

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

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Response to reviewers 1 and 4

We would like to thank both reviewers for a very thoughtful and detailed review of our manuscript. In particular their remarks with respect to the theoretical background of the model are very important and helpful for us to improve the paper. In the following we address, the major points raised in the reviews.

1. *“hybrid coordinate in the troposphere: pressure versus terrain following coordinate”*

The use of the pressure-like coordinate below the tropopause ($\eta = p/p_0$ instead of $\eta = p/p_{surf}$ (i.e. terrain following, the so-called η -coordinates) is motivated solely by the simpleness of this coordinate. We use the ECMWF ana-

lyzed fields on pressure levels (even if these fields were originally determined in η -coordinates) and calculate with the radiation module (that works only on pressure and not η -coordinates) the radiative heating/cooling rates \dot{T} and $\dot{\theta}$. The values of $\dot{\theta}$ are used in eq. (4) for the calculation of $\dot{\zeta}$. In the Eulerian models, the use of the η -coordinate allows to define a compact, terrain following surface as the lower boundary of the model on which boundary conditions can be formulated.

In a Lagrangian approach and in particular within the framework of the CLaMS hybrid ζ -coordinate, we define the boundary conditions within a layer $\Delta\zeta$ following the orography (defined by the highest levels with missing values in ECMWF pressure). Here, the mixing ratios of the air parcels are redefined according to prescribed boundary conditions (every 24 hours). Other tropospheric CLaMS layers can intersect this terrain following layer. If the ECMWF velocities are correct, the trajectories of the air parcels should overcome all possible orographic obstacles. But it is not a problem for the CLaMS model if an air parcel would “crash” with an orographic obstacle. In such a case this air parcel will be removed from the model domain. The mixing algorithm creates a new one by filling the resulting hole. The mixing ratio of this air parcel is interpolated from the next available neighbors.

Thus, in contrast to the Eulerian approach, it is not necessary to have a single, terrain-following surface as the lowest boundary of the model. Nevertheless, some studies comparing the positions of trajectories calculated in η -coordinates (FLEXPART) with the corresponding positions in p -coordinates (LAGRANTO) show some advantages of using the terrain following coordinates (mainly due to an improved interpolation technique in η -coordinates Stohl2001).

In summary, while our method might possible cause problems in a simulation of mixing ratios in the boundary layer, it will have no impact on the CLaMS simulations of the TTL

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2. “hybrid coordinate in the troposphere: pressure versus isentropic coordinate”

The question of the extension of the isentropic coordinates down to the planetary boundary layer is still relevant and surely a tempting alternative. The main problem along this way is that the temperature tendencies, \dot{T} , due to convection and latent heat release (due to radiation can be derived from a radiation module) are only available for ERA-40 but not for ECMWF operational analysis that we prefer to use for the comparison with in-situ observation. And, of course, ERA-40 is not available for TROCCINOX. It is generally accepted that the assimilated ECMWF meteorological fields contain “the most complete” and global information on convection (i.e. convection understood as a convection-induced bulk or mean vertical velocity within a ECMWF grid box). So, it is desirable to take this information into account, in particular in the tropics, up to 100 hPa, where many people believe that deep tropical convection is crucial.

Thus, our main motivation for the hybrid ζ –coordinate as defined by eq. (1) was to couple the tropospheric ECMWF-based vertical velocities (best global, bulk information on convection) with the radiation-driven vertical velocities in the stratosphere (best stratospheric approach as shown in many stratospheric transport studies with SLIMCAT, Match-trajectories and CLaMS). It should be emphasized, that this is only the beginning of our UT/LS CLaMS studies and that we are open to extend our approach (e.g. by extending downward the θ –coordinates). We plan to put some sentences explaining our motivation into the discussion at the end of the manuscript.

3. “entropy preserving air parcels in CLaMS”

What we really mean, or even postulate, is that the volume of each air parcel contains the same amount of entropy S (i.e. entropy understood as an extensive quantity, so we have to multiply the entropy density $s = c_p \ln(\theta/\theta_0)$ with the air density n). We admit, that we cannot prove this assumption. We have only some supporting arguments as the Shannon concept of the the “same amount of

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information" per volume unit.

The most important argument for our assumption is that this definition of air parcels and consequently CLaMS layers implicates an increase of the vertical diffusivity with the altitude (understood as numerical diffusion per mixing event, see below) as shown in the right panel of Fig. 2 (red line) and that such a choice has some justification from observations and 1-D age studies Ehhalt2004 (for the definition of the vertical diffusivity see below).

