Atmos. Chem. Phys. Discuss., 7, S7658–S7663, 2007
www.atmos-chem-phys-discuss.net/7/S7658/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term climatology of air mass transport through the Tropical Tropopause Layer (TTL) during NH winter" *by* K. Krüger et al.

K. Krüger et al.

Received and published: 14 December 2007

Answers to the anonymous reviewer # 1:

We would like to thank reviewer 1 for his valuable comments, which helps to improve our paper. A detailed point by point answer and on the general comments is given below.

General comments:

1. "The calculated radiative diabatic heating combined ...presented here.":

According to many other chemical transport model (CTM) studies using radiative diabatic heating as vertical velocity e.g. the SLIMCAT model or CLaMS, the success-

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ful simulation of transport processes is demonstrated in the tropical tropopause and stratosphere, where the violation of air mass conservation is not a topic. Publications using these CTMs demonstrate the great advantage and the usefulness of these model calculations using diabatic heating rates w/o taking explicitly care of the mass conversation in the TTL. Our study is concentrating on the cold point tropopause and above, which is placed above the net zero heating point so we are governing the stratospheric overworld.

2. "The elimination of spurious transport... by this work are close to the true value.":

In our study we never state that using our approach is giving the true value, we just say that it is more realistic. There are also concerns which could affect our heating rates, which are obvious. Especially the accuracy of the input data for the radiation code e.g. temperature, ozone and cloud properties are having an impact on the calculated heating rates as was demonstrated for polar latitudes by Tegtmeier 2007 and for the tropics by e.g. Mc Farlane et al 2007. Nevertheless we believe, that the approach of using diabatic heating rates is closer to reality as using the noisy and too strong vertical winds which was also demonstrated by several other studies (e.g. Schoeberl et al 2003, McKenna et al., 2002; Chipperfield, 2006; Konopka et al 2007, Immler et al 2007a, Tegtmeier et al., 2007). In that sense using the phrase more realistic is a fair statement.

Indeed the paper by Mc Farlane et al compares heating rates derived from ARM observations and from two different GCMs by the offline radiation code from Fu-Liou. Mc Farlane et al conclude that especially the different input fields for the cloud properties are having an impact on the radiative heating rates. The simulated clouds by the two GCMs showed larger deviations from observations over two tropical Western Pacific stations, impacting of course the vertical profile of the heating rates. The simulation of clouds in ECMWF is already addressed by our companion studies from Immler et al 2007a/b (JGR and ACPD), who showed the surprisingly good performance of the assimilated 4D-VAR ECMWF cirrus cloud occurrence compared to observed cirrus

ACPD
7, S7658–S7663, 2007

clouds over the tropics and also over mid-latitudes. Comparison of ERA40 analyses with tropical campaign measurements also revealed an astonishingly good correspondence compared to a ship based campaign in the tropics (presentations by Krüger et al at the SCOUT-O3 annual meeting, 2005 in Zuerich). Concluding for this point 2, we do not want to give the misleading impression reviewer 1 got. We will widen the discussion of these uncertainties into our paper and will correct the text due to the Mc Farlane citation.

3. a) The conclusions are changed to that the composite of: Volcanic eruptions are listed as a new result and the QBO and ENSO results are moved before the new result list adding that in contrast to F&H 2005 more years are included in the analyses showing the following results. As a new result the derived zonally averaged diabatic ascends in the upper part of the TTL over different time periods is added.

3b) "Also note QBO-induced temperature anomalies occur in phases of easterly/ westerly wind *shear*, not east or west phase (or perhaps QBOE/QBOW has this meaning?":

For our composites, we are taking the phase (zonal wind) of the QBO between 50 and 40 hPa (e.g. Labitzke et al). With this approach we are taking direct (in the tropics via the thermal wind balance) and indirect effects (via a different strength of the BDC caused by different extratropical wave driving $>$ Holtan and Tan mechanism) of the QBO into account. As the QBO is not so clearly defined around 100 hPa it is hard to calculate a phase or a shear zone index at these lower levels. Therefore we have chosen a pressure level commonly used as the QBO reference. We also tested the impact of using other reference levels e.g. 70 hPa, which did not change our QBO results. The details are added in the paper.

4) "The authors link TTL temperatures... This aspect deserves more discussion.":

We are aware of the temperature biases in ERA40 data, which is clearly stated in the discussion. The drop of water vapour in 2000-01 is added. As we indeed do not show

ACPD

7, S7658–S7663, 2007

a difference between the late 1990s and the early 1990s we change our text to: during the 1990s and early 2000s a colder and drier TTL is predicted together with an increase of zonally averaged diabatic ascent during the early 2000s. This will be also changed in the other text phrases. However, there is clear evidence that the NH wave driving has increased since the late 1990s compared to the early and mid 1990s (e.g. Manney et al 2005 and others).

- "Of interest would be also a comparison of results for pre/after 1979 (satellite era).":

Results of the pre and after satellite era will be added in text without showing the pictures. We want to avoid the discussion on trends and pre and after satellite era due to the shortcomings in the ERA40 analyses.

Specific comments answered point by point:

P13990/L8: Mote et al 1996 shows the tape recorder from 100 hPa upward. From the Mote et al study figure one can deduce the diabatic ascend for the lowermost levels which should have some overlap with our cold point to 400 K layer. However, we will cut the reference out.

L10: The lowermost part of the BDC within the TTL. Words are rearranged.

P13992/L15: even is cut out. TTL definition is already given.

L25ff: We used this sentence to clarify the differences between the Lagrangian and the Eulerian approach which was unclear to a previous reviewer. We will cut this sentence out according to reviewers 2 suggestions.

P13993/L11: Tegtmeier (2007) and the submitted JGR paper by Tegtmeier et al. 2007 did this intercomparison study. Words will be re-arranged.

P13994/L19: We have also carried out clear sky calculations for several case studies (e.g. Figure 1), which did not show big differences in the distribution of CPT nor the frequency. The cloud effects are added in the text and in the discussion.

ACPD

7, S7658–S7663, 2007

P13995/L13: They refer to TS ensemble of trajectories as is clearly stated in section 2.

P13997/L23: We are using the prognostic ozone fields from ECMWF analyses.

P13998/L5 and P14000/L1-4: These results belong to Diplom thesis by Wiebke Brunn (2007) which is available online. The reference is given below and will be added in the paper.

P14002/L10: The differences are smaller compared to the other indices; however the agreement between this study and other solar cycle related model studies (w/o ENSO forcing) in the tropical tropopause indicate relevance of these results (personal communication Kodera 2007 and see also the given references in the paper).

-"Is there perhaps a fortuitous correlation of the solar cycle with ENSO over the period used here?"

The signal over the eastern pacific would hint towards a La Nina influence on both solar cycle composites, which can be not found for the solar maximum composites and for solar minimum (n=18), 3 El Nino and 4 La Nina events happened simultaneously. So we do not think that an overlapping correlation exists but we can not rule this out. See our comment in the existing conclusions.

Companion references

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ACPD

7, S7658–S7663, 2007

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13989, 2007.