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Interactive comment on “Trajectory model simulations of ozone and carbon monoxide in the Upper Troposphere and Lower Stratosphere (UTLS)” by T. Wang et al.

Anonymous Referee #2

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The paper describes an extension of a model for CO and ozone purely based upon Lagrangian trajectories that was used before in a similar setup to determine stratospheric water vapour. With the current setup, some questions arise, especially if the large number of air parcels is really needed. The described model setup seems to be not ideal and caveats of that setup must at least be mentioned as explained in detail below. I would suggest to focus more on the part of the domain where the model can be used in a meaningful matter. With these changes, the paper may be a valuable paper.

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1. The chemical lifetime of CO in the model domain ranges from 1-3 months at the lower boundary to orders of a day or less at the top of the domain. The chemical lifetime of ozone at the bottom of the model domain is rather large, but it also decreases in the tropics to about 10 days at 10 hPa and less than a day at the top of the domain.

The chemical loss and production rates are imprinted from the WACCM simulation. That means that the model results relax to the WACCM results with the time constant given by the chemical lifetime. Therefore it is clear why the results, especially in the upper model domain are virtually identical. In order to understand this problem and to interpret the model results, it would be necessary to show the chemical lifetimes e.g. similar as given in figure 2. As the purpose of the paper is not the validation of the WACCM model, the focus of the plot should more clearly be the regions in which the transport time scales are faster than the chemical time scales.

2. The model setup does not consider any mixing (if I understand it correctly). It may not be so important for most results shown here which are mostly averages, but it is not clear, how in general the neglect of mixing influences the results. Especially in correlations like those displayed in fig 10, the process of mixing should change the results
3. The model air parcels are initialized at the 370K level from the MLS climatology. This is typically between 100 and 150 hPa in the considered range (40°N-40°S). A comparison of the model results with data at 100 hPa is close to just comparing the initial conditions. Differences at 100 hPa (figs 4b, 9b) are potentially more due to vertical interpolation of the data than due to any process reflected by the model. What is the typical age of the trajectories at 100 hPa in the plot?

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4. Is in the comparison with ACE-FTS (e.g. fig 8) the latitudinal sampling taken into account? The plots could be either zonal mean cross sections (as indicated in the caption) or based on model interpolations onto the exact observation locations. The pattern of sampling times and latitudes of ACE-FTS may cause some of the shown difference.
5. The critical point in the simulation is the method, how the diabatic heating rates are determined. It is said that they are determined including all radiation, latent heat etc. Please verify that this is the case for all reanalysis data sets. This is not trivial, since not all terms are equally saved in all data sets and must be reconstructed.

Minor Points

1. **5999/ fig 4a:** The error bars probably denote the vertical averaging kernel. From that it seems that one cannot decide whether vertical velocities derived from ERAi or MERRA are better. Error bar/uncertainty of the mixing ratio would also be interesting.
2. **Fig. 1:** right y axis label (pressure) is not completely visible
3. **I. 5995/fig1 caption:** different latitude ranges are given. Is it 15 or 18 degrees?

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