

## ***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.**

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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 approach since to the author's point of view the use of a different (and more robust) methodology do not change the

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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 mentioned 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

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1) The paper often refers to Law et al., (2010) which is not provided as reference in the present paper and to my knowledge, is not available in ACPD yet. It is hard to estimate how the companion paper is complementary to the Fierli et al. paper, to check the compatibility between the conclusions of the two papers, and check whether the points missing in the first paper are addressed in the second one. Furthermore, Fierli et al. use results from Law et al., (2010) to reach their own results. At present, it is mandatory to add a summary of Law et al. (2010) to better identify the contribution of each study.

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. An outline of Law et al. is provided in the introduction where are also described the differences between the two papers.

2) The methods. I have several questions about the approach used, some of which are in the "minor comments" section. a) The way the trajectories are computed from the BOLAM outputs should be described in a few more line, not only referring to Gheusi and Stein (2002) since all the interpretation of the measurements is based on this method. b) The authors mention that the vertical diffusion is taken into account to modify the position of the air parcels. How this method deals with the relatively coarse grid resolution (24 km), with possibly several trajectories of different origin within the same grid mesh at the same time? How this approach deals with horizontal diffusion and mixing in the TTL where convective trajectories can encounter horizontal UT trajectories?

This is a key remark, shared by reviewer 1 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) Ozone. The authors show O<sub>3</sub> measurements from the AMMA campaign but further analysis from BOLAM trajectories is not provided. Why commenting the observed ozone profiles then? I wish the authors could give an analysis of the shape of the measured ozone profiles (not always constant at the bottom of the TTL as stated by

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the authors) in relation to the convective activity and the region where the convective uplift is from. For instance, let's take the case of August 8: the profile exhibits the typical S-shape described in Folkins et al., (2002) and very recently in Reeves et al., (2010). There is a local minimum at 348 K in the latitude range around 12 N whereas the profile is almost constant for latitudes > 13 N above 350 K. One could notice a small local minimum at 346 K, 13 N. Can the trajectories say something about this? Is the maximum altitude of the outflow in BOLAM is different at 12 N and 13 N? Are the geographical origins of the uplift different for the trajectories ending at 13 N and 12 N? If yes, this could highlight different source of ozone in the lower troposphere, in each area. Are the corresponding uplifts of the same age ( $t_c$ ) ? The same analysis should be given for the case of August 7 when ozone measurements are different at 12 N and at  $\sim 11$  N. The authors could also refer to other tropical campaigns measuring ozone in other regions of the globe (e.g. Pommereau et al., 2007; Thompson et al., 2003)

The analysis of the average profiles is done in Law et al. where it is shown that the S-shape is less pronounced than in Oceanic convection. We share the reviewer remark on the differences in ozone on August 8th in the lowermost TTL (around 355-360 K). We have added the analysis aircraft sampled the 345-360 K layer in the western part of the domain and it is possible to see that northern part of the flight path is more influenced by convection than southern part leading to higher ozone values. Large scale trajectories (described in Law et al.) shows that August 8th is largely influenced also by uplift occurring in the Indian Oceanic region. So it is possible to show that local convection tends to increase ozone in lower TTL with respect to the influence of far oceanic convection that would reduce ozone concentration. This is now discussed in the new version of the paper and connected with the analysis of August 11th where ozone profile is almost constant, due to local convection impact. A sentence on ozone observations on August 8th is added to the conclusions.

Minor comments

1) Description of BOLAM. Please give a description of the ice microphysics since com-

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parison between modeled of observed ice particles is given in the paper. Does BOLAM allow supersaturation? How many hydrometeors are taken into account? Etc...

This is done in the new version of the manuscript. We have added the references to Schultz et al., from which the scheme is derived and to Drofa et al., 2003 that describes in detail the implementation in the BOLAM model.

2) Page 4935. About Fig. 2, 3 and 4 “non-convective profile”: How are defined convective profiles and non-convective profiles in this study? This should be added and justified in the text. If this is due to the proximity of an MCS, I’m afraid such a criterion is wrong since, as shown later in the study, convection may occur significantly upwind (Niger/Nigeria border, Chad or Sudan) and play a significant role in the TTL composition.

See also minor comment 2, reviewer 1. This is done using a match criterion between synoptic backtrajectories and MSCs in the whole Sahelian region and described in section 2.

3) About Figure 9 and the CALIPSO cloud top. I wish I could see cloud top from BOLAM in this Figure. It could give an indication of how well BOLAM deals with the intensity of convection for this extreme case. In case of differences, a comment is needed: what does it imply for the comparison between observed and modeled diagnostics. Please note that an agreement between observed and modeled TCBT does not necessarily mean an agreement between and modeled cloud top.

The figure is added. See minor comment 3, reviewer 1.

4) Page 4930 line 25-28: reference to be added here.

We are still referring to Ancellet et al, 2009.

