Atmos. Chem. Phys. Discuss., 6, S6858–S6863, 2007
www.atmos-chem-phys-discuss.net/6/S6858/2007/

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.

Received and published: 18 February 2007

(Reviewers comments are in italics)

Specific comments 1) p. 9565, line 5-10: The description lacks the relationship between temperature, WV at the TLS and HTF at the mid-latitude stratosphere. Different from ozone and temperature, variations in WV in the TLS (or at the tropopause level) is not controlled directly by BDC (Randel et al., 2004), but directly by the tropopause level temperature. Conversely, tropopause level temperature is controlled by BDC (e.g., Rosenlof et al., 1995). Related to the third point in general comments, I recommend the authors to clarify what is the new discovery in the paper and different from the two papers.

We agree with the reviewer. We have modified as follows: ”Another important source of stratospheric WV variability is the changes in tropical tropopause temperatures which controls the WV VMRs entering in to the stratosphere (Brewer, 1949; Rosenlof, 2003). The tropical tropoapuse point temperatures are not increased as would be needed for the stratospheric H2O increase (SPARC, 2000).”

2) p. 9566, line 8-10: The results and difference of Randel et al (2004, 2006) are unclear. As far as I remember, Randel et al. (2004) clearly showed the sudden drop of WV at the tropopause level, and presents tight correlation of interannual variations in WV and temperature including after 2000 at the tropopause level, and Randel et al. (2006) explained the drop of WV after 2000 by EP flux divergence.

Please see the reply to general comment 1. In this study we show the correlation between TLS WV and wave driving (not with cold point temperatures). We were more concerned sbout the interannual variability in the strength of the BD circulation and its impact on the TLS WV.

3) p. 9566, line 15: How is the squared measurement errors is defined? How different are the weighted profiles from raw profiles? Could you describe the reason why the WV profiles are weighted in that way, and why not use raw profiles? I recommend you to put the description of WV data set and analysis data into one additional section, such as Data and Analysis.

Each measurement is weighted with the inverse of square of measurement error. So measurements with high errors get less importance (weight) in the avaraging. Both SAGE and HALOE use the solar occultation technique and have, therefore, their largest uncertainties in the retrieval near the TTL due to presence of cirrus clouds/aerosols (see Thomason et. al., 2004 and Harries et. al., 1996). So, it is an obvious choice to give higher weight to the measurements with less error.

ACPD
6, S6858–S6863, 2007

EGU

4) p. 9566, line 22: Why do you discuss upwelling TLS and TTL? Why and how does upwelling at TTL affect upwelling and temperature at the TLS? According to my knowledge of downward control theory, upwelling and temperature at TLS is controlled by the HTF from the whole globe at the same level.

We clarify that through downward control stratospheric air is transported in to the troposphere at higher latitudes and this is compensated by the advection of tropospheric air at lower latitudes. This results in adiabatic heating (polar region) and cooling (tropics).

6) p. 9566, line 24: The description "TTL temperature ... into the stratosphere" is ambiguous about causes and results. Ascending motion at the TTL controls the TTL temperature directly, and as a result controls the amount of WV entering the stratosphere indirectly.

See answer to specific comment 4.

7)p. 9567, line 14: Why do you use tropical WV VMRs averaged between the levels of 16-20 km? Is the phase difference of WV tape recorder between the levels of 16 and 20 km small?

Averaging of WV VMRs between 16-20 km surely smears out the tape recorder. On the other side the WV data appears to be quite noisy and averaging over altitudes improves the analyses.

8) p. 9567, line 18: Do you calculate HTF using daily mean fields or monthly mean fields of NCEP data? In addition, is the upwelling of BDC estimated from HTF averaged over 45-75 deg at 50 hPa to a good approximation? Randel et al. (2006) shows the

ACPD
6, S6858–S6863, 2007

change of wave momentum flux (horizontal component of EP flux) equatorward of 45 deg seems to be large at the tropopause level. Now downward control theory is still discussed about whether mid-latitude EP flux divergence can induce upwelling at the TTL (Plumb and Eluszkiewicz, 1999; Semeniuk and Shepherd, 2001; Keer-Munslow and Norton, 2006).

