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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 #2**

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This paper extends the work of Krueger et al 2008 which calculated residence times in the TTL using a new diabatic trajectory code. In the new paper more details of interannual variability are given and also there is a greater emphasis on results for the 360K-380K layer, as well as for the 380K-400K and LCP-400K layers which were the focus of the 2008 paper.

Taking an overall view, it is useful to see the extra results presented in this new paper, not least because they provide a first impression of how large the effects of interannual variability on transport through the TTL are likely to be.

However, I feel that there are various aspects of this paper that could be clarified and I also feel it is important to consider new information that might not have been available when this line of work was started.



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One overall point is that the lower in the TTL being considered, the larger the potential role for non-radiative diabatic effects, principally latent heating. While calculating diabatic trajectories on the basis of radiative calculations (albeit radiative calculations including cloud effects) might be a very good approximation in the 380K-400K layer, it is less self-evidently a good approximation in the 360K-380K layer. Some estimates of the magnitude of latent heating have been given by Fueglistaler et al (2009, Quart. J. Roy. Met. Soc, 135, 21-37) and the implications of latent heating for trajectories has been considered by Ploeger et al (2009, paper submitted to J. Geophys. Res.). Note that what is actually considered in these two papers is 'residual heating' – i.e. diabatic heating minus radiative terms – but that is also relevant. Note also that whilst the magnitude of this heating might appear to be small (e.g. from Fueglistaler et al 2009 Figure 2f) the fact that vertical gradients of potential temperature tend to be smaller in the lower part of the TTL than in the upper part meanm that the implications for trajectories cannot be neglected out of hand. So I feel that some comment should be given on this point in any revised paper.

Detailed comments:

l7: confusing sentence – better as 'The residence time tau_{LCP-400K}, being the duration time for air parcels in the layer between the Lagrangian Cold Point (LCP) and 400K, varies spatially and is longer (> 50 days) over the maritime continent as the LCP is lowest there (< 370K).'

p12598 l13: omit comma

p12598 l17: 'The interannual variability is influenced ...' – sentence seems unnecessary.

p12598 l24: 'large potential for depleting stratospheric ozone' – 'large' overstated? ('large' relative to what?)

p12598 l26: 'bromocarbons' – since multiple species?



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p12599 l4: 'plays a dominant role' is a bit vague – and I'm not convinced that Holton and Gettelman (2001) showed that the maritime continent played a dominant role e.g. in mass transport. What they did point out, importantly, was the potential role of horizontal advection in ensuring that parcels moving from troposphere to stratosphere experienced cold temperatures. The fact that, say, the mass flux from troposphere to stratosphere is relatively concentrated in the maritime continent region, or that the much of the air that reaches the stratosphere has come from the lower troposphere in the maritime continent region is suggested by other papers – e.g. those based on trajectory studies. (Your own paper is part of the continuing effort to assess what is robust and believable about the trajectory studies.)

p12599 l20: For 'density of trajctories' to mean anything you need to give a few more details – e.g. 'density of 380K crossing points of trajectories that reach the stratosphere'

p12600: To confirm – Q is simply Delta Theta/ residence time (for each trajectory)? So for 360K-380K and similar layers the statistics of Q is completely determined by the statistics of residence time (and vice versa)?

p12601 l18: A clear statement that 'tilde' means zonal mean might be useful here. My general view – this comment and previous comment – is that in several places you could use a few more words to make notation absolutely clear.

p12602 l8: It is not very satisfactory to have 'contradictory' results. If 1992-2001 was cold relative to 1962-2001, this could have been due to colder 'radiative equilibrium' temperatures (perhaps due to different temperatures in the underlying troposphere or different concentrations of radiative species) or it could have been of a greater difference between actual and 'radiative equilibrium' temperatures – implying greater heating rates. Your heating rate calculations seem to rule out the latter, so the question is whether the former is an explanation? Do you really have enough confidence in records of temperatures, radiative trace gases, clouds etc over the 1962-2001 period to regard the 1962-2001 vs 1992-2001 difference as something that can be believed?



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One option would simply be to omit the 1962-2001 averages. (The 1962-1991 period could be still be included in Figure 3, since the focus here is year-to-year changes rather than small differences in multi-decade averages.)

p12605 l24: 'must' seems too strong – 'might'?

p12606 l12: what do you mean by 'shows the maximum correlation'? – have you considered correlations between many different quantities and chosen these two quantities as exhibiting the strongest correlation? Many previous authors have chosen to restrict stratospheric data analysis to periods significantly less than the 1962-2001 period which you use, sometimes restricting the period to post-1979 when stratospheric satellite data was used in the analysis. Can you justify using pre-1979 data?

Figure 4: I think the data period and more details of the EP flux (e.g. integrated over range of latitudes) should be mentioned in the caption. Certainly there should be clarify on what months are used – the caption is clear but the labelling of the axes is not.

p12606 l26: You say 'the subtropical wave driving' but do not define what exactly is calculated/plotted, so further details are needed. It would be best if you used some measure that has been justified previously – e.g. something used in one of the previous Randel et al papers on this topic, but perhaps omitting the velocity tendency term.

p12606 l27: This paragraph seems rambling and unfocussed. The important point is that subtropical wave forcing determines tropical upwelling (as discussed by various previous authors) and you are showing that there is a negative correlation between some measure of subtropical wave driving (further details to be supplied) and residence times.

p12608 l14: I don't really see why the trend towards shorter residence times should correspond straightforwardly to the trend in LCP tropopause height. You have already noted 'an increase of the tropical tropopause height .... could be connected to a stronger tropospherically induced upwelling' on p12605 l22 – I don't really follow



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that either. Why 'tropospherically induced'? You should either provide one or two references which include arguments to back up these ideas, or else postpone making this connections until such arguments are available.

p12608 l26: 'maximum density of LCP trajectories' needs explanation – see previous comment on this. What exactly are 'LCP trajectories'?

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