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Atmospheric
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Interactive comment on “A model of HDO in the tropical tropopause layer” *by* A. E. Dessler and S. C. Sherwood

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We greatly appreciate Prof. Holton’s thoughtful comments on our paper. With this response, we hope to develop a dialogue on many of these important issues.

Holton: "the model is highly parameterized, and requires a certain amount of somewhat arbitrary ’tuning’ to achieve these results."

D&S: Though some aspects of the model solutions are clearly sensitive to selection of parameters that are not well known, the key results we emphasize are not. The main conclusion of our paper is that the near-zero gradient in HDO depletion seen in the ATMOS data is a fundamental property of our theory, and requires no parameter adjustment. As we discuss in the paper, changing the amount of lofted ice, or the HDO depletion of the ice, or how the depletion is specified (constant, random, or altitude dependent), does not change the predicted gradient of HDO depletion in the TTL.

Under all scenarios, our model predicts a near-zero gradient, in agreement with the ATMOS data. The reason for this is that dehydration in the TTL in our model occurs primarily through injection of dry air, which does not change isotopic depletion.

Holton: Perhaps more importantly, in the real world the tropical tropopause layer is subject not only to penetrative convection, but also is filled with rather long-lived cirrus clouds. Some of these are anvil clouds associated with convection, but others appear to be formed in-situ, independently of any underlying convection. It seems hard to believe that these ubiquitous cirrus do not play some role in stratospheric dehydration.

D&S: Cirrus clouds may indeed play a role in dehydrating the TTL. However, it is far from evident that this process is important. Models like those of Holton and Gettelman [2001], Gettelman et al. [2002], and Jensen et al. [in preparation] have shown that cirrus dehydration can generate realistic TTL water vapor distributions, but these models are untested against other trace species. There are reasons to believe that cirrus dehydration models will fail to accurately reproduce the distribution of other trace species. For example, it has been argued that cirrus dehydration models will produce unrealistic HDO gradients in the TTL [e.g., Moyer et al., 1996; Dessler and Sherwood, 2003], and a recent paper [Sherwood and Dessler, 2003, preprint at http://www.meto.umd.edu/˜dessler/ttl-isot2_pp.pdf] has shown the difficulty that slow-ascent cirrus-dehydration models, such as the Holton and Gettelman model or the Jensen et al. model, might have in reproducing the phase and amplitude of the observed CO2 seasonal cycle in the lower stratosphere. Our simple model using convective mixing can account for all of these constituents. We challenge those supporting cirrus dehydration to show that models incorporating cirrus dehydration can account for the observed distributions of trace species besides H2O in the TTL.

There were also a number of specific comments. Below are our responses to those that benefit from one. Others we will consider in revision, but we provide no specific response here.

Holton: p. 4495, line 7: This assumption of a constant ice water = 4 times the water vapor in the clouds seems pretty arbitrary.

D&S: As we said above, our simulation of a near-zero gradient in HDO depletion is not dependent on this parameter.

Holton: p 4495, line 17: It is likely that descending cold parcels with collide with subsequent convective upwelling, producing enhanced mixing with air of differing characteristics. It is not clear how this may affect the results.

D&S: Our representation of mixing assumes that parcels ascend into an unmodified environment characteristic of a large region, and mix with that environment. Mixing with previous convective elements is an added complication that we have not considered. However, since the results here stem essentially from the tendency of convection to homogenize conserved tracers, it seems unlikely that this affect would make a significant difference. Also, observations of overshooting systems in mid-latitudes suggest that downdrafts and updrafts do not "collide" but flow past one another [e.g., Roach, 1967].

Holton: p 4495, line 21: Won't the mixed air still be saturated owing to ice evaporation?

