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Interactive Comment

Interactive comment on "Mixing processes and exchanges in the tropical and the subtropical UT/LS" *by* R. James and B. Legras

Anonymous Referee #1

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The manuscript addresses an important topic about mixing and exchange in the UTLS. The authors combine airborne in-situ tracer measurements with Lagrangian modelling to assess cross tropopause mixing in the subtropics and tropics. They show that very different time scales and particle origins contribute to the composition of air in the TTL the tropical lower stratosphere and the subtropics and confirm previous studies that short-term mixing mainly occurs at the subtropical tropopause region and is mainly restricted to a shallow layer at the local tropopause. Above and in the TTL, mixing time scales are deduced which are on the order of one month. Importantly the authors show that recirculation of stratospheric air into the TTL plays a role and that the tropics strongly affect the trace gas composition of the lowermost stratosphere at higher latitudes.

The highly interesting paper adds new pieces of knowlegde to the scientific community and clearly merits publication in ACP. Though a few changes and particularly clarifications should be included in the final publication, which are listed below:

One general remark:
The authors should take more care when defining and later referring to different regions, which they investigate. They use several times expressions like '...in the dynamical tropopause... ' (e.g. heading of sec.4). I guess a layer in the subtropical tropopause region is meant (defined in Fig. 2c)? It complicates the reading and understanding through the entire manuscript (e.g in section 5: mixing above this aforementioned layer/surface above 350K including TTL or in the subtropical stratosphere above 350 K etc...). It would be very helpful, if the authors could add the respective flight sections on the PDF's in Fig. 4/7/10. Several of the minor comments also refer to this.

Specific:
p.10644, l.23: I do not understand the occurrence of negative values of vertical velocity. They might occur in particular when waves play a role. However, given the large number of trajectories, which contribute to the results on display and the large number of flights I'm surprised to see these negative values in particular during winter, when upward velocities are generally large. How is the velocity calculated and how do heating rates correspond to them? The argument of "special sampling of the measurements" is a dangerous argument since it would also mean, that the whole conclusions of the authors in the presented study are also biased by the selection of flights (which I don't believe). The authors should at least specify, what was special about the sampling. Can the negative vertical velocity and the associated conclusion on the importance of diffusion really be concluded also an artefact of the method? Which role plays numerical noise and the representation of vertical transport in the underlying meteorological field?

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Minor points:

p.10633, l.6-9: The observation of the CO-O3 relationship with mixing lines can only be understood as a result of cross-tropopause transport. Therefore I would say that the idealized tracer experiments in ClaMS ' ... have confirmed that these ditributions are the result of cross tropopause mixing' and that Lagrangian methods can be used to detect them.

p.10633, l.10-21: What is a 'true' mixing line? Do you mean irreversible? Also the second case is not clear to me. Non-unique mixing lines occur quit frequently as the result of different chemical compositions and air masses originating in different regions. Sloped CO-O3 correlations in tracer-tracer space are always the result of mixing. You should clarify this.

p.10633, l.25. I'm not sure if I understood correctly, but the processes you mention bring boundary layer air rapidly to the UTLS, but mixing can be performed by a lot more (in principal all diabatic) processes. The chemical difference in tracer - tracer space mainly depends on chemical processes and their relation to dynamical processes.

p.10635/p.10636: It would be helpfull to redraw the Figure.1/2 of Legras, 2005 to help understanding the method. The initial air parcel is splitted into sub parcels, but does this happen only for the first time step? Furthermore diffusion is only applied to the vertical (cross-isentropic), but how would a horizontal, quasi isentropic (but not necessarily iso-PV) diffusion act on the reconstruction (e.g. Hegglin et al. 2005)? Their results support your assumption that the cross isentropic component is of importance for the trace gas distribution.

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p.10639, l.4: "the dynamical tropopause relationship": You mean the subtropical CO-O3 mixing line in Fig.2c?
l10-14.: Why not the other way around? The stratospheric and the tropospheric parcels in Fig.4 are connected on an isentrope, whereas the stratospheric distribution shows a high probability across isentropes from 320-360K 9 days before the flight. Thus they could have been brought down diabatically 9 days before the flight in the vicinity of the jet and mixed quasi horizontally afterwards due to horizontal shear?

p.10641, l9-12. and l.14-16: 'In the continuity of...'—>'In agreement with Hoor 2005...': Reference should be given to Berthet et al., 2007, who came to similar conclusions also based on a Lagrangian approach.

p.10642, l.20: The ability to reconstruct the time series is impressive, but how where the stratospheric end members for the CO(s) and O3(s) determined? At least it should be consistent with the tropical correlation in Fig. 2c) , from which I would deduce (35/400) or (50/200) for (CO/O3). The vertical gradients of both tracers are a problem when defining stratospheric values, since there is no uniform stratospheric background of ozone at these low altitudes. Does the reconstructtion work with H2O?

Technical:
abstract: l.9: replace '.. the mixing line...' by '.. a mixing line...' since the measured mixing line is not necessarily a common case.

p.10629, l.3/4: Please clarify: Pan et al. 2004 show , that the transition between troposphere and stratosphere is more a layer rather than a jump.

p.10629, l.10-12: "...transport barriers", but permeable if diabatic processes occur and

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with strengths depending on season (e.g Haynes and Shuckburgh).

p.10629, l.28: Do you mean ’below’ or ’south of’?

p.10630, l.24 ’ppbv’ instead of ’ppmv’
l.28 ’distribution’ instead of ’profile’ ?

p.10634, l.9 and l.12.: Maybe you could replace the words ’junction’ and ’meeting point’ by ’branching’ and ’merging’?

p.10635, l.5: ...according to the tropospheric influence’. You mean it’s time scale and origin?

p10635, l.18. "... respect to the dynamical tropopause using different PV-values"?

p.10636, l.28: "Integration time is 9 days in the dynamical tropopause". What does this mean ("in the tropopause", also title of section 4)? Integration time is 9 days for air parcels initialized between 2–4 PVU? Please rewrite this.

p.10637, l.7. ’its distribution among ...’ do you mean the relative fractions between tropospheric and stratospheric?

p.10638 ,l.3: Whats a "flat" tendency? Do you mean linear?

p.10639, l.12: isentrop’E’

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P.10640: Header of section 5: I guess it is meant above the subtropical dynamical tropopause layer or in general above theta=350K?

p.10641, l.l4 reference Desler and Scherwood –> Dessler and Sherwood

p.10644, l.11-15: I don't understand the last sentence. Where does the seasonality comes from since you only discuss winter measurements?

Additional references:

Hegglin M. I., D. Brunner, T. Peter, J. Staehelin, V. Wirth, P. Hoor, H. Fischer (2005), Determination of eddy diffusivity in the lowermost stratosphere, Geophys. Res. Lett., 32, L13812, doi:10.1029/2005GL022495.

Berthet G., J. G. Esler, P. H. Haynes (2007), A Lagrangian perspective of the tropopause and the ventilation of the lowermost stratosphere, J. Geophys. Res., 112, D18102, doi:10.1029/2006JD008295.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10627, 2008.

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