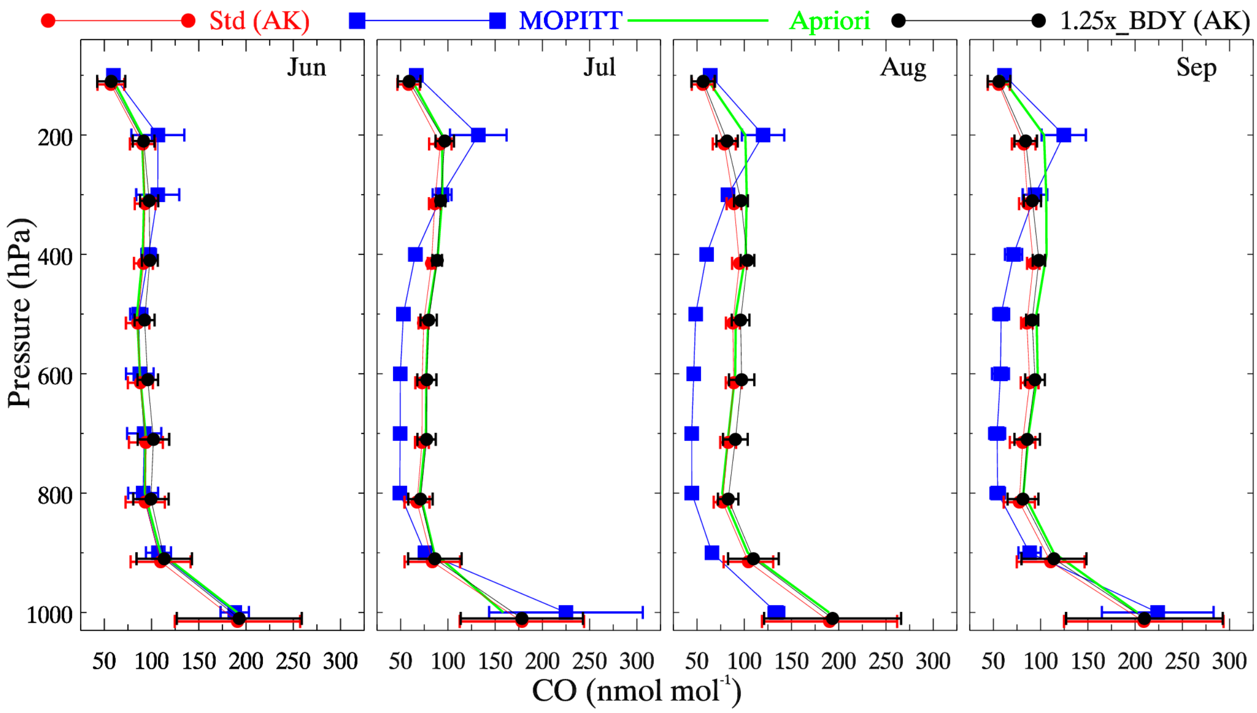
Comment 7: How well is convection (shallow and deep) represented in the model?
Response: Convection in the model has been parameterized using Grell 3D scheme, which is the improved version of GD scheme, combining the ensemble and data assimilation techniques (Grell and Devenyi, GRL, 2002). In the revised manuscript, we show (new Figure 4) that the water vapor profiles in WRF-Chem are in very good agreement with the in situ measurements from CARIBIC, indicating an adequate convective transport in the model.

Comment 8: Why do the authors not use a global model to support their conclusion about a significant African influence?
Response: During Indian summer monsoon period, the CO distribution in global model fields does show highest CO mixing ratios over the central African region associated with biomass burning (e.g. Inness et al., 2013). We combined sensitivity simulations with backward trajectories to suggest significantly higher inflow of CO from the western domain boundary to regional model domain. Analysis of global model data used as boundary condition is also given here showing the underprediction of CO in westerly air mass. Dedicated work towards conducting and evaluating global model simulations is beyond the scope of this paper (which focuses on regional emissions, synoptics-scale transport from within India and identifying uncertainties in the inflow from model boundary conditions of the regional model). A paper on the suggestion has been recently submitted (personal communication).

Comment 9: Is WRF capable of simulating the correct local diurnal variations of the low level winds, which might have a strong impact on the comparison with individual CARIBIC in-situ profiles?
Response: Horizontal winds in the model are nudged with the NCEP reanalysis data. Continuous observations of surface winds were not available and model simulated wind speed variations were compared with radiosonde observations at different pressure levels in the boundary layer. Wind speed in model simulations generally reproduced the observations well (e. g. r= ~0.9 in August), except the occasions of low wind speeds (2-3 m/s) before stagnation event. This is discussed in the revised manuscript (Page 19, Line:12-21; Supplementary material: Figure3,4). It could be noted that large under-prediction of CO is seen during August despite of the fact that wind speeds are very well reproduced.

Comment 10: How does the BDY1.25 case affect the monthly mean WRF-MOPITT inter-comparison?

Response: Model profiles (Std as well as 1.25x_BDY) show a tendency to converge towards satellite a-priori during monsoon, as shown below. This is now discussed in the manuscript (Page:15, Lines:3-6, revised Figure 6).



References

Grell, G. A., and D. Dévényi (2002), A generalized approach to parameterizing convection combining ensemble and data assimilation techniques, Geophys. Res. Lett., 29(14), doi:10.1029/2002GL015311.

Inness, A. et al. (2013), The MACC reanalysis: an 8 yr data set of atmospheric composition, Atmos. Chem. Phys., 13, 4073-4109, doi:10.5194/acp-13-4073-2013.

Kar, J., et al. (2008), Measurement of low-altitude CO over the Indian subcontinent by MOPITT, J. Geophys. Res., 113, D16307, doi:10.1029/2007JD009362.

Osman, M. et al. (2015), Carbon monoxide climatology derived from the trajectory mapping of global MOZAIC-IAGOS data, Atmos. Chem. Phys. Discuss., 15, 29871-29937, doi:10.5194/acpd-15-29871-2015.