5) Page 4931 line 11. “On average 180 hPa”. The authors should give the corresponding potential temperature level and the definition of the TTL it corresponds to.

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This is now done at the beginning of section 2

6) Page 4932 line 20 “by the Meteosat Second...” to be replaced by “by the SEVIRI instrument onboard the Meteosat Second...”

Done

7) Section 2.1.1 about Fig. 1. The country borders in black are not easily visible. A green color could help. Line 10-13. “19 m/s”: Specify the altitude range for such a speed. “It is possible to argue...” It could be interesting to add trajectories in Figure 1 which link the M55 flight path with the location of convective areas (1) and (5). Section 2.1.2 Page 4933: “16 m/s” and section 2.1.3 Page 4934 “13 m/s” Same remark as for section 2.1.1. Note that m/s should be replaced by m s<sup>-1</sup> in the ACP standards.

We used now trajectories to match between M55 flight and MSG observations as described in section 2.1 that is almost re-written.

8) Page 4934 section 2.2. The instruments from which the measurements are obtained should be mentioned with associated references, even if they are briefly described in Cairo et al. (2009). The time range of the measurements should be added as well. This might be important if the reader want to date the time when convection occur upwind as shown in Figs 6, 7, and 8.

We have mentioned the times of the flight in the text (Section 2.1). We still think that referring to Cairo et al. (now in ACP) for the instrumental description would maintain our paper more readable.

9) Line 9. It could be useful to recall the formula here.

We prefer to keep the reference. We consider the formula not fundamental for the comprehension of the paper.

10) Line 12. “values of D” to be replaced by “Values of  $\Delta$ ”. This should be changed everywhere it appears in the paper.

OK. Greek alphabet is often used for aerosol depolarization; in order to keep the coherence with Cairo et al., Applied Optics 1999, we use  $\Delta_a$

11) Line 25. The authors could also refer to the typical S-shape of ozone profiles in the tropics due to low concentration of ozone in the lower troposphere which is uplifted by deep convection and detrained close to the bottom of the TTL (Folkins et al., 2002).

We show, accordingly to Law2010 that convective perturbation can be seen as an increase in O<sub>3</sub>. We have added the reference to Folkins, 2002 to explain differences with respect to the effect of oceanic convection.

12) Line 26. “through lightning activity”. Rivière et al. (2006) have shown a chemical production of ozone associated to lightning NO<sub>x</sub> close to the bottom of the TTL. It could be appropriate to refer to their work here.

OK. Added. We refer also to a review paper from Schumann and Huntreiser, 2007

13) Line 28. The authors should explicit here the typical lifetime of ozone in the UT.

Ozone lifetime in the UT is variable and a single estimate would be probably reductive. We add “high” to better explain that ozone concentration is influenced by remote physical and chemical processes.

14) Page 4935. About Fig. 2, 3 and 4 “non-convective profile”: How are defined convective profiles and non-convective profiles in this study? This should be added and justified in the text. If this is due to the proximity of an MCS, I’m afraid such a criterion is wrong since, as shown later in the study, convection may occur significantly upwind (Niger/Nigeria border, Chad or Sudan) and play a significant role in the TTL composition.

One of our main conclusions is that upwind convection has a relevant role. So the identification of convective profiles is done taking into account far convection. As mentioned above we have added a discussion on the identification of convective profiles. Non-convective profiles are identified in Law et al. due to the absence of perturbations



in tracers and aerosol profiles and the absence of match with MCSs.

15) Line 14-15. “O<sub>3</sub> concentrations” to be replaced by “O<sub>3</sub> mixing ratios”. This should be changed throughout the paper. “range between 45 and 60 ppbv at 350 K (where BSR is enhanced)”: isn’t the variability of ozone at the same level (between 42 and 62 ppbv) for latitudes close to 11.5-12 N an interesting point to discuss? Why should the comments be limited to high BSR values?

16) Line 26-27: “O<sub>3</sub> shows again...”. Most of the measurement points below 360 K correspond to values of BSR higher than 1.2. Please comment the non-constant values for latitudes below 12 N and the local minimum at 348 K, which roughly corresponds to the bottom of the TTL (Folkins et al. 2002.)

Both remarks are addressed in the new version of the paper, see also major point 3.

17) Line 13. “355-365 K layer” Why not commenting the H<sub>2</sub>O enhanced layer at 16 N with unsaturated conditions and BSR < 1.2?

This is the same layer. We have changed the latitude to 16 N.

18) Line 24 “and nearly-constant O<sub>3</sub>”: I do not agree with this general statement, since in Figs 2 and 3, O<sub>3</sub> is not constant at ~12 N. Please rephrase.

