Atmos. Chem. Phys. Discuss., 10, C6374–C6381, 2010
www.atmos-chem-phys-discuss.net/10/C6374/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of deep convection in the tropical tropopause layer in West Africa: in-situ observations and mesoscale modelling" *by* F. Fierli et al.

F. Fierli et al.

f.fierli@isac.cnr.it

Received and published: 13 August 2010

We would like first to thank both reviewers for useful suggestions: we have tried to carefully consider them while restructuring the manuscript.

We consider that most critical issues raised by both reviewers (some answers are in common between the reviewers and are repeated in both replies) are:

1./ The flaw introduced by the backtrajectories methodology based on the Gheusi (QJRMS 2002) approach. For this we have implemented a method based on Eulerian tracer transport that will replace the Lagrangian approace since to the author's point of view the use of a different (and more robust) methodology do not change the

main conclusions of the paper.

2./ The frequent referencing to the companion paper by K.S. Law, Fierli F. et al. The companion paper is now published in ACPD (receiving favourable review), so we refer to it in a more detailed way throughout the paper, especially when discussing the average TTL structure and the identification of observations likely influenced by convection. The reference shortcut is also changed to Law2010.

3./ More detailed analysis of ozone profiles. Our paper shares this analysis with Law et al. as explicitly mntioned in the introduction and in section 2. We have added the analysis of August 8, using the model convective tracer fields to interpret the observed differences, adding relevant references and one sentence in the conclusions.

Several sections in the new version of the paper are substantially re-written and 7 figures modified to follow the reviewer's comments:

- Figure 1 include now the observed infrared temperatures of mesoscale convective systems influencing each analysed event - Section 2.1 Satellite observations to describe new version of figure 1 and to discuss the analysis of synoptic backtrajectories to identify the potential influence of convective outflow on each flight. - Figure 6 includes BOLAM model ice relative humidity, to be compared with CALIPSO observations - Section 3.1 contains the BOLAM validation now based on the discussion of what reported in Figs. 1, 5 and 6. - Section 3.2 contains the description of the Eulerian tracer approach - Figure 6-10 reports the diagnostics from Eulerian tracer that are used instead of Lagrangian ones (described in section 3.2) - Section 4 and 5 take into account new findings from the analysis of Eulerian tracers - Bibliography includes the references to papers appeared in ACP and ACPD in the meanwhile (Law et al., Cairo et al., Homan et al., Real et al., 2010). Referencing in the paper has been modified following suggestions from reviewer 1. Additional references have been added following indications from reviewer 2 (Riviere et al. 2006, Folkins et al., 2003, Reeves et al. 2010).

Detailed comments

1) First, I am concerned that the height and the depth of the TTL are not clearly defined by in-situ observations in the paper. The lower boundary of the TTL is supposedly defined at 350K (line 24 page 4929), but it is neither established with observations, nor it is discussed the appropriateness to adapt a general definition of the TTL on to regional (specific ?) conditions over West Africa. I am also concerned with the absence of a clear determination in the paper of the height of convective clouds for the case studies. Thereof, the vertical depth of the upper tropospheric layer sitting between the top of the convective clouds and the lower boundary of the TTL is not documented, which may discredit the scope of the results. I am still hanging on how much deep convection may influence the composition of the upper troposphere versus of the lower and upper parts of the TTL in the case studies presented here.

The average vertical profile for temperature and aerosol is reported in Law et al., 2010 , figure 2; M55 observations indicates that the main convective outflow tops at 355 K with the thermal tropopause located between 370 and 380 K. The Tropical Tropopause layer ranges between 345 and 420 K. This is now mentioned in our paper. We also agree that it is important to give an indication of the cloud top height. For this, figure 1 and figure 5 have been modified, including the infrared radiance temperature time evolution for both SEVIRI/MSG and BOLAM model. Radiance temperature from BOLAM model is estimated using the RTTOV forward radiative transfer model (see references in the text). We consider that this is the most straightforward way to give a qualitative indication of cloud top height allowing a comparison of model outputs with direct satellite observations. Moreover, we have estimated the times of match between backtrajectories and Meteosat SEVIRI observations to identify which convective system(s) might have influenced M55 observations. This information is also reported in new Fig. 1 and discussed; so, the text of section 2.1 has been substantially re-written. The estimate of the cloud top with other satellite instruments (for instance CALIOP is is not possible for each event due to the limited overpasses (at fixed hours) but is done for the 11th of August when lidar satellite-borne overpassed the convective system likely influencing the M55 observations. BOLAM relative humidity with respect to ice is added to previous

figure 9 (now figure 6) together with simultaneous CALIOP observations.

