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Interactive comment on "Evaluation of UTLS carbon monoxide simulations in GMI and GEOS-Chem chemical transport models using Aura MLS observations" *by* Lei Huang et al.

Anonymous Referee #2

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This manuscript uses updated retrievals from MLS (v4) to evaluate CO distributions in the UT/LS region as simulated by two global chemical transport models, GMI and GEOS-Chem. The comparisons are thorough and the writing is clear, and the climatologies and time series of MLS v4 data presented are a nice addition to the literature. Overall, I feel like the model sections are underdeveloped: the paper is missing the deep analysis of the differences between the models and the satellite data that would provide a strong advance to scientific understanding in this area. More details and some minor comments follow.

Much of the paper (Sections 3 4) focuses on comparing different features in the observations to the two simulations. However, these comparisons are largely descriptive

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(and often qualitative). Section 5 begins to address the causes for the differences, but doesn't go very far (particularly in terms of inter-model differences). As the authors point out, the two models are very similar because they use the same meteorology and emissions. This means the places where they differ would present a very nice opportunity to understand which processes contribute to the differences, but there is very little discussion of this. For example, why is GEOS-Chem generally higher at 100 hPa but lower at 215 hPa? Is it differences in the convective transport parameterisations in the two models? Differences in chemical production or loss in the UT, or loss in the LS? What else could be driving these differences? This would also require a more thorough accounting of the similarities and differences between the models (e.g. how do chemical schemes differ? How similar are convective parameterisations? etc.) Without this level of analysis, it feels a little like an opportunity to deepen our understanding has been lost.

On a related note, I find Sections 3-4 long and hard to parse. Some sub-sections would help, especially in Section 3. There are a lot of qualitative descriptions of features in the figures, paired with phrases like "[Feature X] in the GMI simulation is more consistent with MLS observations than in the GEOS-Chem simulation" – but these are hard to judge from the figures and often not backed up with quantitative information. In many cases, (e.g. Figs 1-3, possibly 4-11 as well) it would be easier to follow the text descriptions if the figures showed for the model differences plots (e.g., GMI – MLS and GEOS-Chem – MLS) rather than absolute concentration plots. The absolute plots could go into a supplement as the paper is already long and contains many figures. It would also be nice if some of the statements could be quantified using e.g. regional or temporal averages, or even mean difference statistics over all grid squares.

My final major concern is that the paper doesn't reference much recent literature. Of the 11 referenced papers published since 2011, 7 were led by authors from this paper. There is significant newer literature surrounding, for example, injection of trace gases to the UTLS in the Asian monsoon (e.g., Park et al., 2009; Randel et al., 2010; Randel

and Jensen 2013). There is also newer literature on CO distributions, including in the upper troposphere, than the 2006 Shindell work cited here (e.g., Naik et al., 2013; Fisher et al., 2015; Zeng et al., 2015).

Minor Comments (by line)

163: "climatological" CH4 files – are these year-specific, and if not do is a trend imposed?

200-201: Are biogenic emission calculations the same between models? GMI section references Guenther; GEOS-Chem section refers to MEGAN. Theoretically these are the same but the implementation could vary. In general it would be really nice to see what exactly is same vs. different between the models (see above).

224-225: Are MLS averaging kernels and a priori profiles time-varying or constant?

243-244: How do you know trans-Pacific transport from East Asia is weaker in the models? If just judging from the figure, couldn't it just be that the East Asian CO is lower to begin with? Can this be quantified? (e.g. relative difference between East Asia East Pacific?)

323: What does "well captured" mean here? To me it looks like models are quite a lot lower (relative difference would help)

391: "Remaining two" isn't quite right here as only 3 of 6 regions have been discussed so far in this paragraph (no mention of East Asia).

421: Should "less than" be "greater than" here?

453-454: Stating that the MLS IWC and modelled convective mass flux have "good linear correlation" is unsatisfying. Given how importance the simulation of convective transport is for this region of the troposphere, it would be really nice to show this comparison to the reader (perhaps in the supplement), or at least quantify it.

Table 1: Does "tropospheric chemical production" really mean tropospheric chemical

production from NMVOCs, or does this include the CH4 contribution? Please clarify.

Figs 12-13: Since error bars are interannual standard deviations (not measurement errors) and simulations cover same period as observations, why not show these for the models as well?

Fig 14: For the analysis, it would be helpful to also show (or in a separate figure) the full vertical profiles from the surface. This would help determine whether the differences seen starting at 215 hPa are there because the two models start with different surface values, or because they are vertically transporting the CO to different altitudes (e.g. maybe there is more GEOS-Chem CO at 300 hPa), or something else.

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