

***Interactive comment on* “Contribution of mixing to the upward transport across the TTL” by P. Konopka et al.**

Anonymous Referee #2

Received and published: 15 January 2007

The manuscript presents the recently performed extension of the CLAMS model to the troposphere using a new hybrid pressure - potential temperature coordinate and applies it to the analysis of two flights from the TROCCINOX campaign in Brazil and to a study of mixing across the TTL. As in previous studies with the stratospheric version of CLAMS, the new model performs very well in the comparison with aircraft ozone. The results of Section 6 are however very surprising and, honestly, not convincing at the present stage. A detailed analysis is required to make sure that they are not due to the results of inaccuracies in the data or in the modelling. There are also a number of minor points that require improvements as detailed below.

Specific comments

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

1. Figure 7 is referred incorrectly many times in section 2. Splitting figures into several panels with same caption make them hardly readable. A nicer appearance might be restored if the paper gets to ACP but the authors should make the effort to make a better presentation for ACPD as well.
2. The arrow in Fig. 3a points to a system located at 30S, that is not usually considered to lie within the ITCZ.
3. In section 2.3, the notion of large-scale convection in analysed winds needs to be better defined and analysed. In the extra-tropics a significant part of the ascent can be performed by quasi-adiabatic motion along the warm conveyor belts and convection is organized mostly along fronts. In the tropics, the ascent is localized in deep convective clouds but analysed winds spread this motion (actually the residual of parametrized updrafts minus downdrafts) over grid points. Hence they provide ascending motion everywhere in the ITCZ although the air is in fact descending outside convective clouds. It is unclear how the analysed winds can represent correctly the vertical transport of pollutants in the tropical region. Water vapour receives, however, a special treatment, as mentioned in the manuscript. The fact that $\dot{\zeta}$ correlates with high water vapour at about 250 hPa is consistent with this picture and does not prove that $\dot{\zeta}$ accounts quantitatively for the tropical vertical transport of tracers.
4. The choice of $\eta = p/p_0$ rather than a *sigma* coordinate needs to be explained and is likely to generate a number of problems in the boundary layer.
5. The choice of the vertical discretization based on entropy density has the undesirable property to modulate the vertical mixing (per event) as shown in Fig. 2
6. The authors do not comment an important fact in Fig.4 which is the presence of a cooling layer near $\zeta = 350\text{K}$ at any time in the tropics. This should be put in

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- relation with the inability of the tracers to cross this level without mixing discussed in Section 6.
7. The case study shown in figure 3 and 5 is very complex and not easily analysed. The validation of ζ should rely on a larger dataset with some quantitative statistics.
 8. Jet split does not necessarily mean instability. It can occur typically as a result of meridional PV transfer due to mature decaying baroclinic disturbance (sometimes referred to as Rossby wave breaking).
 9. At the end of section 4, it is unclear how the linear growth of ECMWF error with altitude is estimated. Is it only from the peak at 18:30 in Fig. 7a or is it checked in other flights as well? The performance of the ECMWF ozone is actually surprisingly good and this should be mentioned.
 10. In Fig.7a again, the authors insist on the reproductions of the small peaks due to filaments. Do they also have an interpretation for the fairly poor performance of the model between 17:30 and 18:00?
 11. As mixing in CLAMS is not just smoothing the curve obtained without mixing, can we consider that the non mixing case is just one realization (according to the initial gridding) while mixing represents a statistical average over possible realizations by the large number of mixing events?
 12. The color bar is missing in Fig. 7b.
 13. In Fig.8, the boundary between tropospheric and stratospheric branch is fairly arbitrarily defined and does not separate apparently the mixing ratio of any other specie than ozone.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

14. Section 6 draws the most interesting but also the most controversial results of this study. While it is easy to accept that mixing plays a role in the TTL, it is hard to believe that after 3 months, no parcel starting in the boundary layer would reach the tropopause without mixing since diabatic motion is ascending above 15 km and there is enough convection reaching that level in the model to feed it with some parcel from the boundary layer within the course of three months. One of the main reason of the discrepancy might be the cooling layer around $\zeta = 350K$ in the tropics which is likely to cause the trapping seen in Fig. 13. This cooling does not match tropical radiative calculations published in the literature and there are some reasons to believe that transport occurs in the tropics where the vertical wind shear is not large. For instance, the low value of water vapour in the tropical lower stratosphere could not be explained if the ascent occurs above the jet streams as found in CLAMS but requires to cross the cold pool in the tropics as trajectories based on analysed velocities do (see, e.g., Fueglistaler et al., JGR, 2005).

Since all contributions to the heating rate are small near the tropopause, and that the total is of the order of 0.5 K per day (Gettelman et al., JGR, 2004) or less, numerical and/or modelling inaccuracies may easily yield incorrect results. The radiative calculations are using the Morcrette scheme and could be compared with ECMWF calculations during ERA-40 or more recent reanalysis experiment for which heating rates are archived. It should also be taken into account that heating budget from ECMWF analysis or reanalysis contains a significant contribution from the assimilation increment that compensates the bias of the model and the excessive vertical transport by the noisy vertical velocity (itself excited by the assimilation). This contribution which can actually be dominant in some regions varies a lot from one version of the model and the assimilation system to the other.

Figure 14 shows that the mass imbalance due to the choice of ζ coordinate is

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

most pronounced where the heating is also negative. This is a worrying feature regarding the global transport properties across this layer.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12217, 2006.

ACPD

6, S6229–S6233, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S6233

EGU