2) Mixing (vertical diffusion and convective transport) is a central atmospheric process with regards to the objective of the paper. However, wouldn't the application of its numerical representation in BOLAM be a flaw as regards the par ticular lagrangian method used in the paper ? I am concerned that the method based on the advection of air parcels positions treated as a passive tracer to reconstruct backward trajectories may be flawed in the presence of mixing represented by the physical parameterizations for ver tical diffusion and for convective transpor t. Conser vation of x, y and z positions is only valid for advection. If the x, y and z passive tracers experience sub-grid ver ti- cal diffusion and convective transpor ts, then the conser vation of the coordinates is lost. The same flaw would apply with the advection scheme that may mix the position tracers in cases of air masses of different origins experiencing a confluence down to the subgridscale. Therefore, under which conditions this method could be used to trace back different origins of air parcels if the passive tracers result from weighted averages of parcels positions coming from several places like the planetar y boundar y layer and the upper troposphere? The authors should present some tests of the method showing that this possible problem is controlled and/or has minor consequences on the results. The authors may take inspiration on the interactive discussion on ACPD about the Gheusi et al., 2004 draft paper (http://www.atmos-chem-phys-discuss.net/4/8103/2004/acpd-4-8103-2004.html).

This is a key remark, shared by reviewer 2 and we consider it the main point of concern of the analysis presented here. We agree that the trajectory cluster approach applied here has several caveats to reconstruct tracer from the PBL to the upper troposphere. We only partially used the potential of Gheusi method (Gheusi and Stein, QJRMS, 2002) since: (1) We do not analyse forward transport from the PBL region, as we only identify air parcels coming from below 500 hPa level and uplifted rapidly to the Upper Troposphere. (2) The convective age is estimated as the (backward) time elapsed between the observation and the 350 hPa level where tracer fields should be mainly modified by advection in the TTL is less dependent on the vertical diffusion and convective parameterization. Nevertheless, following the discussion of the Gheusi et al. paper in ACPD as also suggested by reviewer 1, the use of lagrangian approach would need a methodological analysis that goes beyond the objectives of our paper that is to provide an analysis of observed and modelled convective outflow. So we decided to perform substantial new work and use an approach based on Eulerian tracers (as for instance Mullendore, Durren and Holton, JGR, 2005). We have performed a model run emitting a tracer in the first model level (above the ground) each 6 hours. After 6 hours the tracer is artificially removed for the whole PBL while it is kept in the free troposphere and TTL. So, each tracer is subject to convective uplift only occurring during the 6 hours interval subsequent the emission allowing to: (1) estimate the age of convective uplift at each model grid point from a tracer temporal spectra and (2) to identify air masses in the TTL having been coming from the mixed layer. Mixed layer air fraction is estimated analogously to Bertram et al., Science, 2007 as the ratio between the tracer concentration in the TTL and the average concentration in the mixed layer. Figures 7-10 report the tracer fraction at three potential temperature levels while fig 10 reports the tracer age spectra instead of age and convective fraction estimated using the lagrangian approach. It is possible to observe that similar conclusions can be drawn from Eulerian analysis with respect to Lagrangian approach, i.e.: - August 7th and 8th tracer tops at 360 K while on August 11th convective impact reaches the TTL top (375 K). - Convective ages are different for 7/8 and 11th with more recent uplift on 11th. - Lower layers are more influenced by recent convection. Section 4 and 5 have been completely re-written to include the description of the methodology and the discussion of results based on new figures 6-10.

3) In order to reinforce the analysis of the impact of convection on the composition of the TTL using CO_2 observations and its fraction with concentrations lower than the average value minus its standard deviation (Page 4943, lines 6-7), the context on the global CO_2 seasonal cycle in 2006 and of the boundary condition for CO_2 entering the TTL over West Africa need to be documented in the paper.

