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Interactive comment on "Role of vertical and horizontal mixing in the tape recorder signal near the tropical tropopause" *by* A. A. Glanville and T. Birner

Anonymous Referee #2

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The paper addresses an important topic on transport processes in the tropical lower stratosphere. More accurate simulations of chemistry and climate require improved understanding of the dynamics of this region, and this paper could represent a key contribution towards this effort.

Overall the paper is very well written and the figures are clear. My only major concern surrounds the analysis methodology, as noted below. Otherwise, there are a few minor issues and clarifications noted.

Major:

One primary conclusion of this paper, that vertical mixing in the tropical lower strato-

sphere must be four times larger than the value estimated by Mote et al (1998), rests on the 1-d model simulations. While the model is fairly simple conceptually and is likely to be a useful tool for this kind of analysis, the devil is in the details which are not completely explained, and the impact of the certain assumptions embedded in the model are not fully explored.

1. Seasonality is introduced in the parameters omega, K, and alpha by prescribing reductions and enhancements of 50% over the course of a seasonal cycle. There is no discussion of why a 50% variation is a valid assumption. Is there observational evidence to support fixing this amplitude? How are results impacted if one chooses, say 30% or 70% amplitudes? Are these prescriptions sinusoidal seasonal variations? For the phases of seasonal cycles, the paper has some discussion that justifies the choices based on models or observations; however, "boreal winter" and "boreal summer" are given instead of dates or months, which would be preferable. For example, saying that horizontal mixing maximizes during boreal summer likely refers to the July-August period and not the NH summer solstice. Gettelman et al (2011) discuss the importance of horizontal mixing during July-August (e.g. the Asian monsoon anticyclone).

2. The model's score is based on comparisons with the amplitude, phase, and annual mean of the observed water vapor mixing ratio at 80 hPa (or 400 K), but it is not clear how these are derived from the MLS data. Is this from a simple FFT analysis? If so, Fig 4 indicates that the seasonal variations are not exactly sinusoidal, so how does this impact the analysis if a different functional form is used, one that better simulates the seasonality of the effective transport velocity? In this regard, is there any explanation for why the MLS velocity in Fig 4 has a double minimum, or is the spring dip just noise?

3. Use of a constant, 7-km scale height to convert from pressure velocity: This is not appropriate for a couple of reasons. First, temperatures near 70 hPa are about 200-210 K in the tropical lower stratosphere, so that the scale height is closer to 6 km. Second, there is a well-documented seasonal cycle in temperature that causes variations of 3-4% in the scale height, and this should be included in the calculation of

effective transport velocities, particularly in examining there seasonal behavior.

Minor:

1. Abstract, lines 8-9: This seems to state that the seasonal cycle of residual velocity derived from MLS has a larger amplitude than that in ERA-i, which conflicts with results shown in Figure 4.

2. Abstract, lines 20-21: “as opposed to” implies an either/or scenario, whereas I think this paper finds that a combination of slow upward transport *and* rapid vertical mixing play a role in shaping the tape recorder signal.

3. p 3, first paragraph: The latitude averaging for MLS data should be presented here, along with a discussion/justification of the choice of latitude bounds (appears to be 10S-10N from figure captions).

4. p. 4, lines 30-32: As correctly noted, the effect of methane oxidation is primarily an additive constant. This can be easily accommodated by looking at anomalies for the MLS data analysis, or by a simple parameterization of “S” in equation 1 for the 1-d model. Thus, this reason alone does not seem to be a valid motivation for restricting the analysis to altitudes less than 21 km ($\sim$40 hPa).

5. p. 5, line 32: The midlatitude reference mixing ratio should be allowed to vary seasonally for a correct model simulation. If that is the case, it should be clearly stated here.

6. p. 8, lines 18-20: “while its phase relies more on string enough vertical advection and on allowing for transport seasonality” is unclear. Is this saying something about simulating the phase of the tape recorder? If so, what is “strong enough” and for which transports (advection, vertical mixing, or horizontal mixing) are the seasonality important?

7. p. 10, lines 7-12: A lot of the notation needs to be clarified in the equations, e.g., what do the hat symbols represent?

8. p. 10, lines 27-32, and Figure 9: First, it is not obvious why we should care much about vertical profiles of derived vertical eddy fluxes. The “sanity check” rationale is a stretch, as the vertical gradient only gives consistency with the 1-d model to within a factor of 10, and upon closer inspection, the negative tendency shown in Fig 7 for the vertical eddy mixing in boreal winter should correspond to a negative slope in Fig 9 for DJF at 80 hPa, which is clearly not the case. Thus, it appears that there are very large errors in the calculated eddy fluxes (perhaps as expected when taking differences between two quantities with large inherent uncertainties). A more robust discussion of the uncertainties in these results is warranted, along with a more complete analysis (e.g. comparison with previous studies, or what has been used in the past in 1-d models) of calculated eddy fluxes.

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