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Interactive comment on "Tropical troposphere to stratosphere transport of carbon monoxide and long-lived trace species in the Chemical Lagrangian Model of the Stratosphere (CLaMS)" *by* R. Pommrich et al.

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We thank Tao Wang very much for his comments on the paper. We will discuss the paper by Wang et al. (2014) in the revised version and will change the paper to clarify some points raised by Tao Wang. Our detailed response to his comments is given below with the original comments in italics.

This paper presents a simple way of modeling the CO transport in the UT/LS region. Besides the simplicity, the authors also seem to emphasize the reduced numerical cost of simulation. The paper is well written, and easy to understand. There seems to have several models simulating tracers in the UTLS. For example, one of the recent papers in ACP talks about using trajectory to simulate both ozone and CO (Wang et al., 2014) in the UTLS.

Indeed, we were not aware of the paper by Wang et al. (2014); we agree that it is relevant for our study and will discuss the paper in the revised version.

Their model is also fast (could be finished in 4 days for a 30-year run), and simple (by "borrowing" the circulation from reanalysis and the chemical production and loss from WACCM). Their results are quite convincing and matching with that from the MLS and ACE measurements. So I am wondering, compared to that model, what is the major advantage(s) of the model in this paper – besides the deliberate mixing scheme?

We agree that the model presented by Wang et al. (2014) is fast and constitutes a very useful modelling tool. However, the CLaMS (Chemical Lagrangian Model of the Stratosphere) transport scheme includes a unique Lagrangian mixing scheme (see e.g., Konopka et al., 2007 and references therein), which is a very important feature of the CLaMS model. It preserves tracer gradients well in atmospheric circulations (by avoiding Eulerian numerical diffusion), while at the same time allows mixing to occur where flow deformation occurs in the atmosphere. In a pure trajectory scheme, numerical diffusion is obviated, but of course at the cost of not representing atmospheric mixing at all. Furthermore, the CLaMS mixing scheme has the effect to maintain an approximately even distribution of air parcels, which allows integrations over long time periods to be performed. (See also reply to Review 1).

Also, a suggestion is that the authors might need to mention previous work properly,

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because it will lead the readers with more information to discuss about. The other question is that this paper also mentioned limitations of capturing elevated CO due to convection. But at the beginning of the paper, the authors mentioned about using MOPPIT and MLS observations as boundary/initial conditions. So I am guessing the convection's influence is already included in the boundary conditions, it is correct?

The CO is initialised below 3–4 km, which means that convection is not already included in the boundary conditions. We have now clarified this point right at the top of section 3.3. Further, the information from satellite instruments like MOPPIT and MLS cannot capture fine scale structures e.g., as they are observed by high resolution in situ measurements (see e.g., Fig. 4 in the paper).

Most of the figures in this paper only compare the anomaly, which is only a qualitative measure of patterns. I am guessing, if the climatology of circulation and chemistry is correct, the anomalies could be pretty resembling no matter what. Therefore, I am wondering if the authors could show some comparisons of modeled CO comparing to measurements quantitatively in real values? This helps in evaluating the model's performance.

We agree that if the climatology of circulation and chemistry is correct, the reproduction of the anomaly fields should be largely correct. However, we do not consider it straightforward that a model can achieve such a result, which is why we have provided the respective tests in the paper. A comparison in "real values" is an important next step, but introduces further complications, most notably the problem of biases in the satellite data themselves. For example Fig. 9b in Wang et al. (2014) clearly shows that there is a bias between MLS and ACE CO measurements. Therefore, for the present paper, we have concentrated on the comparison in absolute values for the in situ data, for which we were able to conduct some analysis of possible biases (see e.g., the Appendix of the paper). But we agree with T. Wang that further analysis of the CLaMS CO fields against measurements should follow.

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