This is done in section 2 explaining why low CO_2 is expected to be signature of convective uplift form continental Africa. We refer also to conclusions from Homan et al., ACP, 2010 where a detailed discussion of CO_2 distribution is reported.

Minor comments

1./ The reference Law (2010) is used many times. Unfortunately, the reference is missing and this draft paper is not available to the readers. Unless this situation can change, it would be preferable that the Fierli et al. paper stands by itself and avoids using this non-referenced work in order to reach some conclusions.

Law et al is now published ACPD and, to the author's point of view (including K.S. Law) it is preferable to avoid duplication of figures and discussion in both papers. Additional discussion on average observed profiles and flight classification is done in our paper with reference to Law2010.

2./ Page 4935, line 8 and figures 2, 3, and 4: Non-convective average profiles are provided for airborne observations. Please explain how "convective" and "non-convective" flights have been split in two families during the AMMA-SCOUT campaign and what is the representativity of the non-convective average profile.

We report the methodology to classify the flights in our paper referring to Law et al. where discussion is done in detail. We also added the results of the match between MSG observations and synoptic backtrajectories originating from the flight to confirm the potential impact of convection while discussing figure 1.

3./ Page 4942, lines 15-18: Where is it demonstrated that there is a coherent picture between BOLAM trajectories and the CALIPSO overpass ? This statement seems to override what is in the paper.

The agreement in terms of cloud extent and height between CALIPSO and BOLAM is now shown in figure 6. We agree that this do not imply that trajectories are correct. The sentence is removed. The discussion of the figure has been anticipated to the validation section (3.1) since we consider it more pertinent.

4./ Page 4936, lines 2-4: Fierli et al. use of the reference Park et al., 2007 to confirm that convection uplifts air poor in CO_2 originating from the surface. This is a clumsy usage of this reference. Indeed, Park et al. showed airborne measurements in the TTL for which high CO_2 spikes were associated with local convection near Central America (their section 3.2.1) and for which strong low CO_2 signals appear to be explained by the convective input from Amazon into the TTL (their section 3.2.2). In each case, explanations were built with backtrajectories, convective diagnoses, magnitude of the uptake of the forest at the surface and the knowledge of surface CO_2 mixing ratio. This is not the case in Fierli et al. and such a context description is needed. A reference to the paper by Homan et al. (http://www.atmos-chem-phys-discuss.net/9/25049/2009/acpd-9-25049-2009.pdf) would be more appropriate.

The reference to Park et al has been removed and the text is re-phrased referring to Homan et al. ACP 2010

5) Page 4929, lines 13-14 and page 4945, line 7:

We cannot fully understand this remark. We have removed “on average” from both sentences.

6) Page 4943, lines 14, 26: exposant and indice of the convective fraction of ice are inver ted compared to the definition Page 4941, line 3.

OK

7) Page 4944, lines 4-5: “. . . in agreement with the observations . . .”: It is not that obvious that observations (Figure 4) show two distinct thin layers of ice particles: re-read your interpretation page 4936 (lines 11-21) and page 4937 (lines 3-5).

Yes, there are in fact several thin layers of ice particles (two is removed). The vertical average profile of Fbsr clearly shows an enhancement of aerosol in the whole 355-370 K layer.

8) References: Page 4934, lines 25-26: a reference is missing for ozone source through lightning activity.

We added Schumann and Huntreiser, ACP, 2007 and Riviere et al., ACP 2006

9) Page 4938, lines 15-16: The reference to Orlandi et al. (2009) is missing.

We added the reference to the paper now published in QJRMS

10) Figures: Figures 1, 5, 6, 7 and 8: Add latitudes and longitudes.

Added in Figs. 1,5,7,8,9

11) Captions: Caption of figure 6: “. . . where air parcels cross . . .”, “Dashed box indicates . . .”, “where trajectories . . . originate”. “Color indicate . . .”

Modified accordingly to the new figure

12) Caption of figure 10: change “mice” to “ice”

Same as above

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4927, 2010.