I appreciate the response of the authors to my previous review, and think it is appropriate to reply in turn to several of their 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.

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 in turn demand a larger positive tendency from the other terms to compensate. This would 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) âˇAˇT 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).

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.

4) Note also that the lower panel in Fig. 6 shows that a) our synthetic solution does a 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.

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 coordinates and 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 neglect of 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).

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.

- Please note that nowhere in the paper do we claim that we've found the final answers to the transport problem near the tropical tropopause, nor do we claim that we have 100% proof that vertical mixing is as strong as indicated by our results (e.g. statement on line 7, page 12). Rather, we present evidence that points to a potentially greater importance of vertical mixing for transport just above the tropical tropopause than previously assumed.

Carl Sagan noted that ‘extraordinary claims require extraordinary evidence’. My opinion remains that the important new result here (large transport due to vertical diffusion in the lower stratosphere) is not supported by the analysis. I appreciate that the authors have put significant effort into this work. I suggest either explicitly including dehydration in the calculations (probably difficult to do in an accurate manner) or simply focus on the region above 83 hPa, where the idealized model is more appropriate.