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Interactive Comment

# *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 their time reviewing this manuscript. One of the major criticisms of the paper appears to be how the model uses the MLS temperature. As published, the model in the paper uses horizontal transport through a 2D Gaussian cold pool perturbation as originally proposed by Holton and Gettelman 2001. Here we used the MLS temperature measurements for the background and cold pool. I have recently modified the model to use longitudinally gridded MLS temperature measurements as suggested by the reviewer where now the horizontal coordinate in the model closely represents longitude. Although it is now possible perform a longitude dependent comparison with MLS H2O, I do not think it is appropriate for this paper. The horizontal velocity is still handled in a conceptual way being a constant value circumnavigating the globe. The improved handling of the MLS temperature field also addresses concerns regarding

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the handling of the 100 hPa value.

Another criticism was the motivation behind the model. The motivation is to provide a simple conceptual model involving multiple mechanisms thought to be important to explain some observations that can not be explained with a single mechanism. For example the concentration and annual cycle of H2O is well explained by in situ dehydration (e.g. Fueglistaler et al.2005 and others) but will fail for HDO (Dessler et al., 2007) and probably fail to produce a semiannual cycle for CO at the CPT tropopause(Sherwood and Dessler, 2003 and this paper). By combining the three processes of in situ freeze drying, extratropical mixing and convection one can reproduce the behaviors of H2O CO and HDO. A criticism repeated by other referees relates to the number of parameters that are introduced on account of adding multiple mechanisms. In the revised manuscript I will make an effort to distinguish which features are robust to a given mechanism and insensitive to the choice of parameterization.

Vertical diffusion is a carry-over from the original HG01 model. I will eliminate it. The results are quite insensitive to value I use.

Regarding calculation of the detrainment, I will make it more explicit that it is computed from the vertical velocity (omega) as suggested.

Responses to specific comments (*in italics*).

*page 3966, line 11: "extratropical supply" -> "extratropical mixing ratios"?*

Will do

*page 3968, line 26: "non-condensible" -> "insoluble"*

Will do

*page 3969, line 1: "In the CCT - TTL model, CO is represented by the time tendency of the vapor equation and all terms involving condensed phases vanish." Better expressed as: "In the CCT - TTL model, the rate of change of CO with time can be represented*



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*by an equation similar to that given for the time tendency of water vapor, except that all terms involving condensed phases are removed."*

Will do

*page 3971, lines 1 - 3: did not understand this sentence. Perhaps a diagram, or equation, would help.*

Let me work on this, a figure is a good idea.

*page 3972, lines 13 - 15: The 147 hPa Aura MLS H2O and CO would only be input to the bottom of the SA version, presumably, since that is the version where air is rising at the bottom (?). In the other two versions, air is sinking at 147 hPa, and a bottom boundary condition would not be needed? Also, it seems odd that a 147 hPa CO value would be used for a convective input mixing ratio. Would it not be more appropriate to use a boundary layer CO value, as this would be more consistent with the assumption of no mixing during convective ascent (which is invoked elsewhere).*

All this is true. I choose the 147 hPa MLS CO because it is the best CO measurement I know of that is close to the level of maximum convective detrainement which I use as a proxy for boundary layer CO. This has been done in work studying convection by Folkins et al., 2006. CO at 215 hPa might be better but MLS is biased high by a factor of two. Obviously it would be best to use boundary layer CO but I don't have that information.

*page 3975,line 20: The presence of subvisible cirrus does not necessarily imply dehydration, e.g. the ice crystals may be too small to fall out.*

This is correct I will reword to say something like the presence of persistent subvisual cirrus throughout the year is consistent with the tropopause being maintained at saturataion ($>$ 100% RHi) all year.

*page 3975, line 28: previous authors have argued that convective dehydration, can, at least in principle, explain the annual cycle.*



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If you are referring to the Sherwood and Dessler 2003 paper, in order to produce a realistic annual cycle for H2O they had to introduce a time dependent ice reevaporation factor that was related to the CPT temperature. Without this a convective dehydration model produces too weak an H2O An Osc. I am able to reproduce this behavior with our model. I would therefore say it is arguable that even with a convective dehydration model it is the An Osc in the CPT temperature driving the H2O seasonal cycle.

*page 3978, lines 19 - 20: "Therefore it appears difficult to simultaneously achieve good agreement with H2O and dD with convection mixing 100constant; i.e. no net evaporative or condensational growth since the evaporative flux equals the condensational flux. However, the water molecules on the surface of an ice crystal at 100% RHi continue to evaporate from it, so that the isotopic signature of the surrounding water vapor can still be influenced by the presence of the ice. I am not sure if the model took this effect into consideration. I agree that in the absence of net evaporation of ice, the exchange of water molecules might be restricted to the topmost layers of the water molecules, so would be slower than with unsaturated RHi $<$ 100% air.*

The model does not include exchange effects as described. The problem presented is that if convection detrains both H2O vapor at 100% RHi (at the environmental T) and an equal amount of ice, there will be significant moistening above that observed probably from re-evaporated ice and vapor detrained above the CPT. In this scenario I would speculate that HDO resupply from out-right ice evaporation probably dominates exchange effects.

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



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