Yes, done in the new version. O<sub>3</sub> shows opposite convective signatures with respect to oceanic convection. We have moved the comment on the ozone profiles later: “The identification of convective signatures in O<sub>3</sub> is less straightforward due to high ozone variability in West Africa troposphere. As discussed further in Law2010 local sources of O<sub>3</sub> over West Africa which can be convectively uplifted are mixed with air masses advected from upwind regions and air from the lower stratosphere in the upper TTL. O<sub>3</sub> mixing ratios are variable below 355 K and increase above during the first two flights (August 7 and 8) while the last analyzed event (August 11) shows a nearly constant profile up to 360 K.”

19) Page 4938 Line 11. The authors should specify the latitude/longitude range of the

horizontal domain.

Done

20) Page 4939 Line 28 “to resolve” to be replaced by “to resolve convective transport implicitly thanks to the Kain-Fritsch subgrid scale parametrization...”

Done

21) Page 4941 Line 1.  $f_{ice}$ ,  $f_{cice}$ ,  $f_{ice}$ ,  $f_{cice}$ : the authors should use the same notation throughout the paper. How much the values of  $f_{ice}$  or  $f_{cice}$  depend on the initial H<sub>2</sub>O field?

Notation is changed in the new version. We consider that dependence from initial conditions is negligible since simulation starts on August 4 at 0 UTC, 87 hours before the first analysed flight.

22) Page 4944, lines 5-6. The authors state that the fraction between  $f_{cice}$  and  $f_{ice}$  indicates the age of ice. They should justify this more explicitly since the ice particles could be of convective origin but could have formed significantly upwind and could have been advected to the end of the trajectory.

Not exactly.  $f_i$  is the fraction of ice particles and  $f_{ci}$  is the fraction of ice formed in convective outflow. If  $f_{ic} < f_i$  means that ice clouds are not formed in the convective outflow within the model domain that covers continental Africa. They could be formed elsewhere. Nevertheless, this would imply a longer lifetime ( $> 72 / 96$  hours).

23) Line 17. About  $t_c$ . It could be interesting for the reader to deduce from  $t_c$  the time when the uplifts occur. The authors should provide the reference time of the measurements.

The lagrangian analysis has been removed from the paper. The time  $t$  is now clearly reported in the figures and the text. So it is possible to estimate the time when uplift occurs with 3 hours uncertainty due to the resolution of tracer emission (6 hours).

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24) Page 4942 line 12. Figure 8. Lower panel: the CALIPSO track should be in red. Upper panels: the country borders are not easily distinguishable (better in red).

Figure 9 (previous figure 8) is changed and cannot include the CALIPSO track.

25) About Figure 9 and the CALIPSO cloud top. I wish I could see cloud top from BOLAM in this Figure. It could give an indication of how well BOLAM deals with the intensity of convection for this extreme case. In case of differences, a comment is needed: what does it imply for the comparison between observed and modeled diagnostics. Please note that an agreement between observed and modeled TCBT does not necessarily mean an agreement between and modeled cloud top.

We agree. Figure 9 (now figure 6) has been modified adding BOLAM ice clouds

26) Page 4943. fBSR. To be compared with fice, a better criterion for fBSR should not be at the same time  $BSR > 1.2$  and  $RHi > 100\%$ ?

Saturation is almost observed with enhanced BSR, so a joint condition would not change the results.

27) Line 16-18. I do not understand this sentence. Do the authors mean fice instead of fc?

The sentence is modified in the new version. We do hope this is more clear to the readers.

28) Line 25 “;,” There is a typo.

OK

29) Lines 13-14. The authors should be more precise about the analysis of “hydration”. Do they mean that there is an upward transport of water vapour? An upward transport of ice particles which later evaporate?

It is difficult from Eulerian diagnostics to unambiguously discern the role of two pro-

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cesses. Nevertheless, low convective age can be a signature of direct hydration. Sentence is rephrased.

30) Figs 2, 3 and 4. The mean ozone or H<sub>2</sub>O profiles  $\pm \sigma$  could be more visible if shown in red. I'm not convinced that these mean profiles are relevant because of the large  $\sigma$ . Instead, I propose to plot the minimum and the maximum values of the non-convective profiles. The color bar is too thin. Caption: D to be replaced by  $\Delta$ .

We consider that standard deviations are a necessary parameters. Add max-min range would lead to a too busy figure. Red lines are fine for figure 2 but difficult to discern in figs. 3-4. Caption is modified.

31) Figs 6, 7 & 8: the flight path should be added in the right panel (353 K).

Done

32) Fig 7. The latitude axis should be stretched for a more realistic scale between latitude and longitude.

The new figure has a more realistic y/x ratio

33) Figs. 8 and 9: see above.

34) Fig. 10. Please add indices and exposures when needed in the x-axis title, and change theta into  $\theta$  or potential temperature in the y-axis title. Caption: "fmice" to be replaced by "fice", "CO<sub>2</sub>fCO<sub>2</sub>" to be replaced by "CO<sub>2</sub> fCO<sub>2</sub>".

The figure has a new caption

References:

We have added the reference to Reeves et al., 2010 in the campaign description, Riviere et al. and Folkins et al. in section 2.

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