

Interactive comment on “Interpreting Space-Based Trends in Carbon Monoxide” by Sarah A. Strode et al.

Anonymous Referee #2

Received and published: 14 April 2016

The authors have used a series of chemistry-climate and chemical transport model simulations to understand the negative trends in CO observed by MOPITT. They find that the negative trend in the bottom-up inventories reproduce the trend observed over North America and Europe, but is incapable of capturing the negative trend observed over China. They attributed the discrepancies between the modeled and observed trend in CO over China to changes in the MOPITT vertical sensitivity and to biases in the modeled ozone abundances, which produces a bias in modeled OH and, thus, CO. The paper is well written and the authors did a careful analysis of the trends in the models. I would recommend publication in ACP after the authors have revised the manuscript to address my comments below.

Comments

C1

1) Line 85: The Level 3 MOPITT data are daily or monthly gridded data. Are the authors using the daily gridded data here? Are they using nighttime and daytime, or just daytime data? If they are using Level 3 data, how do they compare the model to the MOPITT data. The model should be sampled at the MOPITT observation locations and times when transforming the model with the MOPITT averaging kernels and a priori profiles. They need to better explain in the paper how this is done.

2) Lines 137 – 141: In giving Equation (1), the authors should explain that the MOPITT retrievals are with respect to the log of the mixing ratio and they should also cite Deeter (2009) “MOPITT (Measurements of Pollution in the Troposphere) Validated Version 4 Product User’s Guide” for providing guidance for calculating the column averaging kernels from the averaging kernels that is with respect to the log of the mixing ratio.

3) Lines 201-203: The authors state here that the discrepancy is “driven largely by the failure of the simulations to capture the 2008 dip,” but the models are also strongly biased in 2010, for example (Fig. 2f). Indeed, this 2010 bias is the focus of the ozone analysis in Figs. 3 and 4.

4) Please state the regional boundaries for the regions considered in Fig. 2.

5) Lines 226 – 244: I find this discussion confusing. I understand how time-dependent variations in the vertical sensitivity of the MOPITT retrievals could contribute to trends in the data. But I don’t understand how, as stated on Lines 228-321, the bias in the modeled CO can produce an artificial trend. It seems to me that there are two possible way this could happen:

a) Are the authors suggesting that changes in the vertical distribution of the model bias, combined with the varying vertical sensitivity of the MOPITT retrievals, produces an artificial trend when the model is convolved with the averaging kernels and a priori profile?

b) If the model bias is constant in time, then the convolved modeled columns should

C2

exhibit the same trend as the data. The presence of a fixed bias in the model together with temporally varying averaging kernels should only impact the trend if the biased model state is so far from the a priori MOPITT state that the linearization assumption in Equation (1) is invalid i.e., the averaging kernels do not accurately capture the sensitivity of the retrieval between the modeled state and the MOPITT a priori. Is this what the authors are trying to say on lines 226-228?

The authors need to explain more clearly how the bias in the model could be contributing to an artificial trend.

6) Lines 261-262: Yes, we expect that anomalies in OH to be inversely related to anomalies in total ozone, however, the OH and ozone anomalies do not seem to be strongly correlated in Fig. 3. It would be helpful if the authors gave the correlation coefficient between the two quantities for different latitude bands in the tropics and extratropical northern hemisphere.

7) Lines 263-264: The ozone column anomaly in Fig. 3 is in the extratropical northern hemisphere, mainly in early (Jan-Mar) 2010. Although the global, annual mean CO lifetime is 1-2 months, in the extratropics in winter it could be longer than a season. If that is the case, it is unclear to me how the changes in OH in early 2010 could drive such large changes in CO between 30-60N in winter.

8) Figure 4: The 2010 ozone anomaly is about 5%. What are the altitude ranges that are contributing to this bias in the column? Are these changes mainly in the UTLS?

Technical comments

1. Line 64: Change “results of (Li and Liu, 2010)” to “results of Li and Liu (2010)”
2. Figure 2: It is difficult to see the seven different lines in each panel. If the authors remove the titles on the y-axes on the panels in the right column and reduce the spacing between the panels, it may be possible to enlarge each panel to make the plots more legible.

C3

3. Figure 3: What are the units for the colorbars?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-87, 2016.

C4