Atmos. Chem. Phys. Discuss., 9, C2903–C2906, 2009
www.atmos-chem-phys-discuss.net/9/C2903/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of residence time in the Tropical Tropopause Layer during Northern Hemisphere winter" *by* K. Krüger et al.

Anonymous Referee #1

Received and published: 17 July 2009

The paper by Krüger et al. is an extension of a study published in ACP in 2008. This study uses backward trajectories to calculate air mass transport through the TTL up to a potential temperature level of 400 K. The paper in 2008 presented typical diabatic heating rates calculated for the TTL based on ERA40 ozone, water vapour, temperatures and cloud parameters. Instead of using the vertical wind from the ERA40 data set, which is known to be very noisy and show too high vertical velocities, Krüger et al., 2008, used offline calculated diabatic heating rates. The present study uses the same set of trajectories to study the residence time in the TTL. In so far as the residence time in a potential temperature layer is easily derived by dividing the potential temperature range by the diabatic heating rates, the transit times themselves can only be regarded as simple extension to the previous work. The more interesting part of the paper is the look at variabilities and factors influencing the residence times.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The paper is well written and organised and the scientific questions are relevant to ACP. However, I have some comments on the paper which I suggest that the authors should consider in a revised version of the manuscript.

My major comments are related to the calculations themselves and the conclusions which are drawn from the study. The authors derive transit times from the trajectories presented in Krüger et al., 2008 and discuss variabilities of these transit times. However, they present these model calculated transit times as being the actual transit times without a thorough validation using observations. Some possible mechanisms, like e.g. mixing (Konopka et al., 2007) and radiative heating in cirrus clouds (Corti et al., 2006) which have been discussed recently are not included in the calculations. Such mechanisms could play an important role near the bottom of the TTL. I suggest that the authors should include a discussion on a comparison between mean transit times derived from CO2 observations (e.g. Boering et al., 1994, Andrews et al., 1999) (see also Sherwood and Dessler, J. Atmos. Sci, 2003) to put more convidence in the model derived residence times.

Furthermore the authors present variabilities and discuss the influence of different factors and possible long term changes. To my feeling it would be necessary, especially for possible long term changes, to see a sensitivity study, showing how reliable the calculated heating rates and thus transit times are. For instance uncertainties in ERA40 ozone, water vapour, cloud cover, temperature etc. are certain to induce uncertainties in the calculated heating rates. I suggest including such a sensitivity study, which should aim at estimating the uncertainty in the derived residence times and could serve to better understand if changes derived from the ERA40 data set are statistically significant.

Detailed, minor comments.

Section 1, Introduction: The study is restricted to the Northern Hemishere winter months. I suggest to include an explanation of this choice.

ACPD
9, C2903–C2906, 2009

Interactive Comment

P12601. l. 1: Even after reading this section several times I still do not understand how the ascent rates are mapped geographically. Is only the portion of the trajectory within the grid-box used or are trajectories associated with the grid box in which they were initialised? Any parcel ascending in the TTL will also travel horizontally, so I suggest adding a more detailed explanation of how the trajectories were mapped geographically. The same is true for the LCP.

p. 12602., l. 22: what is meant by τ for the upper TTL here? Is is τ380-400K?

p 12603., l. 18: replace faster by shorter

p. 12605., l 5: replace faster by shorter

p. 12605., l. 29: as this is a very important point, I suggest to summarize the main conclusion from K08 here. Also, as stated above, I believe that a sensitivity study on this issue would be worthwhile. When looking at Figure 3 it is obvious, that the conclusions strongly depend on the time period considered and a possible negative trend in residence times between 360 and 380K is dominated by consistently high levels between 1962 and 1975. I am sure that the observational data base for the ERA40 was different during that time then for more recent years.

p. 12606., l. 12: What is meant by maximum correlation in this respect? I suppose that maximum refers to the pressure level for which the EP Flux is calculated. please explain more clearly.

p. 12606., l 18: I find the wording “diabatic upwelling” somewhat misleading. If the extratropical planetary wave activity induces the upwelling, then the diabatic process balances this upwelling and is thus a reaction to the upwelling and not, as might be suggested by the term diabatic upwelling the driving mechanism. I suggest to replace by forced diabatic ascent, as in Fueglistaler et al., (2009).

p. 12606, l. 25: again, what is meant by maximum anticorrelation?

p. 12607.l, is there a reason that this correlation is only shown for the 360-400 K levels?

It would be interesting to see if the anticorrelation is stronger in the upper part of the TTL than near the bottom.

p. 12608, l.22: I suppose the winter 2001-2002 is meant.

p. 12608, l. 24: I found no discussion explaining this conclusion in the paper.

p. 12609, l.1: I find it hard to derive this conclusion from what is presented in the paper.

p. 12612., Fig. 2, caption: I suppose that 5 d centred time bins (not Theta bins) are meant.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12597, 2009.

ACPD

9, C2903–C2906, 2009

Interactive Comment