Another argument is based on the maximum of entropy S slightly above the tropopause (black line in the right panel of Fig.2, see also K. Emanuel in Physics Today, August 2006, p.74 and references therein). This implicates that if questions of mixing, i.e. of entropy production (one expects that entropy production $\sim S$) are discussed, one needs a higher resolution of those parts of the atmosphere where highest values of S are expected (i.e. near the tropopause). The entropy-based measure of mixing at the tropopause was recently discussed by [Patmore and Toumi(2006)] showing that half of the entropy produced by mixing can be attributed to subtropical "Rossby-driven" tropopause folding events and that the remaining part can be associated with tropical convective mixing and shear-induced mixing at subtropical jet streaks. Thus, these results also support the idea that if mixing is considered within a discrete model, the entropy should be equally resolved over the whole model domain.

Furthermore, because our first long-term simulations (5 years) also satisfactorily represents the distributions of CH₄, CO₂, age of air and the upward velocity of the tape recorder, we have some additional reasons to trust this concept. Here, we will rewrite the section 2.2 as proposed by the reviewer 1 and discuss in more detail these entropy-related arguments.

4. *"is the aspect ratio α dimensionless ?"*

Here, we completely agree with the reviewers. It was an error in our manuscript. Using a given value of the aspect ratio α and the mean horizontal distance be-

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tween the air parcels r_0 (in km), we first calculate $\Delta z = \alpha r_0$ and then transform Δz to $\Delta \zeta$ using the US standard atmosphere and the definition (1). This explanation will be included in the new version of section 2.2. Also we will add some additional information explaining the definition of the air parcels in CLaMS as pivotal points representing a volume of the atmosphere with a given amount of entropy S and defining the (mean) mixing ratios of all relevant species averaged over such a volume.

5. *“ECMWF H₂O enhancement due to convection or latent heat release, Fig. 3 and Fig. 5”*

Here we agree that not all enhanced values of ECMWF-H₂O which are shown in Fig. 3c are due to strong convection within MCS. We will discuss this point more carefully in the revised version. We also agree that the example denoted by the black arrow is probably due to a warm conveyor belt rather than due to a MCS. Nevertheless, a significantly increased density of high clouds can be seen the GOES satellite data in the region denoted by the black arrow (not shown in the paper). We plan to look more into the details of this example.

However, over south Brazil and north Argentina, strong convection was reported by the Brazilian met. office for the same day (MCS that also manifests in enhanced ECMWF H₂O even if this structure is not so pronounced as the signature denoted by the black arrow). We plan to discuss this point more carefully, in particular motivated by the additional recommendation of the reviewer 3 to demonstrate more explicitly how shear-induced mixing in the outflow region of large-scale convection contributes to an uplift of the air masses. Here we plan to add an additional figure to the manuscript supporting this point.

6. *“artificial vertical transport within the Ferrel cells”*

Here, we also completely agree with the reviewer and will improve this part of the text. We are also aware that the use of the potential temperature as the vertical coordinate and as proposed by the reviewer would widely remove this “apparent

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transport problem" (with exception of the vertical transport induced by the latent heat release).

7. *"justification of the estimate for vertical mixing intensity: ($D_v = \Delta z^2/4\Delta t$)"*

Here, we agree that the discussion of this point is too short and too strongly based on published results. The estimate $D_v = \Delta z^2/4\Delta t$ for the vertical diffusivity can be justified in the following way:

Let us consider 2 adjacent air parcels separated by the horizontal and vertical distances r_0 and $\Delta z/2$, respectively. r_0 and $\Delta z/2$ are the mean horizontal and vertical separations between 2 adjacent air parcels within a layer with a thickness Δz . Now, we follow the CLaMS mixing procedure that is applied with the frequency $1/\Delta t$ (here $\Delta t = 24$ hours denotes the length of the pure advection in terms of the trajectories and has nothing to do with the trajectory integration time step that is of the order 10 minutes). This mixing procedure includes new air parcels into the irregular grid. In particular, such new air parcels are included in those parts of the grid where distances between the next neighbors (calculated before the advection step) have exceeded above or have fallen below a critical value (for details see [[Konopka et al.\(2003b\)](#), [Konopka et al.\(2004\)](#)]). The numerical horizontal and vertical diffusion due to interpolation of the vmr on these new air parcels can be estimated as $D_h = r_0^2/\Delta t$ and $D_v = \Delta z^2/4\Delta t$, respectively (numerical error of the time and special interpolations due to discretization that can be reinterpreted as the diffusion coefficient of a discretized diffusion equation). This is, how we connect in CLaMS the regridding procedure with diffusivity (in m^2/s)

We will include this more detailed explanation into the revised version.

8. *"Rossby wave breaking instead of unstable jet bifurcation"*

Here, we agree with the reviewer and will include the corresponding sentence into the revised version.

