

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

Anonymous Referee #4

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General Comments

The paper addresses the issues of the mechanisms responsible for the uplift of air masses from the tropical troposphere into the stratosphere. The authors run a lagrangian chemical transport model with and without a parameterization of mixing, and compare results for passive tracer species to data from airborne instruments taken during the TROCCINOX tropical campaign. Model upgrading from a previous version is described and results from the simulation are discussed also from a climatological point of view. From the analyses of the results, the authors conclude that additional mixing is essential for the uplift of air masses from the main convective outflow to the stratosphere.

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While the paper is in general well structured, much more work, in term of analyses and discussions, is needed before the conclusions can be convincing. Several points in the model description need clarifications. In particular, there seem to be many ‘ad hoc’ choices for tuning the model runs that need justification and extended discussion. Comparisons with aircraft data is only qualitative and can not represent a real validation of the model at this stage. Also, the paper lacks a discussion about the physical meaning of the chosen parameterization.

Substantial revisions are needed before the paper could be considered for publication in ACP.

Specific Comments

- Section 2.1 Reasons and real benefits in changing from isentropic to hybrid pressure coordinates in the troposphere need extended discussion. In particular, it is not clear why one should not use isentropic assumption everywhere and instead apply a transition between radiation to ECMWF-related vertical velocities. A well presented and discussed comparisons between the two approaches in the troposphere are needed.

- Section 2.2: This section needs a major revision. It is not clear what an “air parcel” really is and how it is initialized and followed during the integration in the model. Even more confusing is the discussion about “distance” between different “parcels”, especially because in the chosen formulation the so called “aspect ratio”, that should control 3D resolution and diffusivity in the model, has dimensions.

The choice of DS as “total amount of entropy“, to be kept constant in each model layer, is puzzling. As defined by the authors, S is entropy per unit of volume. Should it be integrated over the volume of the layers to get a “total amount” of entropy? Or perhaps the authors are simply looking for constant entropy differences between two isentropic layers, or even for a simple division in layers of constant volume?

- Section 2.4 What are the physical reasons behind the chosen parameterization for

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the mixing ? What kind of mixing the authors intend to simulate in the model? Is it just a lagrangian irreversible mixing (as it seems from the fact that in the model it is triggered by shears and gradients and diagnosed by means of lyapunov exponents)? Are there any processes in the sub grid scale or other kind of (molecular) diffusivity implied? There are several works in literature addressing the issue of transport by Rossby wave breaking and high resolution tracer structures as diagnosed by means of lagrangian approaches. The real existence of such structures found in lagrangian models and how their irreversible mixing into the main flux can be simulated, have been subject of debate. The authors should make references to past works on this subject outlining what are the differences in their approach.

- Section 3. The use of the aircraft data to validate the model is one of the major problem of the paper. Simple visual comparisons along flight paths between tracers from airborne instruments and model interpolated results is too much qualitative and inconclusive. Perhaps the authors should consider some statistical approach to better quantify correlations, using for example correlation coefficients or statistical analysis similar to the Kolmogorov-Smirnov test used in Dragani et al. (J. Atmos. Sci., 59, 1943-1958, 2002) that takes into account the overall properties of tracer distribution along the flight track. The fact that a filament or any high resolution feature does not show up in such a simple tracer to tracer comparison does not mean in principle that it is missing, but it could only indicate a simply horizontally or vertically displacement.

- Section 6/7: Those sections need to be upgraded with a more complete discussion about the physical meaning of the “mixing” considered by the authors, and of all the other sources of errors or potential problems in the model. What is the implication of the change in coordinates? What are the variations linked to the noise in the ECMWF vertical winds used or also to the temporal/spatial resolution of the whole meteorological data set used to integrate the trajectories? Lagrangian models without mixing parameterization, even when run pure isentropically, produce filaments that have been compared in the past with in situ data. How does the new formulation of the CLAMS

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model improve this kind of comparison? Can this improvement be quantified? If looking for sub-scale mixing and structures, why do not consider instead of large scale flow, a set of high resolution winds from mesoscale models, as already done in the past for the analysis of the Geophysica data? Again, the authors should make references to past works that compare high resolution tracer data simulated with lagrangian model with aircraft (or other in situ) data, discussing what is new in their approach and on what basis it can be considered as an improvement.

- The paper contains many abbreviations. Some of them seem unnecessary or confusing for the reader. While some of them are commonly used, other are uncommon. For example ST for Stratospheric Tracer (abbreviated also as “(Strat. Air)” in figure caption) and BL for “boundary layer tracers” (abbreviated also as “(B. Layer)” in figure caption). In particular AP for air parcel is strongly unusual and should be removed, also because it is not clear what an “air parcel” is in the intention of the authors (a simple air mass (with volume)? A material point advected by the model?). Some of the abbreviations are also defined in the paper only after their first use, (i.e. STT is used in page 12221 line 15 and later defined in page 12231 line 11).

- The presentation of the figures would clearly benefit from a general rearrangement. First of all there are many erroneous references to figure numbers in the text and in the captions, and error in the indication of the colors. Also, figures are often referred as a part of a single “panel”, while they are instead plotted as different pictures (a,b,c) with different captions, with each single caption repeating the description of the other parts. The author should rearrange together the figures belonging to a single panel to improve readability. This is particularly necessary to avoid confusion especially in fig 7b that has the color bar plotted in fig 7a in the previous page.

Technical Comments

Page 12222 - line 13: Plotting colors of pressure and theta are wrong (also in fig. 2 caption)

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Page 12223 - line 27 and page 12230 line 1: Grooss et al., 2005 seems to refer to two different papers. Should be 2005a,b

Page 12226 - line 6: Example instead of exemple?

Page 12226 - line 7: should be Fig 3 instead of Fig 7

Page 12228 - line 17 and page 12229 line 6: should be Fig 5 instead of Fig 7

Page 12234 - line 11: It is not clear what the authors mean with “pure trajectory calculation”. Simple trajectories from the flight path? Interpolation of 3D CLAMS results without mixing ?

Page 12239 - line 4: How do the authors define the number of parcels to be “insufficient” ?

Page 12242 - line 10 Summarize instead of summerize?

Fig 5a,b,c captions: should be Fig 3 instead of Fig 7

Fig 6 caption: What does STE stand for ?

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