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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

Anonymous Referee #1

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Summary

The paper examined the relationship between water vapor (WV) in the tropical lower stratosphere (TLS) and the strength of the Brewer-Dobson circulation (BDC). The stratospheric WV analysis is based on different satellite measurements (HALOE, SAGE II, and POAM III). They showed the anti-correlation between the WV amount and the strength of BDC, and indicate the increase trend in stratospheric WV until late 1990s is caused by the decrease in the BDC inferred from planetary wave forcing at 50 hPa, and the sudden drop in the lower stratospheric WV after 2000 is in agreement with a sudden rise in eddy heat flux with nearly equal contribution from both hemispheres, which leads to 0.7 K cooling in the TLS.

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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.

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

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 multi-regression analysis, the result about a cooling of about 0.7 K by the HTF still remain robust? 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

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of WV had better be discussed in the results and clarified in the abstract, since the paper treats the relationship between WV in the TLS and HTF. 3) 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?

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).

Specific comments

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.

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

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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.

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".

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.

p. 9566, line 24: "TTL temperature controls" or "TTL temperatures control"

p. 9566, line 24: The description "TTL temperature Ěinto 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.

p. 9567, line14: 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?

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 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

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and Norton., 2006).

p. 9568, line 15: Where is the abbreviation “SOI” defined?

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.

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

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

p. 9576, Figure 1: “averaged” is doubling.

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.

p. 9577, Figure 2: “wintertime” should be replaced by “boreal wintertime” or “northern wintertime".

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?

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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9563, 2006.