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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The roles of convection, extratropical mixing, and in-situ freeze-drying in the tropical tropopause layer" *by* W. G. Read et al.

W. G. Read et al.

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I thank the referee for taking the time to review and offer suggestions for improving the paper. The central criticism is how seasonal variations will affect the results produced by this model. It is straightforward to include seasonal behavior for the tropical in mixing since I have at hand the seasonal behavior of the observations. In the revised manuscript I will include the seasonal behavior for the tropical in mixing.

Replies to specific comments are (Referees comment in *italics*).

p.3968/69: CO-modelling, maybe also of importance for the discussion of CO p3976 or 3977,l.3: How sensitive is the model CO to changes of OH, which in turn depend on available H2O, which is calculated? The mean profiles of the loss and production rates might differ significantly from local spatial and temporal scales.

Sensitivity to chemistry will be difficult to address in this model especially since it has no OH parameterization. I have used the empirical representation based on work in the Folkins et al., 2006 paper. I believe the main conclusion from the CO results, that is the semiannual oscillation at the tropopause followed by an annual oscillation at 68 hPa are evidences for direct convective injection biomass burning into the TTL and the annual oscillation of the Brewer Dobson circulation acting on the vertical gradient on CO is robust and doesn't rely on the details of the chemistry. Certainly greater attention should be applied to the chemistry and circulation patterns (3D instead of 2D) to obtain better quantitative agreement.

p.3970,l.20/21: Which radii are assumed for the major modes of ice and what are the associated velocities or residence times in the TTL?

The answer to this is on page 3968 lines 10–16. Specifically radii is interpolated from this function r = [4.125,5.175,6.475,8.4,11.65,18.95,75.0,100.0] um iwc=[0.001,0.01,0.1,1.0,10.0,100.0,1000.0,10000.0] mg/m^3 for small particles < 100 um w_fall = w(clear sky) - 0.274*r$^{1.672}$ mm/s

p3975,l.19-27 (and associated conclusions): Are there any in-situ measurements available, which support the interpretation of the MLS data or at least other satellite platforms (ACE, AIRS)?

I think the problem here is a limitation of the model. The seasonal cycle produced by MLS for temperature and H2O closely matches that from analyses (for temperature. Analyses uses AIRS temperature) and HALOE (for H2O, I have not looked at ACE H2O apart from validation but I am confident it will show the same view. I may take at it look anyway).

p.3976.: Please clarify: Why does the absence of extratropical mixing steepens the vertical CO-gradient? That would be only the case when only tropospheric air masses are involved. If air from the lowermost stratosphere were mixed in, which happens (e.g. Tuck et al. 2003) this would reduce CO-mixing ratios in the TTL and thus steepen the

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vertical gradient rather than flattening.

This is based on comparing the extratropical CO measurements by MLS to the tropical CO measurements. See page 3966 line 15-16 to figure 4. I have no further insight why this appears to contratict the Tuck et al. 2003. What might be happening here is an interplay of where in altitude the seasonal amplitude of the BD circulation is largest versus where the vertical gradient in CO is largest. In the model, the seasonal cycle in the BD circulation is largest above the CPT whereas the extratropical mixing tends to lower the CO gradient at these altitudes.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3961, 2008.