

***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: This paper discusses the relationship between stratospheric water vapor, the strength of the Brewer-Dobson Circulation (BDC), and tropospheric wave forcing. The main idea is that a stronger BDC due to more tropospheric wave activity leads to colder tropopause temperatures, and to a drier stratosphere. This topic is relevant for publication in ACP; the mechanisms controlling the water vapor concentrations in the stratosphere have recently received a lot of attention, and is important for, e.g., ozone depletion. The results are interesting, but not novel. Furthermore, the structure of the paper should be improved. I suggest reconsidering publication after major revisions.

We thank reviewer 3 for his useful comments which helped us to add clarity to our paper. See specifically the reply to reviewers 1 and 2, who have raised similar concerns.

Specific comments

The results presented in this paper confirm the results presented by Randel et al., 2006: more tropospheric wave activity leads to a stronger BDC, colder tropical tropopause temperatures, and, through dehydration, to a drier stratosphere. The authors should clearly state which aspects of their results are novel. On a similar note, the difference between the mechanism described in p. 9565 l. 20-end (increased Brewer-Dobson circulation), and that in p. 9565 l. 15-17 (decreased tropical tropopause temperatures) should be explained.

We have expanded our discussion on the relationship between TLS temperature and the strength of the BD circulation. Also see our reply to reviewer 1 (comment 1)

1) The paper needs some restructuring. I suggest adding a section entitled "Data", in which more technical issues are discussed (e.g., p.9567 line 22-end). The discussion on the NCEP reanalysis (p. 9570, l 12- p. 9571 l. 6), could be described in an additional section entitled "Discussion"

We agree with the reviewer. In revised version, we have included a section entitled "Data", but the discussion about the results has been expanded in the respective sections.

2) p. 9565, line 4: 'Some observations': please specify

We specify that long term WV observations are available only from a single site at

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4) *Fig. 1: Discuss the time lag in the seasonal cycles of eddy heat flux (maximizing November-February) and the stratospheric water vapor (minimizing between February and March).*

We have expanded our discussion about the tape recorder signal. We also state that due to vertical smoothing assuming a vertical velocity of less than 1 km /month, these minima are shifted to the NH spring season.

5) *p. 9567, line 5: Please give more physical arguments for the high anti-correlation for the selected periods*

Please see expanded discussion in the revised version.

6) *p. 9567, line 1: Numbers of correlation should be given including and excluding data from 1997. The low anti-correlation in that year is not a valid reason for excluding it from the statistics, especially since the low anti-correlation is not understood.*

We have included correlation coefficients with and without the years 1991 and 1997

7) *Fig. 3: How are the anomalies calculated?*

Monthly mean anomalies are calculated using HALOE data for years 1992-2005.

8) *p. 9570, line 7-12: Superfluous. A cooling of the TTL has been directly associated with a strengthening of the BDC in previous studies.*

We disagree with the reviewer. Using selected radiosonde data Randel et. al., 2004 clearly showed that cooling trends near TTL (not in TLS) in NCEP data may be

questionable.

9) *Fig. 6: Since a large part of the discussion of the first part of Section 3 focuses on 70 hPa, this level should be included in Fig. 6.*

In the "Data" section of the revised manuscript we clarify that radiosonde data is not available at 70 hPa.

10) *p. 9570, line 26: Since the authors discuss differences between 1995-2000 and 2000- 2005, it is essential for the NCEP data to be reliable not only after 2000, but also between 1995 and 2000.*

We agree with the reviewer. We have modified sentence as "Comparison shows that there are no significant bias in the NCEP data with respect to the homogenized radiosonde data (especially after 2000)"

11) *p. 9581, Fig 6 caption: The bias of the ECMWF operational data is not really relevant for the ideas presented in the paper.*

The biases between ECMWF (ERA40 + operational) shown in Figure 6, highlights the problem with the meteorological analysis, particularly near the TTL.

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

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