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Interactive Comment

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

P. Konopka et al.

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Response to reviewer 3

We would like to thank the reviewer for a very thoughtful and detailed review of our manuscript. In the following we address, the major points raised in the review.

1. *"CLaMS mixing driven by flow deformations in particular by the vertical shear"*

   The real physical mixing occurs on much smaller scales than the scales resolved by CLaMS 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 flow (strain - horizontal or shear vertical). This idea that was first postulated by Smagorinsky1963 couples mixing (that occurs on spatially unresolved scales) with gradients of the large-scale flow. The CLaMS parameterization of mixing is nothing else than the Lagrangian realization of this approach. We discuss this point in the last part of section 6 of our paper.

We agree with the reviewer that the connection between the shear in the flow, in particular in the outflow region of convection, and the mixing intensity in the model should be shown more explicitely. We plan to discuss this point more carefully and will add an additional figure to the manuscript discussing the impact of convection on vertical mixing and on the observed tracer distributions on February, 8, 2005.

2. *“correct reference of Gettelman et al. 2004"*
   will be included

3. *“intersection of topography with gray line in Fig. 2, definition of boundary condition"*
   The intersections of the gray line with topography should not have any implications on the motion of air parcels. If the ECMWF velocities are correct, the air parcels should overcome all possible orographic obstacles. But it is not a problem for the model if the air parcel would crash with an orographic obstacle. In such a case this air parcel will be removed from model domain. The mixing algorithm creates a new air parcel by filling the resulting hole. The mixing ratio of this air parcel is interpolated from the next available neighbors. Within the boundary layer that follows the orography, all air parcels are replaced (every 24 hours) by the grid of air parcels that was defined at the begin of the simulation. Their mixing ratios are redefined according to a prescribed boundary condition (e.g. for CO according to MOPITT data or for CO2 according to the the NOAA-CMDL zonally averaged observations. CO and CO2 are now available in the current version of the model but not considered in the manuscript).

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Thus, in the version of the model published by ACPD, the prescribed mixing ratios of O3, CH4 and of the boundary layer tracer BL are used (and not the fluxes). Of course, the prescribed mixing ratios implicate a flux of species into or from the model domain.

4. *“mass conservation"*
The used transport scheme is not under control of the mass conservation as, e.g. the well-known Prather advection scheme. Furthermore, the mass balanced ECMWF velocity fields in the troposphere are coupled with winds in the stratosphere where the vertical velocity is derived from a radiation scheme (ζ hybrid coordinate) - the latter ones are not mass conserving. We do not use any mass fixer. The isentropic studies as discussed in the past McKenna2002a show that the CLaMS transport is approximately mass conserving for such species as CH4 or N2O on a time scale of several weeks. The check of mass conservation in the current version of the model is difficult due to the use of the velocity fields which are not mass conserving. We argue that if we can successfully reproduce the tracer distributions (such as those of CO2) at a time scale of several years (current time scales of CLaMS simulations), the transport and consequently the mass conservation are sufficiently validated.

5. *“z describes the geometric altitude"*
Yes, this will be additionally explained.

6. *“the first arrow in figure 7 is not clearly elevated ozone"*
Yes, we agree and we will change our text.

7. *“objective criteria for the definition of the AB and BC parts of the flight in Fig. 7"*
It is difficult to find a clear boundary between these air masses. We will try to find it as recommended in the observed correlations.

8. *clear mixing lines in the observed Ž2O/Ø3 correlations*

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We agree that the formulation “clear mixing line" is not correct. This is more like a mixing region than mixing line. We will reformulate these sentences.

9. *“lightning NO production in the stratosphere"*
   No, we deduce only that signatures of lightning NO were probably produced in the upper troposphere and than mixed into the lower stratosphere. We will reformulate this part of the text to make this clear.

10. *“How does shear mix"*
    As described in the first point, we will focus on this point by adding a new figure and a new text describing how shear, at least in the model, triggers vertical mixing.

11. *“some implication on seasonality"*
    We agree that our simulation are not long enough for full discussion of the seasonality of transport. On the other side, we believe that some clear differences of the permeability of the subtropical jet on the winter and on the summer hemisphere can be found in our simulations (which are also confirmed by our current long-term simulation). Effective diffusivity analysis of [*Haynes and Shuckburgh*(2000)] shows that such barrier properties form on a time scale of several weeks that is well-covered by our 108 days simulations.

12. *“general comments on the abbreviations and figures"*
    Here, we agree and will replace the abbreviation “AP" by the full name “air parcel". Also as intended 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.

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13. *“technical comments"*
    We agree with all technical comments.

References

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[*McKenna et al.*(2002)] McKenna, D. S., P. Konopka, J.-U. Grooß, G. Günther, R. Müller, R. Spang, D. Offermann, and Y. Orsolini (2002), A new Chemical Lagrangian Model of the Stratosphere (CLaMS): Part I Formulation of advection and mixing, *J. Geophys. Res.*, *107*(D16), 4309, doi:10.1029/2000JD000114.

[*Smagorinsky*(1963)] Smagorinsky, J. (1963), General circulation experiments with the primitive equations: I. the basic experiment, *Mon. Wea. Rev.*, *91*, 99–164.

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