Atmos. Chem. Phys. Discuss., 7, S6080–S6084, 2007
www.atmos-chem-phys-discuss.net/7/S6080/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

Anonymous Referee #2

Received and published: 17 October 2007

General

This paper presents a climatology of transport across the TTL based on a Lagrangian analysis using isentropic surfaces as the vertical coordinate and heating rates as the vertical velocity. The authors provide strong arguments that this procedure has advantages compared to using trajectories based on a pressure (p) grid using $\omega = \dot{p}$ as the vertical velocity. Using this concept, they are able to discuss the modulation of the cold point temperature by ENSO, QBO and solar cycle. These topics are clearly of interest to the ACP readership. I recommend publication after a revision taking into account the comments below.

The concept of $\hat{Q}_{\mathrm{LCP}}$ is introduced and used frequently throughout the paper. This concept is immediately relevant for water vapour in the TTL and in the stratosphere as it measures the diabatic ascent since crossing the cold point. However, in the discussion of this quantity little effort is made to link $\hat{Q}_{\mathrm{LCP}}$ with observed water vapour trends and variability in the stratosphere (see e.g., Randel et al, 2006, and references therein, cited in the paper). On the other hand, the $\hat{Q}_{\mathrm{LCP}}$ concept is used when discussing the fate of VSL species in the TTL. Here, I am less convinced that the concept is useful. The cold point is not so relevant for the chemical degradation of VSL species; for this purpose a concept like an integrated chemical processing 'strength' along the trajectory would be more useful. I suggest to extend the discussion of stratospheric water vapour variability (at least provide better links to the existing literature) and to clarify the discussion regarding VSL species.

Further, I suggest to try to go a bit further in the analysis of the variability imposed by natural forcing components. For example, the QBO introduces a secondary circulation that might well be connected with the LCP patterns found here.

In the Figures, I note that there are some LCPs around about 30° that are larger than 200 K. I suggest some discussion on this point. How much larger will be the saturation mixing ratio than for the colder cold points? Is this water vapour input into the stratosphere really negligible?

Detailed remarks

Abstract: The abstract is well written. However, it contains terms (tape recorder) and acronyms (ENSO, QBOE, etc.) that might not be known to everyone potentially inter-

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ested in this paper. For example, ‘diabatic ascent rates deduced from observed H2O signatures’ (or similar) might be better than ‘tape recorder’.

Abstract, l 15-16: Quantify (in %) how much drier the TTL is during QBOE.

p. 13990, l. 23: ‘entry points’ of individual air parcels is of course an idealised concept, as air parcels are mixed during the transit from the troposphere to the stratosphere, which also does not occur at a ‘point’ like Caesar crossing the Rubicon. . .

p. 13990, l. 23: ‘representation’? where? in models?

p. 13992, l. 12: Konopka et al., ACP, 2007 have applied this approach also in the TTL region.

p. 13992, l. 23: larger than what?

p. 13993, l. 1-2: To me, what is stated here seems obvious for any trajectory model – what is the point here?

p. 13993, l. 10: Reference for Fu-Liou scheme?

p. 13993, l. 14: which are the two calculations – unclear.

p. 13993, l. 23: What is a saturation mixing ratio tropopause?

p. 13993, l. 25: What is the reason for preferring the Sonntag formulation over other formulations. Would the conclusions differ if other formulations would be used?

p. 13994, l. 1: I believe what Fig. 1 shows is the LCP temperature at the location where the cold point was reached along the trajectory, irrespective of the altitude at which the LCP was reached. I suggest to be very clear about this issue (if I am right. . .).

p. 13994, l. 24: 40 is an extremely gracious definition for a *tropical* belt; isn’t 40 already

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mid-latitudes?

p. 13995, l. 3: 'vertical winds', well these are reported as pressure tendencies in the ECMWF. Therefore it should be explained here, how this information is converted into $\dot{\theta}$.

p. 13995, l. 13: Here and elsewhere: do not report percentages to two significant figures. This gives an incorrect impression of the accuracy of these numbers.

p. 13997, l. 1: focused $\longrightarrow$ concentrated/dense.

p. 13997, l. 12: bias, but which data set is 'correct'?

p. 13998, l. 1: insert 'e.g.' in front of Krueger 2005.

p. 13999, l. 7: briefly discuss the 'physical mechanisms' rather than just refer to the Kodera paper.

p. 13999, l. 8 and 18: Do not use 'e.g.' if you refer to a specific paper.

p. 13999, l. 17: systematically

p. 13999, l. 21: What is meant by 'inhomogeneities' of the ERA40 series? Probably errors. Be more precise here.

p. 14000, l. 3-4: This piece of information is not available in the literature yet. I suggest to include it here, where it would make an important point relevant to the paper. The number of figures in the paper as it stands is certainly not to large.

p. 14001, l. 22: is it the TTL or the LCP? The paper is focusing on the LCP and not on the entire TTL...

p. 14002, l. 8: How much 'colder'? Quantify the impact of solar variability here in the

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Conclusions.

p. 14002, l. 15: ‘taking air mass sampling into account’ What is the point here? Unclear.

p. 14002, l. 16-17: Personally, I do not like the idea of ending this paper with a statement about what it can not deliver (and what is therefore postponed to future studies). The paper has points enough to make and I suggest to focus on these here at the end of the conclusions.

Figures 1 and 3: If I understand correctly, the LCP temperature is shown here at the location where the cold point was reached along the trajectory, irrespective of the altitude at which the LCP was reached. I suggest to be more clear about this issue and to not ignore the altitude information completely. Could it not be important if the LCP is located at different potential temperatures for different regions? Further, I suggest to distinguish better between differences in LCP caused by sampling issues and those caused by the actual differences in the opECMWF and ERA40 temperature fields.

Fig. 4: This figure shows a lot of detail that is not well visible if the ACPD version is sent to a printer. In the ACP version it should be made sure that the Figure is readable in a printed version.

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