

## ***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.***

### **Anonymous Referee #3**

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The manuscript is a very interesting attempt to bring together the hypothesis about dehydration processes in the TTL within a simple model and to discuss the results in comparison with the most recent observations. This work is made at the convergence of different streams of research and is very stimulating. It should be published with minor corrections. The points that I would like to suggest to be considered by the authors and which have not been listed (or in a different way) in previous reports are the following ones

1) Although the model is quite simple, incorporating many effects leads to a fairly large number of parameters or parameterizations and ad hoc choices that may impact the conclusions. It is certainly very difficult to conduct and present an extensive study of sensitivity to the numerous hypothesis within the model, but it is also very very difficult

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to figure out what are the robust results in this study (that are not sensitive to some tuning of the parameters) and the hard points that should orientate future research. It seems that averaged profile of water vapour and its isotopologues are not really discriminating the various hypothesis, provided enough mixing from the extratropics is allowed. It is perhaps by addressing in more details the temporal and spatial variability of the tracer distribution that further progresses could be accomplished.

2) The model displays a large sensitivity to mixing from the extratropics but it is hardly seen how the seasonal variations, in particular associated with the Asian monsoon are taken into account here. Using data bounded between 12S and 12N is perhaps not the best way to sample the monsoon.

3) The definition of the environmental temperature and the fact that CSD01-ICE carries the same total amount of water as C-NOICE appear as fairly strong hypothesis that deserve some further comments on how they can bias the comparisons.

4) Sedimentation is mentioned on p.396, *but I do not see any corresponding term in (1)*.

5) The detrainment of isotopologues depends to a large extent of the precipitation conversion rate. This would predict a maximum depletion near 14-15 km, where most of the air involved in heavy precipitation detrains while ice can be lofted with air above this level resulting in much less depletion. I do not see how this important effect is taken in account in the model.

6) The cold point temperature is a main control parameter but it is unclear that the MLS temperature provides the required vertical resolution in the vicinity of the tropopause. This choice, rather than using analysed temperature (which have admittedly their own caveats), should be justified.

7) There are significant differences between the values of H<sub>2</sub>O from MLS V1.5 and V2.2 retrievals (which are even more pronounced in the spatial distribution) that draw some uncertainty on the whole discussion. This should deserve at least a few words.

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8) There is an excessive use of acronyms in the manuscript which could be somewhat tempered in expressions like, e.g., "cold trap CPT temperature" on 3974,110.

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

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