Reviewer #1 comments in plain font.

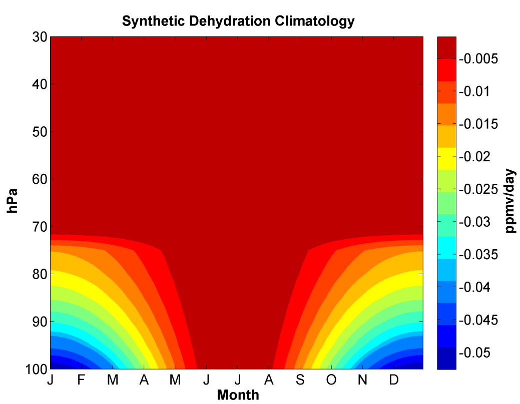
Author response on 1st round in public discussion in bold font, new responses in blue italic font.

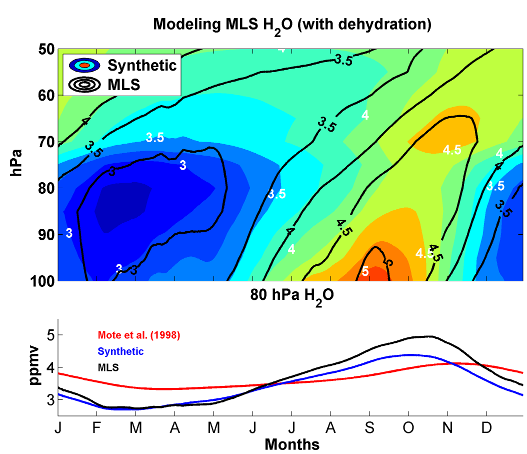
Revised manuscript with highlighted changes attached as supplement.

General comments:

The primary issue this reviewer sees with our results is the neglect of explicit dehydration in the 1-d transport model. In our initial reply to the reviewer (see public discussion) we present arguments (highlighted in bold font below) why we think that it's quite unlikely that the neglect of dehydration significantly influences our results.

As discussed in section 6 of the revised manuscript (3rd and 4th paragraph in that section), we have also performed sensitivity experiments with our 1-d transport model by incorporating prescribed amounts of dehydration at its lowest levels (see plots shown below). These experiments confirm our expectation that dehydration brings the simulated water vapor signal closer to the observed one during boreal winter. However, it actually degrades the simulation during boreal summer, even though we don't apply dehydration during that season. This is because the now drier signal during DJF is propagated somewhat into JJA. This would then require an even larger amount of mixing during JJA. Also, the overall agreement of the entire seasonal water vapor evolution is not much different from the simulation without any dehydration. We conclude that it's quite unlikely that the neglect of dehydration explains the diagnosed levels of mixing strength in our simulations.





We have furthermore followed the suggestion by the reviewer to analyze higher levels (above 80 hPa) where the effect of dehydration can be neglected (see added text in discussion section of revised manuscript – paragraph 5 in section 6). While the diagnosed strength of vertical mixing (diffusion) does decrease with altitude (as expected *physically – as one moves away from the convective tops and the tropopause with their associated turbulence and small scale wave activity), it is still significantly enhanced relative to the control value. At 70 hPa the top-scoring solution still uses 2 times the control value for K. In isentropic coordinates, however, the control values remain adequate, which again is consistent with our arguments related to the difference in coordinates used.*

In summary, we appreciate the issue brought forward by the reviewer but feel that we have sufficient evidence to show that this issue is not severe enough to invalidate our main conclusions. We have incorporated additional discussion in section 6 of the revised manuscript, which we hope makes this section as a whole a more balanced discussion of the strengths and weaknesses of our approaches and conclusions.

Specific Comments

1) Our results are consistent between MLS and HALOE (as stated on line 6, page 12), the latter having a better vertical resolution (~1.5 km) and hence presumably less impact from sources/sinks at the tropopause.

HALOE data shows an absolute minimum in water vapor during boreal winter at 83 hPa (e.g. Mote et al, 1998, Plate 1), similar to MLS, and I would argue that the results from both satellites are influenced by dehydration near the cold point. Note, however, that the Mote et al 1998 paper utilizes an EOF reconstruction of the HALOE data to perform their calculations of diffusion and dilution, and this reconstruction has water vapor extrema at the lowest level (100 hPa), and hence avoids dealing with the relative minimum at 83 hPa.

While we agree that results from both satellites are influenced by dehydration, we expect the degree of that influence to be smaller for HALOE, due to its finer vertical sampling. As elaborated in our general comments above, we also agree that the minimum water vapor at 80 hPa during Feb-March is likely a signature of this dehydration influence (and we have added a remark in the discussion section of the revised manuscript). However, our sensitivity experiment shown in the general comments suggests that the overall influence by dehydration is small (although it does improve the simulation during the season where one would expect it – DJF).

