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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Transport timescales and tracer properties in the extratropical UTLS" *by* P. Hoor et al.

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We thank the reviewer for the review and her/his constructive suggestions. Below are the detailed comments to the referees suggestions.

1. The conclusions could be sharpened a bit. What is the main point of this paper? To define timescales? What feature should the reader take away?
We changed the conclusions and emphasized in the first paragraph the role of t_{TST} for the distribution and potential chemical impact of any tracer in the LMS. The distributions of CO mirrors this and therefore shows the tropopause following distribution whereas H_2O is controlled by the LCP in the tropics, which in turn is largely decoupled from TST. As a result both tracers show different properties of the underlying transport. We further changed the bullet points in the conclusions and also reformulated the

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abstract to sharpen the main points. The outcome of the study is to think about the lowermost stratosphere as a transition region, which is goverened by the temporal structure of TST. This structure is a function of horizontal and vertical distance relative to the local tropopause.
This structure in turn determines the chemical composition of the the LMS for any tracer with a finite photochemical lifetime and thus it's potential range for chemical impact. The distribution of CO is a direct result of this fact.
The second focus of the paper is on the systematic difference between tracers with finite chemical lifetime and water vapour, which is long-lived, but strongly related to temperature. However, both tracer have been used to determine properties of the ExTL. We want to emphasize, that both tracers indicate totally different properties of transport and are distributed differently throughout the LMS. Therefore they are not analogous, when e.g. determining properties of an ExTL such as ExTL depths or the range of tropospheric influence in the stratosphere.

2. It would be nice to also show a scatter plot of H2O$_{LCPsat}$ v. t$_{TST}$ that would correpond to a H2O v. CO scatterplot from observations.
The idea of a scatter-plot of H2O$_{LCPsat}$ v. t$_{TST}$ is a very appealing idea. However, H2O$_{LCPsat}$ does not resemble H_2O as observed since it is a mixture of stratospheric air and dehydrated air from TST. Since we don't have exact information on the stratospheric H_2O-fraction we did not include the correlation plot. However, we included a new Figure of the ACE-FTS data, which have been filtered to account for stratospheric CO and H_2O values only, which shows a much better agreement between the tracer isopleths and the trajectory properties H2O$_{LCPsat}$ and t$_{TST}$.

3. There are some aspects of the figures that could be improved, and a few points that need to be clarified as I outline below.
Specific:

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Abstract: define TST please
Done

Abstract, last line: "encountered TST" is probably not the right english verb. "encountered the tropopause" would work.
The abstract has been changed.

Pg 1, last paragraph, first sentence: "Trajectory experiments that have investigated .... have mostly focused on distinct processes and regions..."
Done

Put a tropopause on figure 1?
Figure has changed and only stratospheric data are shown (see also reply to reviewer 2).

Pg2, column 2, 2nd paragraph, 1st sentence is awkward. How about: "investigate, using a Lagrangian approach, the relation between transport time and temperature. Both transport time and temperature affect CO and H2O in the stratosphere.
Changed: In this study we use a Lagrangian approach to investigate in particular the relation between transport time and temperature, which both affect the abundance of CO and H_2O in the stratosphere.

pg2, section2 (set up): what is the impact of using a different year? Can you estimate somehow or provide a reference? How sensitive are your results to interannual variability?
This is a point which we cannot quantify from our results. We cannot exclude potential effects from variations of the tropospheric circulation such as the QBO or ENSO, which

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might be related to variations of STE (e.g. Zeng and Pyle, 2005). However, a similar experiment carried out for the period of 2002 and 2003 with ERA interim data shows a very similar structure.
Further, our results seem to be relatively robust given the observational evidence provided in Fig. 11, which use a combination of CO data from different years over Europe. Note also, that the ACE-FTS data combine different years and show a good agreement of the tracer structure to the corresponding trajectory parameters.
CO measurements in published literature (e.g. Hoor et al. 2004) confirm the temporal structure of the LMS independent from year and the strong link to the local tropopause. It would be a study of it's own to investigate variations of the structure of t_{TST} and thus tropospheric influence and it's relation to interannual circulation patterns.

