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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 #1

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This paper analyzes the seasonal cycle of tropical stratospheric water vapor based on satellite observations from MLS and an idealized tracer continuity equation (Eq. 1, with parameterized horizontal and vertical mixing terms). Comparisons are also made for water vapor in the ERAinterim reanalysis, which is completely a model result (no stratospheric water vapor data are assimilated in ERAinterim). The idealized model is used to fit the vertical propagation of the water vapor minimum derived from MLS data, assuming zero sources/sinks in Eq. 1 (i.e. changes above 100 hPa are interpreted as a combination of vertical advection and horizontal/vertical mixing above that level). Much of the focus is on variability at 80 hPa, and results are interpreted as demonstrating large amounts of vertical diffusion in the lower stratosphere (many times larger

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Discussion paper

than previous estimates). However, my opinion is that the authors have neglected to consider the important effects of explicit dehydration for the 80 hPa level (the S term in Eq. 1). The tropical cold point tropopause is near 90 hPa, and the water vapor estimated from the ∼3 km broad layer MLS retrievals at 83 hPa certainly includes the influence of dehydration near the cold point. Note that an absolute minimum in water vapor is seen near 83 hPa in ∼March in Fig. 2; note also that the red dashed line in Fig. 2 does not accurately trace the water vapor minima in altitude. Interpreting variations at 80 hPa as solely resulting from vertical advection and mixing from the 100 hPa level is incorrect, resulting in the large (and likely unrealistic) derived values of vertical diffusion. The calculations at 80 hPa are incorrect because of the neglect of explicit dehydration; the model may work better at altitudes above the dehydration level, but that is not the focus here. There are other aspects of the results that I am not comfortable with, as the details of the isentropic calculations and effective transport velocity in the second half of the paper get very complicated and difficult to understand. However, this doesn't matter, as the same fundamental problem exists as in the pressure coordinate formulation. Overall I am unconvinced by the analyses presented in the paper, and I do not suggest publication of this paper in anything close to present form.

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