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Atmospheric Chemistry and Physics Discussions

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 constructive comments and the suggestions, which helped to improve the paper.

1) General question: The results here focus on analysis of trajectories that experience TST, but it is unclear to me how big of a fraction this represents (compared to the number that remain in the stratosphere). This is an important point for comparing TST statistics with observed H2O and CO distributions. For the parcels that do experience TST, what is the primary mechanism for the systematic increase of PV?
We agree that the information of the fraction of air parcels is an important information and added contours showing the percentage of air parcels undergoing a TST to Figure

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3. It is evident, that the fraction also shows a tropopause following structure. Thus, it supports our interpretation of the CO-kink as being a result of the convolved effect of rapid and frequent mixing close to the local tropopause, but longer timescales with less air parcels at larger distances.
We also added information about the tropopause to Figure 3, which shows, that not a wrong definition of the tropopause leads to the gradient change (see also comment to 3)

2) Regarding the water vapor calculations: My overall impression is that the calculated saturation mixing ratio results (Figs. 8-10) are much more isentropic that the observed behavior (Fig. 1), especially during winter. No direct comparison of calculated and observed H2O is presented, but the patterns (and magnitudes) look quite different to me. These differences may result from a significant fraction of parcels in the lowermost stratosphere not being associated with recent TST (see comment 1 above), or other uncertainties. An idealized calculation to test the sensitivity of including non-TST parcels (Fig. 9) shows relatively small differences regarding this too-isentropic behavior. These significant differences between simulated and observed H2O structure suggests there could be substantial uncertainties in interpreting these TST calculations. I suggest the authors discuss these points and the associated uncertainties in more detail.
2) We thank the reviewer for that point. The structural difference between the ACE-FTS observations and the $H_2O_{LCP,sat}$ arose from the fact, that the trajectory parameters were deduced from stratospheric data only (PV>2 PVU). The satellite data were not filtered and thus included tropospheric data. In the revised paper we changed the ACE-FTS data accordingly and filtered the observations for stratospheric data only. The revised observations show a much more isentropic structure of H_2O as well as a stronger contrast to CO, thus corroborating our conclusions.
Absolute H_2O deduced from the trajectories is lower than the observed values, but a perfect match can't be expected since we compare multiannual ACE-FTS data with

ACPD
10, C6047–C6051, 2010

a single day assuming a constant stratospheric background-H_2O. Further we don't resolve microphysical and small scale processes with the trajectories, which may lead to higher values for the observations. The more striking is the good structural agreement between the highly idealized experiment and the observations.

3) The interpretation of the transit time distribution and CO structure (Fig. 11) suggests a kink about 30 K in potential temperature above the PV=2 tropopause. Can the authors suggest a physical explanation for the presence of this kink (why should the transit time distribution reveal any discontinuity?). In particular I wonder if the kink might be associated with the (statistical) location of the thermal tropopause, which could present some barrier to transport and exchange? As the authors are aware, there are complementary perspectives on the chemical structure of the ExTL regarding relevance of the thermal vs. dynamical tropopause. For example, two issues are that tracers often appear more compact in thermal tropopause coordinates, and PV in this region is not well conserved even for relatively short time scales (as demonstrated in the calculations here). While this is still an area of active research and discussion in the community, helping to consolidate these different perspectives would be a very useful addition to this paper. Accordingly, I encourage the authors to discuss their transit time results also from the perspective of the thermal tropopause.

3) The relatively aprupt change of the transit time distribution is indeed somewhat surprising. We added the thermal tropopause to Figure 3 as well as the location of the 2 PVU surface. It is evident, that the thermal tropopause is at higher locations, than the 2 PVU surface. Importantly the thermal tropopause does not coincide with the non-linearity of the t_{TST} distribution, indicating that the tropopasuse definition is not the reason for the kink in the CO observations in Figure 11 (or e.g. Hoor et al 2004.).
The statement, that tracer profiles relative to the thermal tropopause are more compact than to the dynamical tropopause is not correct, since it has not yet been shown. This is subject of a separate study, which indicates, that the scatter of trace gas observations is reduced when using the dynamical tropopause definition as reference

ACPD
10, C6047–C6051, 2010

surface (note that this does not imply a good correlation to PV!). In most cases the thermal tropopause is within the transition layer (e.g. Pan et al., 2004)

A better correlation of trace gases relative to the location of the thermal tropopause compared to the dynamical one would also be surprising from a dynamical consideration. PV, which is conserved under adiabatic conditions combines relative vorticity and static stability, the latter also defining the thermal tropopause. Thus a change of static stability along a trajectory associated with a change of the location relative to the thermal tropopause can be balanced by relative vorticity. Such an air parcel would change it's location relative to the thermal tropopause, but still keep it's dynamical signature of the original reservoir (stratospheric or tropospheric PV). Only if diabatic processes act on either the static stability or vorticity or both the air parcel can be irreversibly transferred from e.g. the troposphere into the stratosphere and mix with the chemically different surrounding. Since diabatic processes change PV, the changing PV along air parcel trajectories allows to differentiate between irreversible (mixing) and reversible (conserved PV) processes, which is not possible when only considering the temperature profile.

To illustrate the non-linear behaviour of t_{TST} we added a plot of t_{TST} as a function of equivalent latitude. It clearly shows that the region of frequent and rapid exchange mainly affects a band following the tropopause. Further away from the tropopause, the fraction of TST-affected air decreases and t_{TST} increases. The non-linear transtion occurs around 25-30 K above the 2 PVU surface as previously deduced from CO data (Hoor et al., 2004).

A potential reason could be, that the diabatic processes, which lead to positive PV-changes, which are required for TST are related to processes, which are related to the location of the tropopause. Numerous processes potentially contribute such as wind shear at the jets, stirring processes driven by synoptic scale waves, small scale wave breaking, radiative processes from concentration contrasts at the tropopause, cloud effects. Although some of these are not explicitly resolved in ECMWF data and thus trajectories, they might be implicitly included through the assimilation of the

ACPD

10, C6047–C6051, 2010

underlying observations. Deeper in the stratosphere diabatic motions are goverend by other slower processes (radiation, stirring through the large scale flow). Note that this is also a potential explanation for the correlation of PV(t=0) and t_{TST} in Fig. 4. Although we have no final proof, this would be consistent with the observations.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12953, 2010.