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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 want to thank you for your comments. I will try to address these as best I can. Referee comments in *italics*.

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 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 discreminating the various hypothesis, provided enough mixing from the extratropics is allowed. It is perhaps by adressing in more details the temporal and spatial variability *of the tracer distribution that further progresses could be accomplished.*

I agree this is a problem. I will focus on establishing what is robust, that is insensitive to uncertain parameterizations. I know that this is certainly a problem with the isotopolgues. There have been a few papers in the past that have been able to reproduce the observations emphasizing certain mechanisms. One thing I am trying to do in this paper is to explain why this is the case. I believe that CO adequately distinguishes between convection versus extra tropical mixing as the source for enriched HDO in the TTL. In this context, the isotopologue are useful for distinguishing at least in a broad sense the microphysical behavior of convection.

2) The model displays a large sensivity 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.

I will investigate including a seasonal variation in the extratropical mixing. This is straight forward to do since it will be based on observations and incorporate some effect for the Northern hemisphere monsoons. The focus of this paper is on the deep tropics. I used 12S–12N based on Mote et al. 1996 analysis.

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.

Oftentimes when one adds a convective effect to a trajectory based model, it is done by injecting 100% (or higher if supersaturation is being considered) relative humidity air into the grid box. RH is computed based on the grid box temperature (this is what I mean by environmental temperature). Following this lead I have used these conditions as a starting point to study convection's effect. It is also a useful test case for studying the isotopologues. In the second convective case where ice is also mixed in, I agree that things can become open ended because the amount of ice injected is not well

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constrained. However, based on work by Jensen et al. 2007, I think it can be reasonably argued that it amounts to less than 10 ppmv (amount in small crystals that can evaporate before sedimenting out). I did consider differing amounts of ice rentention for the two convective cases considered in a discussion on page 3978 line 12 onwards.

4) Sedimentation is mentioned on p.396 but I donot see any corresponding term in (1).

The sedimentation takes place in the parcel advection viz. $\frac{D[X]}{Dt} = \frac{\partial[X]}{\partial t} + w\frac{\partial[X]}{\partial z} + u\frac{\partial[X]}{\partial x}$. I will add this equation.

5) The detrainment of isotopologues depends to a large extend 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.

Rain-out in the model occurs by ice falling out of the model's bottom level. If I understand what is meant by precipitation conversion rate I assume this means the fraction of condensed phase that is instantly removed. This model does does not have this but instead only considers the amount of retained ice available for evaporation after convection collapses. Consistent with the referees expectations the model does show a maximum depletion at 14 km (the model bottom) which occurs because the model is producing less evaporation and more precipitation.

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.

In the revised manuscript I will be using the MLS v2.2 temperature which has better vertical resolution than v1.5. I have also made bias corrections in the v2.2 based on correlative data sets such as GPS, and analyzed temperature. The biases are

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stable over time and even latitude. I believe this should adequately handle the vertical resolution problem. One advantage gained by using MLS temperatures is that they are measured coincidently with H2O and this affords the opportunity to correlate short time behaviors between both fields. I make this point in the paper and I will probably show a time series of MLS temperature in the final manuscript to emphasize this point.

7) There are significant differences between the values of H2O 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 leat a few words.

I was not aware that there are significant spatial differences between version 2.2 and v1.5 H2O. The difference in the values arise from different vertical smoothing behaviors in the product. Version 2.2 is gridded every 1.3 km which is half that of version 1.5, hence it is important to smooth each data set with its respective averaging kernel. Since v2.2 reprocessing is about 1 week from completion, I plan to use only v2.2 in the revised manusript. I recently used v2.2 MLS temperature to run the model and it now shows better agreement with MLS H2O (particularly at 147 hPa) than using v1.5 temperature and comparing with v2.2 H2O.

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,l10.

I will work on improving this aspect of the paper.

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