We have added a data section. We clarify that eddy heat flux is calculated from daily data. We also discuss and justify the use of the 50 hPa extra-tropical eddy heat flux (see earlier reply). The estimation of momentum fluxes in the tropics are highly uncertain (Randel et. al., 2006). We also agree with the reviewer that the contribution from extra-tropical wave driving to the tropical upwelling is still under discussion (e.g. Plumb and Eluszkiewicz,1999; Semenuik and Shepard;2001; Keer-Munslow and Norton, 2006; Norton, 2006). These references are included in the revised version.

10) p. 9569, line 26-27: Refer to Randel et al. (2006), which present the levels (15-20 km) of temperature change after 2000. Furthermore, I recommend emphasizing the difference of results between Randel et al. (2006) and the regression analysis in the paper.

Please see reply to the general comment 1.

11) p. 9570, line 2-4: The authors mention the overestimation of the cooling above 70 hPa, which is partially due to solar effects. Could you quantify the change of HTF and the change of temperature? The quantified information is certainly useful and intriguing, because the increase of HTF (dFz) (and thus BDC) and the decrease of temperature at TLS (dT) can be quantified at each level based on the momentum equation. About the relationship of temperature and BDC at the TLS is theoretically inferred, assuming the vertical profile of radiative time-scale (e.g., Randel et al., 2002; Niwano et al., 2003). Does the result from the regression analysis disagree with the

ACPD
6, S6858–S6863, 2007

EGU

well-known relationship between dT (decrease of temperature) and dw (increase of upwelling)?

Our regression analysis results does not contradict that the increase in tropical upwelling leads to decrease in temperatures actually they are in line with our current understanding. As shown in Figure 4, increase in wave driving (BD circulation) leads to cooling in the tropical stratosphere.

12) p. 9576, Figure 1: eddy heat flux (bottom). The sentence such as "colors indicate the years" helps readers to understand easily."

Necessary changes have been done.

14) p. 9577, Figure 2: Why is TLS WV VMRs at the TLS for JFM used, but not for all the seasons? Why do the authors choose only northern winter time for scattering plot? In addition, why do the authors choose HTF averaged between September-February (6 month average), while using JFM TLS WV (3 month average)? Being different from ozone, WV at 15-20 km is expected to be correlated with HTF at the tropopause level from both hemispheres through the whole seasons and months with a certain phase lag of a few months.

As stated in the paper the anti-correlation is highest (in magnitude) for the months JFM. A three month averaging is used to minimize the sampling problem with the occultation instrument. The six-month average eddy flux reflects the cumulative effect of the BD circulation during SH spring and NH winter season.

16) p. 9579, Figure 4: Colors and position of the lines (bottom or top) can be noted in the caption so that readers can understand easily. Is temperature contribution from HTF calculated from based on HTF at 50 hPa?

For all the analyses we have used 50 hPa eddy heat flux and changes are made in the

ACPD

6, S6858–S6863, 2007

caption of Figure 4. Contribution from individual proxy is indicated in different colors.

17) p. 9580, Figure 5: Does the notation "NCEP re-analysis" indicate the temperature change between the periods July 2000-June 2005 and July 1996-June 2000 based on raw data of NCEP reanalysis? If so, horizontal bars can be drawn over the raw data of NCEP in Figure 4, in addition to horizontal bars over HTF components. The sentence might cause confusion that other profiles are calculated from using other data set.

As suggested Figure 5 has been modified.

18) p. 9581, Figure 6: Two horizontal bars (the temperature change of between the two periods) can be plotted for each data set. That information might be helpful for understanding.

We agree with the reviewer, but with three different colors already in use (four - including operational data), horizontal bars seem to cause more confusion than clarity, therefore, we opted not to plot them.

All specific comments made by the reviewer that are not mentioned here have been corrected as suggested.

ACPD

6, S6858–S6863, 2007