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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The relationship between tropospheric wave forcing and tropical lower stratospheric water vapor" *by* S. Dhomse et al.

S. Dhomse et al.

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Reply to general comments
by **S. Dhomse**
(Reviewer's comments are in italics.)

General comments: The study for WV variability in the TLS is an important topic to understand the tropospheric-stratospheric exchange and predict the future climate in the stratosphere. The methodology and topic in this study is suitable for ACP, but new results which this study provided are unclear. Therefore, the paper should be revised before published in ACP.

1) The first point to be revised is to clarify the difference between the results from this

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study and Randel et al. (2006). The relationship between WV in the TLS and tropical tropopause temperature has been examined by Randel et al. (2004) and Fueglistaler and Haynes (2005), and the relationship between WV in the TLS and wave activity in the lower stratosphere has been investigate by Randel et al. (2006). The reader will confuse new results and well-known facts, because section 1 lacks the detailed review about the relationship between WV and temperature in the TLS, and the relationship between WV in the TLS and wave forcing: what has been known already and what has been not known yet. So the authors should devote their effort to clarify the new discoveries of the study both in the abstract and the conclusions, as the new discoveries can be distinguished from known results. If the authors could do it, the paper will provide convincing results to ACP and the atmospheric science field.

We thank reviewer 1 for carefully examining our manuscript and his/her detailed comments which helped us to improve our manuscript. We agree with the reviewer that our earlier manuscript did not provide complete review of the earlier studies. In the revised version, we expanded the introduction for a better summary of previous work and put it into persepctive to our work.

Fueglistaler and Haynes (2005) and Randel et. al., (2004, 2006) showed that WV VMRs in tropical tropopause region are controlled by cold point temperatures. Randel et. al. (2006), also showed that cold point temperatures and tropical upwelling are strongly correlated and that the observed decrease in WV VMRs in TLS (as well as cold point temperatures) is consistent with an increase in the tropical upwelling as well as decreases in ozone near TTL. He also showed that the drop in water vapor after 2001 can be explained by a change in the strength of the BD circulation that was higher after 2000 compared to the five year period before 2000. In this study, we want to look more on the interannual variability in the BD circulation strength and its possible impact on variation in TLS water vapor. We show that (with an exception of 1991, 1997) increases in wave driving that is a direct measure of the BBD circulation strength was responsible for decreases in WV VMRs between 16-20

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km. The observed departure from this relationship during 1991 and 1997, however, also highlight that the inter-annual variability in TLS WV can not be solely attributed to extra-tropical wave driving.

Another concern is the multi-regression analysis based on a linear trend which differs in seasons, QBO and global eddy heat flux (Figure 4 and 5). The authors propose the possibility that a drop of stratospheric WV after 2000 is caused not only by a sudden increase of wave forcing, but also by linear trend. To convince this result, however, some points below are worth discussing.

1) Is linear trend component included in eddy heat flux (HTF)? If so, how can the BD circulation and linear trend components in temperature be interpreted? If the linear trend components are excluded from the multiregression analysis, the result about a cooling of about 0.7 K by the HTF still remain robust?

In the revised manuscript we make it clear that the eddy heat flux contribution to the temperature remain robust even after removing the long term trend from HTF, there is very little change in its contribution (0.68 K) for the cooling at 70 hPa. Here we are not trying to quantify influence of wave driving on cold point temperatures (or in turn dehydration mechanism). We clarify that at this level (70 hPa) cooling represents intensification of upwelling in tropical stratosphere and not necessarily the cooling in TTL. Also it should be kept in mind that in our regression analysis we try to include all important processes which (we assume) contribute significantly to changes in the TLS temperatures. The linear trend term accounts for other processes such as changes in GHG, SST, and convection patterns.

2) How much WV decrease can be explained by the cooling of about 0.7 K induced by HTF and by the linear cooling trend in the TLS? The decrease amount of WV had

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better be discussed in the results and clarified in the abstract, since the paper treats the relationship between WV in the TLS and HTF.

From Fig. 3 TLS water vapor decreased by about 0.5ppm (HALOE) after 2000 can be related to the 0.7 K cooling in the TLS inferred from Fig. 4. Füglistaler and Haynes (2005) came to a simlar result that a This is s a 1 K cooling in the TTL cold point temperature decreases water vapor by 0.5 ppm.

Does the importance of linear trend in the multi-regression analysis in Figure 5 still remains in the same analysis using ERA40, HadAT2, and ECMWF Operational analysis in Figure 6?

We explain that long term linear trends are not statistically significant (for both NCEP and ERA40). Because of the biases between ERA40 and ECMWF operational data, we perform regression analysis with ERA40 only up to August 2002. The regression coefficients are nearly identical for both data sets and slightly lower for HadAT2.

4) Furthermore, the authors calculate HTF at 50-hPa level to infer the BDC in the TLS in Figure 1-3, while using temperature at 70 hPa and WV averaged between the levels of 16-20 km. The conventional theory predicts that tropical upwelling at a certain level can be inferred from the HFT from both hemispheres at the level (Haynes et al., 1991). The authors are required to describe why they choose the HTF at 50 hPa, but not 70 or 100 hPa. In addition, is 50-hPa HTF used for regression analysis at each level? If so, the authors need to add the reason. The paper uses the terminology planetary wave activity *to describe the HTF, but lack the detailed method of HTF calculation. How do you calculate the HTF for planetary wave? At the tropopause level, waves with planetary wave number 4-5 (or synoptic scale) are also thought to play an important role to induce the seasonal cycle of upwelling at the TLS (e.g., Seol and Yamazaki, 1999; Randel et al. 2002).*

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We agree with the reviewer that the contribution from higher frequency waves is an important factor. We extended our discussion about eddy heat flux in the revised version. Selection of eddy heat flux at a particular level does not matter in NH where increase/decrease in wave activity is generally observed at all stratospheric levels simultaneously. In the NH, correlation between HTF at 100 hPa and 70 hPa with 50 hPa HTF is always above 0.95, but in the SH, correlation ranges from 0.7 to 0.8. For the period selected in this study, 50 hPa eddy heat flux gives maximum correlation with the WV VMRs between 16-20 km. As pointed out by the reviewer and others (e.g., Seol and Yamazaki, 1999; Randel et al. 2002, Tanaka et. al., 2004) stationary waves (wavenumbers 1-3) and transient waves (wavenumbers 4-7) play an important role in driving the BD circulation (or EP flux) in NH while in the SH most of the contribution comes from transient waves. The contribution of higher frequency waves to total eddy heat flux at 100 hPa is quite small but at higher levels they become more important. In the revised version we also mention that the 2.5 X 2.5 degree resolution data is used to calculate eddy heat fluxes, which will include contributions from both low and high frequency waves.

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