Interactive comments on "Model simulated trend of surface carbon monoxide for the 2001–2010 decade"

The manuscript presents 10-year trend analysis of simulated surface CO by an atmospheric chemistry transport model ECHAM5/MESSy (EMAC). The authors used monthly MOPITT CO to evaluated the model performance and suggested that mode simulations with RG scenarios agrees well with global MOPITT-retrieved surface CO. Based on the 10 years data and simulations, the authors looked into the trends of surface CO based on model simulation and the ground observations from WDCGG. They also discussed the possible controlling factors for these trends. In general, I found the main points and the structure of this manuscript are clear. But some of discussions are weak and unclear. Below are my comments for making the manuscript more concise. I recommended accepting the manuscript for publication but with revisions.

Main comments:

1: Trend analysis: Fig 9, 10, 11, I suggest that the authors should add MOPITT data and do a cross comparison among model, station observation and MOPITT.

2: MOPITT data: According to MOPITT science team, "the new joint (multispectral) TIR/NIR products, featuring the maximum sensitivity to near-surface CO are fundamentally much more capable of characterizing surface-level CO than either purely TIR- or NIR-based products". The authors may want to compare the model simulations with TIR/NIR joint product since the manuscript is looking into the trend near surface.

3: The sensitivity run with constant emissions: The purpose of CE simulation is highlighting the influence of emission/transport on the trend and spatial distribution of surface CO. Therefore, I don't think it is necessary to discuss the comparison between CE simulation and MOPITT data during the evaluation section (e.g. Fig 4). Instead, it is more important to evaluate the model during different burning seasons. Therefore, I suggest the author removing CE simulation in Fig4 and add another season comparison (e.g. September, the SH biomass burning season).

4: Trend in East Asian: the surface CO trends are positive but not significant, while the emission is negatively significant. And Worden et al 2013 showed a negative trend in MOPIT tropospheric column CO. The authors argue this is influenced by transport or chemistry. From Fig9, the transport actually produced a negative trend over EC. So the authors should remove the transport in Line 13 and discuss more about other reasons with evidences for the positive trend over EC at surface.

Detailed comments:

P12410 Line 12: Western Europe, Eastern USA, and Northern Australia. Should be de-capitalized for “western, eastern, and northern”. Please make same changes in the rest of manuscript for the similar situation.

P12410, line 16: remove “significant” or change into another word

P12410, line 22, define Medium-lived.

P12411, line 3: remove in unpolluted and non-forested locations. Change the CO-OH and the lifetime of CO description like: The main sink of CO is oxidation by OH and results in a ~2-month mean lifetime. Because of this relatively short lifetime, CO is not well-mixed in the troposphere

P12411, line 7: replace finally with therefore.

P12411, line 15: Using GEOS-Chem model, Liu et al. 2010 (Analysis of CO in the tropical troposphere using Aura satellite data and the GEOS-Chem model: insights into transport characteristics of the GEOS meteorological product) looked into the interannual variation of tropical tropospheric CO in 2005 and 2006. In her 2013 paper, she looked into the IAV of tropical CO in UTLS during the Aura period also with GEOS-Chem model.

P12411 line18: remove “allow scientists and researchers to”

P12411 line 19: remove the 2$^{nd}$ global

P12411 line 20: The main purpose of this paragraph is showing the limitation of using satellite data or ground station solely to explain the CO trends. The authors should not just list all the satellite names. The authors should provide more detailed discussion of using satellite data (here MOPITT) to evaluate model simulation and to do the IAV and trend analysis.

Another point is: the last sentence of this paragraph seems indicating that in the rest of paper, the authors will combine the satellite data and ground based data. So the introducing of model in the next paragraph seems unexpected. Please make sure the logical is smooth.

P12412 line8: remove in contrast.

P12412 line10: change into available ground stations

P12413 line 13: specify the vertical resolution.

P12414 line 12: change into urban megacities.

P12415 line8: remove finally

P12416 line 16: Change into"It is quite challenging to retrieve tropospheric CO profiles based on mostly passive remote sensing instruments (including MOPITT) because ..

P12417 line19: add "in December 2008".

It is obvious that PG simulation agree better than CE simulation with the observations. So remove the comparison to CE simulation and corresponding discussion and Fig 5. Also in Fig 4 add another burning season (e.g. September) and corresponding discussion of spatial distribution of surface CO. The authors should also consider adding one panel showing the difference between model and MOPITT in these two months.

P12418 line 7: Pacific

P12418 line 10: Not clear and please explain this sentence. "the failings to consider significant influences of natural sources (e.g. effects of the El Niño on tropospheric CO, Chandra et al., 2009) in the EMAC model"

P12418 line 15: WDCGG-archived data (Xt)

P12418 line 22: what does (t/12) stands for?

P12419 line 22: for the correlation between only significant trends, r increased to 0.7, but n drops to 7 (Fig 7). What is the p-value for this correlation? Does the model significantly capture the trends in observations?

P12419 Line 10: make n and N consistent in the equation.

P12419 line 10: I am confused with the method to calculate the standard deviation of the trends. The standard way of doing this is first calculate the autocorrelation coefficients, then infer the effective degree of freedom then calculate the standard deviation of the trends. Line 11: I guess in equation (5) $\sigma_N$ is the standard deviation of the x time series. If so, please correct in your definition.

P12420 line 15: wrong reference. Novelli et al 2003 was examine the effect of 1997-1998 fire on tropospheric CO. They didn't mention the perturbation of Pinatubo. Furthermore, the influence of Pinatubo should only last for a few years (its effect on stratospheric and tropospheric ozone lasted until 1994). How should this contribute to the decrease trend of CO from 1991 to 2001?

P12421 Line 27: keep the unit of trends uniform @@y-1?

P12422 line2: replace tendencies into trends

P12424 Line11: for east China, the surface CO trends is positive but not significant, while the emission is negatively significant. And Worden et al 2013 showed a negative trend in MOPIT tropospheric column CO. The authors argue this is influenced by transport or chemistry. From Fig9, the transport actually produced a negative trend over EC. So the authors should remove the transport in Line 13 and discuss more about other reasons caused the positive trend over EC at surface. The authors could put the discussion in the conclusion part, since the summary and conclusion part is quite short and nothing new in this section.

Tables and Figures:

Table 3: please clarify the the definition of mean and corresponding statistcs. For example, for PAR mean MOPITT CO is 91.78+-33.49. My understanding is 33.49 is the 1 or 2 $\sigma$ of the mean CO. But $\sigma$ in the table is 7.32+-5.28.

Fig 2 and Fig 12: Is there any specific reason of using bar plots for emission time series? It is better to change them into line plots.

Fig4: I suggest removing CE results and adding a 3$^{rd}$ panel for difference. Also adding another set of results in SH burning season (e.g. September).

Fig5: Suggesting removing Fig5. But what are the blue and yellow color?

Fig11: It is hard to see the trends. I suggest putting all the line plots into two horizontal panels.