

## General Comments:

The manuscript entitled 'Model simulated trend of surface carbon monoxide for the 2001-2011 decade' by Yoon and Pozzer presents CO results determined from an atmospheric chemistry transport model and compares them to distributions and trends extracted from surface measurements and the MOPITT surface product. The forward model uses the RCP.85 anthropogenic emissions with GFED V. 3.1 biomass burning to simulate CO distributions from 2001 to 2011.

This manuscript first compares model results from simulations using decadal constant emissions to results using the similar base conditions but including the RCP and GFED time varying emissions. Regional mean CO concentrations using the time-varying sources are found to be more similar to MOPITT than constant emissions. This section could be shortened.

The time varying anthropogenic and biomass burning emissions were used with chemistry to derive a more realistic model response. Model CO distributions give reasonably good agreement with the MOPITT surface product, although there are very large aggregation errors in both data sets. Decadal trends from the model are compared to those determined from a number surface sites, again with good agreement. In addition to model/surface site comparisons in the manuscript, I suggest the model also be compared to trends in the MOPITT products.

There is a brief discussion on trends in OH, CO and NO<sub>x</sub> trends from the model and the influence of NO<sub>x</sub> on OH. This section is relatively weak and could be removed. Some conclusions, such as the observed decreases in concentration over Europe and the US are due to decreased anthropogenic emissions, are not new. But in general I found the work interesting and with revision would be suitable for publication in ACP. Below I give some comments the authors should consider.

## Specific Comments:

P. 12410, line 13. It should be noted that the downward trends observed in the 1990s have been attributed primarily to decreases in anthropogenic emissions (Duncan et al. 2007, Novelli et al., 2003, among others).

P. 12410-12411. All the satellites which measure CO need not be mentioned. It would be better to say the MOPITT results are used here because of their rigorous evaluation/ validation and their 13 year continuous record. The pros and cons of using satellite retrievals to validate models and to estimate long-term trends should be given.

P. 12414, Section 2.3. This section would benefit from more detail on the MOPITT surface product, including the pressure levels that define the surface product, its precision and any bias.

Section 3.1, P. 12415-12416. I found it hard to follow how the model data were transformed to reflect the pressure and a priori constraints of the MOPITT retrieval. Is there something missing in equation 1? This section needs to be written more clearly (e.g. see Deeter et al., JGR, 2010).

P. 12416, lines 8-21, Figures 4, 5 and 6. These figures all depict the correlation of the model and MOPITT CO. I think the Taylor diagram (Figure 6) contains the most information and the other 2 can be removed. The authors should briefly say why this type of diagram is useful (e.g. Taylor, JGR, 2001).

P. 12417, Table 3. The mean decadal model results for the constant source and time varying source runs are compared. Mean CO over the Eastern USA and Western Europe are greater in the time varying scenario even though emissions have decreased. In Figure 12 a decadal decrease trend is shown for most regions. Transport from Central South America (the only region showing a strong increase in emissions) seems unlikely. Shouldn't have mean CO decreased?

P. 12416, line 23. Is 'resume' the correct word?

P. 12418, Table 4. Would the authors comment on why the model gives statistically significant trends at about twice as many sites as the measurements.

P. 12418-12419, Figures 7, 8. A majority of the trends determined from the WDCGG surface data and the model (Table 4) fall in the range of 0-20 ppb decade<sup>-1</sup>. The high statistical agreement between model and measured trends in Figures 7 and 8 appears to be driven by a few locations. Is this the case? Are there commonalities among the sites falling outside of the cluster?

P. 12420, Figure 9a. Why do the model results with constant emissions show decreasing trends in the Southern Pacific, Indian and Southern Oceans?

P. 12421, line 9. The oxidation of non-methane hydrocarbons should be included as a major source of CO (Duncan et al., JGR, 2007 and references therein).

P. 12421, Figure 12. GFED 3.1 reports SEAS CO emissions from fires in 2010 were the greatest for the decade but high emissions in Asia are not shown in Figure 12. Would the authors comment on this.

P. 12422, Section 4.2, lines 5-29. The trends discussed here are given in four or five different units. Can they be normalized to % change per decade? The trends from MOPITT and AIRS instrument come from Worden et al. 2013 and represent the total column. I would like this work to compare model trends with trends calculated from the MOPITT surface product. The model and MOPITT could also be compared in the mid and upper troposphere.

Pp. 12422-12423, Figure 13. The changes in CO from the model are compared its emissions. I don't think it is surprising they are similar. This rather long discussion could be shortened. Perhaps say that regional-scale model trends generally reflect trends in the emissions, except for Eastern China. Then examine China more closely.

P. 12423, lines 9-14. Worden et al., 2013 report a strong decrease in MOPITT column CO over E. China during the 2000s. Emissions from Eastern China in the model show a marginal decrease with time however the model results show an increase. The authors suggest this surprising result may come from transport or secondary chemical production. The results from the constant emission model run (Figure 3) don't seem to support transport. The possibility that chemical production from hydrocarbons are

referenced to Tohjima et al., 2014 and Anglebratt et al., 2011, but neither of these papers quantitatively examine how reasonable changes in VOCs would effect CO trends. This manuscript should look into the E Asia emissions/surface changes in more detail.

P. 12423, Section 5. From model calculated trends of OH, CO and NO<sub>x</sub>, the authors conclude OH trends are largely controlled by NO<sub>x</sub>. The discussion would benefit from a description of CO-OH-CH<sub>4</sub>-NO<sub>x</sub>-O<sub>3</sub> chemistry. (e.g. Tables 1 and 2 in Lelieveld et al., ACP, 2004). The authors should temper this conclusion, e.g. 'These results suggest that more than just the CO trend effects trends in OH'.

The changes in OH and NO<sub>x</sub> shown in Figure 14 are very small and contain large uncertainties. A more robust conclusion would require a multivariate analysis of the important species in the OH cycle.

References: I suggest the number of citations for each reference be limited to no more than 3-4 carefully chosen papers.

Tables and Figures:

Table 2. Note that 'GC-HgO' is the method, RGD is the instrument.

Table 3. Mean MOPITT CO for PAR is reported as  $91.78 \pm 33.4$  with standard deviation of  $7.32 \pm 5.28$ . Are these values the mean and aggregation errors? Please clarify this in the caption

Table 4. As for Table 3. Also add in the caption that  $(\omega/\omega\sigma) > 2$  is significant.

Figures 1a, b. The maps of global mean CO emissions show the general distribution of the fossil fuel and biomass burning emission strengths but say little about changing emissions. Figures showing model emissions by region over time would better serve in discussions of trends.

Figure 3. The names of the regions cannot be read. The dots showing the site locations are hard to see these should all be larger.

Figure 5. What do the yellow, blue and black dots show?

Figure 6. What are the standard deviations normalized to?

Figure 8. There are 4-5 sites which fall outside of the general cluster. Which these are they? Perhaps they can be labelled as in Figures 13 and 14 or defined in the figure caption.

Figure 9. The site symbols should be larger.

Figure 10. The colored panels are not very useful. The trend data are given in several other places. These figures could be removed.