2) Dehydration would produce an additional negative tendency in our budget, especially during boreal winter when the cold point is located higher. However, this would inturn demand a larger positive tendency from the other terms to compensate. Thiswould therefore if anything result in an even larger contribution due to mixing than we diagnose (cf. Fig. 7, possibly a combination of vertical and

horizontal mixing) â˘A˘T the opposite of what the reviewer claims.

The large vertical diffusion calculated in this paper results in a strong negative H2O tendency at 83 hPa during November-January (shown in Fig. 7). I believe this tendency is compensating for the explicit dehydration that was neglected in the idealized model (which would occur exactly at this time).

See above. It's possible that our vertical mixing tendency is off during NDJ, due to the neglect of dehydration but that still only accounts for 25% of the year. Our sensitivity tests shown in the general comment suggests that the incorporation of dehydration doesn't change the diagnosed mixing strength much.

3) Furthermore, we find that vertical mixing is most important during boreal summer when the contribution from vertical advection is too small to keep the tape recorder going (cf. first paragraph of discussion section). But during boreal summer the cold point is lower making the expected contribution from explicit dehydration smaller and therefore contradicting the reviewer's claim.

Figure 7 shows that vertical mixing is strong during August-October and November-January (with opposite signs). I don't understand the derived August-October maximum (and can't think of a reasonable physical mechanism for this timing), but I agree it is probably not tied to explicitly neglecting dehydration.

We agree.

4) Note also that the lower panel in Fig. 6 shows that a) our synthetic solution doesa much better job than Mote et al. at capturing the observed evolution, b) we tend to overestimate the observed values during boreal winter (consistent with the neglect of explicit dehydration), c) we tend to underestimate the observed values during boreal summer (so dehydration would if anything make the situation worse in that season). One possible reason for our bias during boreal summer is that we neglect the potential contribution of convective hydration (due to overshooting convection, e.g. Corti et al. 2008). Estimates of this contribution for the tropics-mean are difficult and so it's hard to say something more definitive about it. Dessler et al. (2016) recently found indirect evidence that this contribution might be significant for future stratospheric water vapor trends.

As noted in the response to (1) above, the Mote et al 1998 analysis focused on an effectively vertically smoothed H2O data set, without the absolute minimum of water vapor at 83 hPa, so comparisons with the current results at this level are not straightforward. Tropical convection extends to higher altitudes in boreal winter compared to boreal summer (e.g. Chae and Sherwood, JAS, 2010), so there is little reason to expect a stronger signal above the tropopause during summer.

We agree and have modified the discussion of the potential role of convective hydration during summer in the revised manuscript.

5) We'd also like to stress again (as in the paper, e.g. lines 13-21 on page 12) that we

obtain physically reasonable differences between pressure and isentropic coordinates. Specifically, vertical mixing does not play an important role in isentropic coordinatesand our results for these coordinates are consistent with previous findings in the literature (e.g. Ploeger et al. 2012). However, the contribution from dehydration (or any other sources/sinks) should be largely independent of the coordinate system used,mixing to be much more important in pressure coordinates, but not so much in isentropic coordinates, then speaks against it being artificially enhanced due to the neglectof sources or sinks.

This may be a valid argument. However, if the model is inappropriate and the results are questionable in pressure coordinates (the native coordinates of the MLS retrievals), I cannot be convinced they are reasonable by comparison to isentropic coordinate calculations (derived from vertical interpolations of the pressure level data).

We don't understand this argument. The isentropic coordinates are derived from a consistent observational product (as opposed to e.g. incorporating temperatures from a reanalysis). The interpolation calculation is simple and straightforward. We maintain that we are able to reproduce findings from the past literature, which are physically reasonable, in isentropic coordinates, and that this supports the validity of our approach.

6) It's possible that the simple 1-d formulation of our model (as in Mote et al. 1998) misrepresents horizontal mixing and that part of our diagnosed vertical mixing in fact represents masked horizontal mixing (cf. line 19-21 on page 12). Hopefully future work can shed more light on this caveat.

I agree it may be difficult to separate horizontal mixing from vertical diffusion using this idealized model. However, the neglect of explicit dehydration is a more important problem at 80 hPa. This idealized model applies to transport above the altitude of dehydration, i.e. tracking the minimum water vapor from the dehydration level to higher altitudes. In the MLS (or HALOE) data, the minimum water vapor occurs at the 83 hPa level, so it should be reasonable to apply the model above that level. However, applying this model to lower altitudes (and neglecting a physically important term) leads to the conclusion that vertical diffusion is a dominant process influencing the 83 hPa level, and I believe this conclusion is incorrect.

See our general comments regarding results at higher levels and explicit incorporation of dehydration.