

***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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(Reviewers comments are in italics.)

General comments: The variability of stratospheric water vapor as well as the mechanisms responsible for the observed variability are a topic of intense research for several years and there are still a lot of uncertainties. Particularly the sudden decrease in lower stratospheric water vapor after the year 2001 raised a lot of questions, since water vapor soundings in the 1980s and 1990s indicated a continuous increase in stratospheric water vapor. The topic and methodology of the present study are certainly appropriate for ACP. However, I think the paper needs further revision and clarification before it would be suitable for publication. 1) My major concern is that a similar study discussing the water vapor decrease after 2001 and the link to the

BDC was published in JGR by Randel et al. (2006). As far as I understand Randel et al. (2006) come to the same conclusion that the water vapor decrease after 2001 is related to a strengthening of the BDC. Does the present study provide any new scientific insights? Both studies are based on nearly the same data (HALOE, POAM, NCEP), the present study additionally uses SAGE water vapor instead of the Boulder balloon soundings. I miss a clear discussion of the current results with the study of Randel et al. (2006), differences, agreements, new insights. The correlation analysis (Fig. 2) shows a clear anti-correlation between tropical lower stratospheric water vapor and the eddy heat flux which is a measure of the strength of the BDC. However, I have some doubts whether this result is in agreement with the presented regression analysis of tropical lower stratospheric temperatures. The regression analysis performed in Section 3 (Fig. 4) indicates a cooling of about 0.7 K in the tropical lower stratosphere (70 hPa) due to a strengthening of the BDC. The authors used the 70 hPa level for their regression analysis, which is already slightly above the TTL. The NCEP data indicate a clear cooling between 100 and 70 hPa (unfortunately only two pressure levels are available), but the regression analysis shows a minor contribution from the BDC changes to the temperature changes in 100 hPa (also stated by the authors p 9571). Seidel et al. (2001, JGR) showed that the tropical cold point tropopause is located between 90 and 100 hPa. Therefore, I wonder whether the 70 hPa-temperatures are representative for the processes in the TTL. Since stratospheric water vapor is controlled by tropical tropopause temperatures, I suggest to perform a similar regression analysis using tropical cold point temperatures, e.g. the time series presented in Randel et al. (2006, Fig. 4).

We thank reviewer 2 for careful examination of our manuscript and useful comments. In the revised version we have clarified the differences between results presented in this study and those by Randel et al., 2006 (also see reply to general comments by reviewer 1). In this study the relationship between TLS WV, TLS temperatures, and extra-tropical wave driving and their inter-annual variability are investigated, we are not

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dealing with the cold point temperatures that has been extensively covered by Randel et. al. (2004, 2006) and others. The monthly mean temperature between 15S-15N at 70 hPa are representative of changes in TLS temperatures related to the rising branch of the BD circulation (adiabatic expansion). The years 1991 and 1997 show a departure from the anti-correlation relationship between wave forcing and tropical TLS water vapor, which highlights that not all changes in TLS WV can be attributed to changes in the BD circulation.

2) Furthermore, in the abstract the authors state that a decrease in planetary wave activity in the mid-nineties might be responsible for increasing stratospheric water vapor. Again, it would be interesting whether there is a relationship between strength of BDC and tropical cold point temperatures. Otherwise, this statement seems to be in disagreement with the observed decrease in tropical tropopause temperatures (Seidel et al., 2001; Zhou et al, 2001, JGR).

The main objective of this study is to investigate the relationship between extra-tropical wave driving and WV in TLS and their inter-annual variability. Although WV VMRs in TLS are controlled by cold point temperatures (Randel et. al., 2004, Fueglistaler and Haynes, 2005), the strength of the BD circulation controls the amount of (dry/humid) air advected into the TLS. In Figure 5, we clearly show the vanishing influence of BDC on temperatures in the tropopause region (100 hPa) and hence the influence of BD circulation on cold point temperatures could not be quantified here.

3) The regression analysis performed in this study quantifies the contribution of the BDC strengthening to the cooling in the tropical lower stratosphere, but not to the water vapor decrease. Fueglistaler and Haynes (2005, JGR) found the empirical relationship that temperature changes of ± 1 K near the cold point correspond to water vapour anomalies of ± 0.5 ppmv. Assuming a linear relation the cooling of 0.7 K presented in the current study would correspond to a water vapor decrease of approximately 0.35

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ppmv, which seems to be in agreement with the water vapor anomalies shown in Figure 3. I suggest to perform a regression analysis for tropical lower stratospheric water vapor in order to quantify the contribution of the BDC changes directly.

A good suggestion, but this paper is not about cold point temperatures. We state that temperature variability at 70 hPa can not necessarily associated with changes in dehydration mechanism but changes in strength of the BD circulation for most years. The relationship between WV and cold point temperatures has been studied for instance by Fueglistaler and Haynes (2005).

Specific comments:

3) p 9566, l 13: How is the eddy heat flux calculated? Which data are used? Overall, I recommend to add a section describing the analyzed observational data, reanalyses data and applied methods.

A data section has been added in the revised version.

4) p 9566, l 17 / Fig. 1: In Figure 1 WV reaches minimum in March/April, not in January/February. This might be related to the large altitude range of the shown WV measurements.

We clarify that due to the altitude range used for vertical averaging the WV minima are shifted to March/April.

5) p 9566, l 24: Ascending motion controls the amount of air entering the stratosphere. The WV content of the air masses is controlled by TTL temperatures.

The sentence has been modified as "Cold point temperatures control the dehydration mechanism while ascending motion control the amount of (humid/dry) air entering into the stratosphere."

6) p 9567, l 20 / Fig. 2: *How large is the anti-correlation between WV and eddy heat flux using northern hemispheric heat flux values only? I would expect that tropical upwelling during September-February is dominated by northern wintertime wave activity. At least, tropical tropopause temperatures are lowest during northern winter.*

We find that using eddy heat flux only from NH, the correlation is about -0.75 for HALOE data (as compared to -0.85 by combining both hemispheres), but for SAGE data it is only -0.36 (-0.68 for both hemispheres).

8) Fig. 2: *Why are POAM data not included in the correlation calculation? From Figure 3, I would expect a similar correlation between POAM and the eddy heat flux, applying a certain time lag.*

Only few years of data POAM data is available and performing such analysis would bias the results towards the late period.

9) p 9568, l 2: *Are the given correlation coefficients statistically significant?*

Significance of the correlation coefficients is included in the revised version.

12) p 9568, l 23: *How are the WV anomalies calculated? Did you subtract a climatological mean annual cycle?*

It is explained in the text.

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