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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 #3

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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.

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)

- 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 capture the vertical CO structure - the whole distribution seems to be wrong.

Further:

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

- 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)

- 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.

ACPD

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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 backtrajectories indicate a totally different air mass origin above 4 km ? Given this behaviour: 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)?

How well is convection (shallow and deep) represented in the model?

Why do the authors not use a global model to support their conclusion about a significant African influence?

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?

How does the BDY1.25 case affect the monthly mean WRF-MOPITT intercomparison?

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