D&S: Our model assumes the processes occur in the following order: transport by convection of the parcel to specified height, removal of all ice except for that retained (4x vapor in most runs), mixing with the environment, and subsidence to detrainment altitude. Thus, even though the convecting parcel is saturated and contains ice, it is so cold that the saturated vapor plus ice equal to 4 times vapor is still a small value. Mixing with subsaturated ambient air followed by descent to lower (and usually) warmer altitudes means that the parcels are subsaturated when they detrain. Whether this is realistic depends on how the rainout timescale compares with the timescale of temperature homogenization within mixtures, an issue that could be investigated with numerical models.

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Holton: Figure 2: Why is there a departure from a monotonic decrease with height? Does this have something to do with the stability profile?

D&S: The detrainment ratio (detrained mass over ambient mass) tends to be proportional to static stability, since the amount of ambient mass between theta surfaces is inversely proportional to deviations from the dry adiabatic lapse rate. The static stability becomes very small in the lower TTL, so the calculation is quite sensitive there. The detrainment profile may actually be accurate, or it may indicate too little detrainment in the lower TTL (due to our neglect of weaker convection) or too much above the tropopause.

Holton: p 4496, lines 10-16: Yes, but in the real world cirrus anvils are formed, so aren't your assumptions pretty unrealistic here?

D&S: Subsaturation in the TTL is not an assumption, but a prediction of our model. Ice content (which is mostly in stratiform clouds) is also predicted reasonably. Remember that our model predicts the tropical average, and on average the TTL is subsaturated even though it also contains ice. We did not include in situ ice formation alongside cumulus-generated ice, because our theory suggests they do not play a role in the TTL. Our model demonstrates that models can ignore cirrus clouds can still get the important properties of the TTL correct.

Holton: p 4496, lines15-18: (also p 4494 lines 20-26) The assumption made here that the amount of overshoot is tuned to balance the assumed large-scale subsidence is a bit worrying given the poor current understanding of the so-called "stratospheric drain" which in any case seems to affect only a small region so that this assumption would seem to produce an overestimate of the overshooting mass on a tropics wide basis.

D&S: We have accounted for the smallness of the drain region in our model. The uncertainties in the convective contribution to the observed cooling are of course large, but the goal of this study was to see if convection could consistently explain both the cooling and the other observed effects.

Holton: p 4498, lines 5-6: It would be nice to know whether the "tuning" for the summer case was much different from the winter, and just how "similar" the results are. In summer much of the convection is over the American and African continents, where the "drain" assumption wouldnŠt apply.

D&S: If the "drain" process is due to overshooting convection, it should occur wherever the deepest convection is. The model calculates similar convective cooling for summer and winter temperature profiles. The near-zero gradient in HDO depletion does not depend on the background temperature profile, and could not be made to disappear without radical changes to the model, so it is certainly predicted regardless of season. Other details of the seasonal cycle runs can be found in Sherwood and Dessler [2003, preprint at http://www.meto.umd.edu/˜dessler/ttl-isot2_pp.pdf].

Holton: p 4501, lines 6-8: This picture of an ice crystal falling approx. 5 km while slowly evaporating seems unrealistic. It is also contrary to the model assumption mentioned on page 4496 of "instantaneous" evaporation.

D&S: Ice crystals do not immediately evaporate, even in subsaturated air. There are, in fact, observations that large crystals can fall significant distances through subsaturated air [Hall and Pruppacher, 1976]. Our model does not assume instantaneous evaporation of retained ice, but that the ice evaporates with a time scale of one day. The comment referred to above reflects the fact that evaporation was much faster than the other time scales of the model, so one could consider the evaporation of the crystals to be happening rapidly in comparison.

Holton: p 4504, lines 11-15: Temperature variations associated with atmospheric waves such as gravity waves and Kelvin waves, would also produce subsaturated regions to facilitate evaporation.

D&S: Waves of all periods are correlated with deep convection in such a way that the TTL is typically colder than average when convection is active, at least in the tropical west Pacific [Sherwood et al., 2003], which would inhibit reevaporation of ice. We

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neglected this, but since we tuned ice reevaporation anyway, this is just one of many physical processes that is implicitly folded into the ice reevaporation parameter. We only stand by results that are insensitive to that parameter.

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