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

General comments

The paper by Fierli et al. aims at studying the impact of deep convection on the distribution of tracers and cirrus /aerosol in the upper troposphere of Sahel, West Africa. For this purpose, they are using in situ airborne measurements onboard the M55-Geophysica, and a mesoscale simulation with the BOLAM model at a regional scale from which trajectories are computed. With these tools, they try to infer the relative role of recent/near convection with respect to further/older convection or non convective air masses in the tracers and ice particle distribution.

They conclude that both types of convection (recent or old) significantly impact the composition of the TTL below the tropopause, and they correlate the presence of cirrus clouds with convective activity.

Though the Sahel scale approach and the results given in the paper are interesting and are worth a publication in ACP, several important points arise and should be addressed before I recommend a publication in ACP. Furthermore, repeatedly in the paper, important information is missing for the understanding and the clarity of the manuscript.

Major points

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.

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?

3) Ozone. The authors show O_3 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 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 ~11 N. The authors could also refers to other tropical campaigns measuring ozone in other regions of the globe (e.g. Pommereau et al., 2007; Thompson et al., 2003)

Minor comments

Description of BOLAM. Please give a description of the ice microphysics since comparison between modeled of observed ice particles is given in the paper. Does BOLAM allow supersaturation? How many hydrometeors are taken into account? Etc…

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

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.

Page 4932 line 20 "by the Meteosat Second…" to be replaced by "by the SEVIRI instrument onboard the Meteosat Second…"

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^{-1} in the ACP standards.

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.

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

Line 12. “values of D” to be replaced by “Values of Δ”. This should be changed everywhere it appears in the paper.

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).

Line 26. “through lightning activity”. Rivière et al. (2006) have shown a chemical production of ozone associated to lighning NO_x close to the bottom of the TTL. It could be appropriate to refer to their work here.

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

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.

Line 12. “Since D” to be replaced by “since Δ”

Line 14-15. “O_3 concentrations” to be replaced by “O_3 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?

Line 26-27: “O_3 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.)

Page 4936. Line 12. “and D” to be replaced by “and Δ”.

Line 13. “355-365 K layer” Why not commenting the H_2O enhanced layer at 16 N with unsaturated conditions and BSR < 1.2?

Line 24 “and nearly-constant O_3”: I do not agree with this general statement, since in Figs 2 and 3, O_3 is not constant at ~12 N. Please rephrase.

Page 4938 Line 11. The authors should specify the latitude/longitude range of the horizontal domain.

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

Page 4941 Line 1. f^{ice}, f_c^{ice}, f_{ice}, f^c_{ice}: the authors should use the same notation throughout the paper. How much the values of f^{ice} or f_c^{ice} depend on the initial H_2O field?

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.

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).

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.

Page 4943. f_{BSR}. To be compared with f^{ice}, a better criterion for f_{BSR} should not be at the same time BSR > 1.2 and RHi > 100%?

Line 16-18. I do not understand this sentence. Do the authors mean f^{ice} instead of f_c?

Line 25 ";," There is a typo.

Page 4944, lines 5-6. The authors state that the fraction between f^c_{ice} 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.

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?

Figs 2, 3 and 4. The mean ozone or H_2O 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 σ. 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 Δ.

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

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

Figs. 8 and 9: see above.

Fig. 10. Please add indices and exposures when needed in the x-axis title, and change theta into θ or potential temperature in the y-axis title. Caption: "f_{mice}" to be replaced by "f_{ice}", "$CO_2 f_{CO2}$" to be replaced by "$CO_2\ f_{CO2}$".

Proposed references:

Folkins, I., Braun, C., Thompson, A. M., and Witte, J.: Tropical ozone as an indicator of deep convection, J. Geophys. Res., 107(D13), doi:10.1029/2001JD001178, 2002.

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Thompson, A. M., Witte, J. C., Oltmans, S. J., et al.: Southern Hemisphere Additional Ozonesondes (SHADOZ) 1998–2000 tropical ozone climatology 2. Tropospheric variability and the zonal wave-one, J. Geophys. Res., 108(D2), 8241, doi:10.1029/2002JD002241, 2003.