pg3, column 2: how do you know that vertical dispersion will have only a minor effect? Can you provide a reference? Has anyone used this analysis before?
A systematic investigation of vertical dispersion for the T799L91 ECMWF operational analysis, which we used is missing. Therefore we can only reference the few systematic studies comparing different vertical transport scenarios and driving wind fields as given in the manuscript. We added references to Meijer et al., 2004 and Monge-Sanz et al., 2007, who demonstrated the improvements of the stratospheric age structure and thus the vertical as well as horizontal transport from the 4D-Var assimilation scheme. Our own unpublished results with ERA-interim data for 2002 and 2003 show a very similar structure providing some credibility that our results are relatively robust.
Note that a high vertical dispersion could have potentially affected the values of $H2O_{LCPsat}$ leading to higher values in the upper LMS and lower values in the lower LMS. This however, would have a minor affect on the structure of the H_2O isopleths since the quasi-horizontal component of transport is stronger. For t_{TST} a stronger vertical dispersion would destroy (or at least smooth) the tropopause following structure. The fact, that the tropopause following structure appears and is even more evident in the newly included equivalent latitude plot shows that the vertical dispersion is of

minor effect in our experiment. It clearly does not limit the results.

pg5, column1, 2nd paragraph: you mention a tropopause following layer in Figure 3, but there is no reference to the tropopause on the plot. Could you show the mean (and std deviation in lines maybe) of the tropopause in theta-latitude space on these two plots. That would prove your point better.
We added tropopause information.

Figure 5: Units are labeled as K but units are deg C. I suggest changing the legend to K.
Done.

pg 8, column1, 3rd para (Fig7): It would be good to note that the positive skewness in Figure 7 indicates (if I read the difference correctly) that the parcels undergo their last saturation (T_{LCP}) in the troposphere before crossing the tropopause. It might be worth mentioning the fraction of parcels that are dehydrated in the tropopshere.
We added a sentence to the text, which highlights the order of events as indicated in the caption of Fig.7.

pg 8, section 4.1: The statement "Since the max H2O in an air parcel.... rather than an enhancement." I do not think is true: THe background water vapor of parcels that do not undergo TST (and come from the overworld is likely lower (since it may have come from the colder tropical tropopause), so even parcels with T_{LCP} of -73C (200K) in the extratropical LMS will be increasing H2O. Please rephrase.
It is correct that air parcels with T_{LCP} of -73C (200K) will increase water vapour. Here we want to emphasize the conclusion from Fig.6, which indicates, that most of the trajectories undergoing TST have encountered $T_{LCP} < 192$ K (-81C) in the

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tropics. Notably these low temperatures are encountered already at Θ=350 K (Fig.6 blue colours). These trajectories potentially dilute H_2O from regions with higher temperatures at the tropopause (in addition to those from higher isentropes). We reworded the sentence.

pg 8, column 2, apra 2: "maximum for dehydration": see comment above. I do not think this is necesarily dehydration. $H2O_{LCP,sat}$ is about 7-10ppmv which would be an enrichment over the overworld.

The expression 'maximum for dehydration' refers to the black contours in Figure 8, which indicate the region, where TST trajectories encounter their T_{LCP} and is basically a subset of contours in Fig.6 as stated in the text. From Fig.6 one can directly deduce the according T_{LCP}, which is clearly higher than in the tropics and around 200K, corresponding to 8.5 ppmv at 200 hPa. The point is that the *spatial distribution* of T_{LCP} and thus dehydration shows a secondary maximum at high latitudes in winter. These trajectories are dehydrated, but of course to higher absolute values of $H2O_{LCP,sat}$. We added the term '... secondary maximum for *the location of* dehydration...'

Pg 11, 2nd column, 2nd to last paragraph: The 90 day limit might still be a cutoff because you are only looking at trajectories with TST: there might be more if you run longer, but in general I think the interpretation is correct.

This is true, here we only want to emphasize that the shape of the profile is not affected by this threshold.

Pg12, 1st paragraph: what about the seasonality of the 'kink' in CO or t_{TST}? Is the layer thicker or thinner? Do the gradients change? Could you plot the slope points for summer?

The summer data do not show a significant difference of the 'kink' in the $\Delta\Theta$-profile of t_{TST} since it shows the transition at a similar distance to the tropopause. The fact

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that t_{TST} appears to be shorter in summer is already evident from Fig.3 and the new Fig.12. We therefore did not include a new Figure here, but added a comment on this behaviour to the text.

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