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Geoscientific
Model Development
Discussions

Interactive comment on "Carbon monoxide as a tracer for tropical troposphere to stratosphere transport in the Chemical Lagrangian Model of the Stratosphere (CLaMS)" *by* R. Pommrich et al.

Anonymous Referee #1

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This paper describes a simulation of the CLaMS model using a simplified chemistry scheme to represent CO in the tropical upper troposphere and stratosphere. The novelty is to initialize CO in the middle and lower troposphere using satellite measurements, rather than initializing CO only at the surface based on emission estimates. The CLaMS model has a successful history of simulating trace gas distributions in the stratosphere, in particular around the polar vortex and tropopause regions, mostly due to it's detailed treatment of mixing. The extension of this model to multi-year runs with simplified chemistry should be useful for examining transport and mixing in the tropical tropopause region.

The paper is well written and appropriate for a journal of model development. My main critique of the paper is in the model description section and the initialization of the tracers. There are some gaps in the discussion of how the tracers are initialized and for a paper focused on model development as much as the results of the model run, this is a shortcoming. I would recommend the paper be published after some modification based on the comments below.

Main comments:

Sections 3.1.1, 4.1.3 Why not use parameterized convective transport? Is this just to save computing time? Even though you show that CO is simulated reasonably well in the tropical lower stratosphere compared to MLS it seems that it would certainly be better with convective transport included. The discrepancy between the model and COLD CO profiles shows the effect of convective transport as you discuss. I'm not convinced that the two profiles shown in Figs. 5 and 6 mean that the model is accurately representing the tropical background conditions. The CO is too low in the model from the level of convective outflow all the way up into the stratosphere in both profiles.

Section 3.1.2 Have you done any comparisons of the other tracers included in the model to observations? Especially CH_4 since that is involved in the chemistry of CO.

Section 3.2 I have the biggest issue with this section since it describes the most important and new aspects of this model run but there are some important details left out. Specific comments on this section are below.

Pg. 1193, Line 18: Do you use ECMWF wind to do the trajectory calculations?

In the description of the CO initialization using MOPITT measurements you start at 500 mb and use a grid filling technique for the lower levels. With this technique it seems like there will be more gaps in the trajectory endpoints at the lower levels. Is that true? In the binning I assume you interpolate vertically as well as horizontally so that the lower levels are likely to be mostly interpolated from higher levels. It would be useful to see what the trajectories look like for a given period to see their vertical extent.

You say you use NOAA ground measurements for the lowest model level but this is a sparse network. How do you interpolate to the whole globe? And how does the MOPITT based boundary look at the first model level above the surface compared to that at the lowest model level? Do you know how NOAA and MOPITT CO measurements compare? There would seem to be a big mismatch here and a lot of smoothing and interpolating to make it look reasonable.

There is a similar issue for the other tracers except that in this case you only use the surface measurements for the lowest model level boundary condition. How do you interpolate a limited number of observation locations to the globe?

For the calculation of mean age and the upper boundary of CO2 I have several issues. First, the upper boundary at 2500 K or roughly 55 km is in the mesosphere. Mesospheric loss of SF6 will make an age estimate using this tracer likely too old. The Stiller (2008) paper only shows age estimates up to 1000 K and even these are affected by SF6 loss. This will make your CO2 upper boundary condition too low. Second, you say that the mean age is determined from a linearly increasing tracer in the boundary layer. So what tracer do you use to calculate age in the model? CO2 or an artificial tracer that you assign a linear growth? If you use CO2 then your estimates of age in the troposphere are going to be affected by the latitudinal origin of the air mass since there is such a large latitudinal gradient in CO2. The ages shown in Figs. 5 and 6 suggest that you are using CO2 to estimate age since the tropospheric ages are not zero at the lowest level shown. The ages are up to several months near the surface, which doesn't seem like a realistic value. You could get these high values near the surface by transport of air from more southern latitudes where surface CO2 is lower than the tropical average. Overall your interpretation of the age estimates in the model and where the convective outflow is located may be correct but it may also be due to a different type of transport. You need to be much more clear in describing the technique you use here and the uncertainties associated with the technique.

Minor comments:

Pg. 1193, line 13: Awkward sentence, I would suggest "We use the MOPITT measurements at 500 hPa, where MOPITT CO is most reliable."

Pg. 1194, line 17: Should use "linearly".

Fig. 3 is quite small and hard to see all but the largest features. I would suggest showing a set of difference plots instead so that there are only 4 plots and the difference features will show much more clearly.

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