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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.

Anonymous Referee #2

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This paper presents an original study of transport in the extratropical UTLS region, based on analysis of trajectories that experience troposphere-to-stratosphere transport (TST). The results are used to explore the distribution of transit times associated with TST, and also the statistical behavior of the TST Lagrangian cold points, and these results are then used to explain differences in ExTL structure between CO (a photochemical tracer) and water vapor. Overall this is an interesting analysis that makes an important new contribution to quantifying transport in the ExUTLS. The methodology is clearly explained and the paper is well written in general. I think this paper is appropriate for ACP, but there are a few important points that I recommend the authors address in revision.

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?

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.

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.

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