9. “*how strong is the statement that vertical mixing is a major contributor to vertical transport*”

Here, we agree with the reviewer that this discussion needs to be more balanced. Our point is that the precise transport mechanisms in the TTL are not well understood and that what we are proposing here is a *hypothesis* about this mechanisms. We certainly do not claim to present the ultimate answer here. With our simulations and comparison with the experimental data, we only would like to show that mixing, in particular the mixing scheme implemented in CLaMS has the potential to close the gap between the main convective outflow and radiation-driven (or extratropical wave drag-driven) transport in the stratosphere.

The sensitivity studies of our results with respect to the choice of p_r (critical pressure level, below p_r we replace the radiation driven transport in the stratosphere by the ECMWF vertical velocities) show that by shifting p_r to higher values we suppress the effect of convection and increase in this way the gap between the main convective outflow and the stratosphere. On the other side, by shifting p_r to lower values, the vertical transport across the TTL can occur without mixing (i.e. in terms of pure trajectory calculation) but, as correctly mentioned by the reviewer is driven mainly by the numerical noise of the ECMWF data. We think that it is at least well-motivated to try to replace this numerical noise by a more physical concepts like radiation and mixing parameterization as implemented in CLaMS. With respect to this point, we also refer to reviewer 2 (remark 14) and our answer on this point.

10. “*what are the physical reasons behind the chosen parameterization for mixing ?*”

The real physical mixing occurs on much smaller scales than the scales resolved by CLaMS (and most other global models if not all) and is (probably) caused by such events like breaking gravity or Kelvin waves or some other sources of instabilities which may occur in the atmosphere. In our approach, we follow the idea that such unresolved processes are driven by deformations in the large-scale

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flow (strain - horizontal or shear vertical). This idea that was first postulated by [[Smagorinsky\(1963\)](#)] couples mixing (that occurs on spatially unresolved scales) with gradients of the large-scale flow. The CLaMS parameterization of mixing is nothing else as the Lagrangian realization of this approach. We discuss this point in the last part of section 6 of our paper.

11. *“past works referencing the RDF-based reconstructions of small-scale tracer structures”*

We agree that this point is only poorly addressed in our paper. We will include this point in revised version in particular the results shown in [[Legras et al.\(2005\)](#)] where the variability of the turbulent diffusivity is discussed. In particular, they show, in agreement with our studies, that turbulent diffusivity within the polar vortex is much weaker than in the surf zone.

Nevertheless, the idea of CLaMS is to overcome the limitations of the RDF-based studies (with or without mixing) and to run a Lagrangian CTM where small-scale structure are continuously created by the flow and smeared out by the mixing. Such simulations have no time limitations whereas RDF-based studies (with or without mixing) can only give some limited insights into mixing processes. Irreversibility is a physical process that can be satisfactorily studied only by forward transport studies. Generally, irreversibility allows to differ between forward and backward-directed processes, or, more placative between the future and the past.

12. *“past works referencing the impact of breaking Rossby waves on mixing”*

Here, we also agree that this point is not satisfactorily addressed. In particular the contributions [[Bradshaw et al.\(2002a\)](#)] and [[Bradshaw et al.\(2002b\)](#)] describing observations of ozone layers in the vicinity of the subtropical jet which were generated by (breaking) Rossby waves are important in context with our work.

13. *“comparison with experimental data, statistical approach to quantify correlations, Kolmogorov-Smirnov test..”*

The biggest advantage of the comparison between the model and insitu observations along the flight track is to show how far or how close are model simulations from the real atmosphere (in situ observations have the potential to resolve the finest atmospheric structures) even if, usually, the so-called "best case" is shown. Using some kind of statistics is desirable and we did this kind of work in the past (using PDF analysis Konopka2003b, Konopka2005b or tracer/tracer correlations Konopka2004). We plan to do this in the future also for the observations within the TTL although such an analysis is beyond the scope of the presented paper. In this paper, the model formulation and its first approach was the main goal of the paper. Nevertheless, we plan to include into the paper some "easy" correlation coefficients describing the overall quality (i.e. for all Geophysica flight) of our methane and ozone distributions.

14. *"general comments on the abbreviations and figures"*

Here, we agree and will replace the abbreviation "AP" by the full name "air parcel". Also as suggested by the reviewer 3, we will reduce the number of the abbreviations in the revised versions. The figures 3, 5 and 7 were split (each panel of these figures as a separate figure) in order to avoid too small figures in the print version of the ACPD version of this manuscript. The consequence was that some of the links to these figures in the text were not correctly set. In the revised version of the manuscript we will correct this problem, remove the spelling errors and have the text revised regarding grammar and style by a native speaker.

15. *"technical comments"*

We agree with all technical comments of both reviewers.

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