

Interactive comment on “Transport analysis and source attribution of seasonal and interannual variability of CO in the tropical upper troposphere and lower stratosphere” by Junhua Liu et al.

Anonymous Referee #2

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General Comments

Liu et al. present analyses on impacts of surface emissions and transport processes on the observed carbon monoxide (CO) variabilities in the upper troposphere and lower stratosphere (UTLS) obtained from Microwave Limb Sounder (MLS) onboard the Aura spacecraft. GEOS-Chem chemistry transport model driven by the GEOS-4 and GEOS-5 assimilated meteorological fields with GFED2 and GFED3 emission inventories are used to quantify various processes in the model. A specific focus is made on the CO tape recorder in the tropical lower stratosphere with detailed analyses on vertical transport in the model. Overall, the models are capable of reproducing observed CO

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distributions in the tropical UTLS region with some limitations. Below the authors may find specific comments with some suggestions.

Specific Comments

1. Overall, CO simulations from the model have reasonable agreement with MLS but with noticeable disagreement. If GEOS-Chem is more suitable for tropospheric chemistry simulations, the UTLS region might not necessarily be well defined in the model even though it covers the stratosphere. In that case, I would recommend including more details and challenges of using GEOS-Chem in the UTLS region with any cautions we should take in interpreting model results.
2. The observed CO variability in the UTLS is obtained exclusively from MLS. Any known biases or issues of the MLS CO data need to be included for quantitative comparison with the model.
3. Multiple simulations of the model based on different emission inventories (GFED2 and GFED3) and assimilated meteorological fields (GEOS-4 and GEOS-5) are used in this study and the difference between the different model setups is insignificant in some cases. Although this provides more information, it is somewhat unclear what the key findings are. The comparison can be done on a select simulation (for example, GOES-5 with GFED3) and the other details can be included in a separate section when it is necessary.
4. The authors tried to explain the discrepancy between modeled vs. MLS CO with limitations in surface emissions and vertical transport (for example Figs. 2 and 6) in the model. However, no attempt has been made for possible model improvement. For example, if the CO emission over South Africa were underestimated in the model in July, would increasing the model emission solve this problem?
5. When the model reproduces observed variability well, tagged CO run might be helpful to understand detailed contributions from various processes. I am not so convinced

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with usefulness of tagged CO results when there is disagreement between the model simulations and observation.

6. Line 79 - In section 2.1 (satellite data), description of MLS IWC data and the procedure of constructing gridded MLS CO data need to be included.

7. Line 134 - CO observed by MLS (black solid line) and simulated by the model driven by GEOS-4 (blue line) and GEOS-5 using GFED2 emissions (red dashed line), . . .

8. Fig. 4 – I am not convinced what new information Fig. 4 has to add to this article. Also MLS zonal mean CO seems to be noisy as monthly averages.

9. Fig. 5 – I wonder the weaker Asian monsoon maximum is unique to CO or other species (for example, water vapor) in the model. By comparing different species in the model, one should be able to tell if it's related to emission or vertical transport in the model.

10. Line 230 – I do not understand what GEOS-4 anomalies constructed this way mean in Fig. 7.

11. I think isoprene chemistry with CO should be included in the text to be able to understand Fig. 9.

12. Fig. 11 is somewhat duplicated with Fig. 1. It would be better to differentiate those figures to address separate questions.

13. Fig. 13 – Black solid lines, supposedly MLS CO, can not be found in this figure.

14. Line 347 – I am not sure if it is fair to say that ACE failed to capture the semi-annual cycle at 215 hPa. ACE has at least four samples per year so it might be possible to capture it.

15. Line 379 – I wonder if there is a way to simplify this paragraph or even remove it.

16. Line 395 – Instead of 'rising', 'vertically propagating' might be more relevant.

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17. Extensive analyses on vertical velocities related to CO tape recorder are included in this study, which includes big uncertainties by nature. The authors need to address potential errors and uncertainties in their analyses.

18. The conclusion contains so much information and some contents are duplicated in the text. It would be recommended to move discussions about vertical velocity in to different section and reduce the content of it significantly for more clarity.

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