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Interactive Comment

Interactive comment on "Seasonality and extent of extratropical TST derived from in-situ CO measurements during SPURT" *by* P. Hoor et al.

P. Hoor et al.

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General comments

The main criticism of reviewer 1 is the definition of the tropopause using a PV-threshold of 2 PVU and the deduced distance to the local tropopause ($\Delta\Theta$ [K]). From these value we inferred a strong coupling between the extratropical upper troposphere and the lowermost stratosphere, which is according to reviewer 1 in contradiction to the paper of Logan(1999). Logan(1999) observed distinct seasonal cycles of ozone in the upper troposphere and lowermost stratosphere using the thermal tropopause definition according to the WMO. Reviewer 1 claims that if the tropopause definition is wrong, the kink in our measured CO- and CO_2-profiles relative to the local tropopause might indicate the real (thermal) tropopause rather than the extent of influence of tropospheric air in the lowermost stratosphere. Consequently the following conclusions of Hoor et al.(2004) concerning the extent of tropospheric influence as well as seasonal cycles

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would be misleading.

Specific comments

We thank the reviewer for that comment and started as suggested a detailed comparison of both the thermal and the dynamical definition. We analyzed our vertical profiles to compare thermal and dynamical tropopause. In our data the thermal tropopause appeared between $\Delta\Theta$ = 5 - 7.5 K relative to the PV = 2 PVU tropopause (see new Fig. 4). This finding is not surprising since e.g. Hoerling et al.(1991), Bethan et al.(1996) and Wirth(2001) also found on average the thermal tropopause above the dynamical tropopause. However, Wirth(2001) shows that the discrepancy partly arises from the fixed value of -2 K/km leading to a bias towards a more 'stratospheric' thermal tropopause. They obtained much better agreements between both definitions when taking the mean between the tropospheric and the stratospheric temperature lapse rate. A detailed discussion of the tropopause definitions is out of the scope of this paper, but the new Fig. 4 illustrates, that our chosen threshold is a good proxy for the tropopause.

Importantly, the observed kink in our CO-profiles which we found in the region between $\Delta\Theta$ = 20 - 30 K depending on season is far above the thermal tropopause and therefore marks the separation between two regions in the lowermost stratosphere. Relative to the thermal tropopause the depth of the mixing layer would decrease, but still our conclusion that the mixing layer is coupled to the local tropopause rather than isentropes would still be valid. The physical meaning of the kink is most likely the balance between isentropic transport and subsequent mixing from the extratropical tropopause and the seasonal changing strengths of the diabatic downward transport of the stratospheric background. The compact correlations between N_2O and CO in the new Fig. 3 illustrate this fact. Furthermore, the decrease of N_2O, which is a clear indication for stratospheric air, begins in the $\Delta\Theta$ =0-10 K-bin in agreement with new Fig. 4. Note, that the N_2O-CO-correlations in the new Fig. 3 also exhibit a kink above the tropopause, which is less pronounced due to the weak gradient of N_2O at the tropopause, but also

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indicates a subdivision above the tropopause. In summer the compactness of the correlation is destroyed due to stronger contribution from the extratropical troposphere. The latter in combination with a weaker diabatic downward transport from the stratosphere leads to a much more inhomogenous composition which is observed in the summer correlations compared to winter.

Concerning the claimed differences between the analysis of Logan(1999) and our analysis we don't see a disagreement of both. The data of Logan(1999) were interpolated on a vertical grid with a vertical resolution of 1 km. Their analysis of the seasonal cycles at the thermal tropopause and 2 km above lead to distinct seasonal cycles. That corroborates our analysis of the distinct CO_2-cycle at $\Delta\Theta$ = 0 K and above $\Delta\Theta > 20$ K, which we included in the paper. The analysis of Logan(1999) contains no statement about the region between the tropopause and 2 km above which we regard as the mixing layer.

Finally the reviewer claimed that our analysis is not quantitative enough and asked for scattered plots. We think that the use of correlations illustrates irreversible mixing but it is difficult to infer really quantitative information from these plots in terms of amounts of air. The N_2O-CO scatter plot in new Fig. 3 plot illustrates the irreversibility of mixing. Using N_2O as tracer adds some new information since it is very long-lived and in particular independent from local photochemistry.

We added the citations of Pan(2000), Logan(1999), and Hoerling(1991) and added a sentence to the abstract according to reviewers point 3. The paragraph dealing with the trajectories and the derivation of the upper tropospheric CO was rewritten to clarify the description (points 4 and 5).

The cited references in this comment have been added to the revised manuscript.

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