

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.

Anonymous Referee #1

Received and published: 12 March 2010

General comments

Fierli et al. have studied the impact of deep convection in the tropical tropopause layer (TTL) in West Africa using in-situ observations and mesoscale modelling. Their result is that deep convection can largely modify chemical composition and aerosol distribution up to the tropical tropopause. The authors suggest that, on average, deep convection occurring over the Sahel has a non negligible role in determining the tropical tropopause layer composition. Such interesting quantifications of the impact of tropical convection on to the TTL composition are tempted using a mesoscale model with the hydrostatic approximation and with a continuous nudging with satellite brightness

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temperatures for deep convection.

I am not able to recommend this paper for publication in ACP at the present time, for the reasons outlined below. I encourage the authors to resubmit their manuscript after they have addressed the following concerns about the appropriateness of the methodology used in the study.

Major points

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.

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 particular 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 vertical diffusion and for convective transport. Conservation of x, y and z positions is only valid for advection. If the x, y and z passive tracers experience sub-grid vertical diffusion and convective transports, then the conservation of the coordinates is lost. The same flaw would apply with the advection scheme that may mix the position tracers

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in cases of air masses of different origins experiencing a confluence down to the sub-gridscale. 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 planetary boundary 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>).

3) In order to reinforce the analysis of the impact of convection on the composition of the TTL using CO₂ 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 CO₂ seasonal cycle in 2006 and of the boundary condition for CO₂ entering the TTL over West Africa need to be documented in the paper.

4) Observed and modeled ice particles are central diagnoses used in the paper. However, the microphysical scheme for atmospheric ice in BOLAM is neither described nor referenced. Please, provide a description of the scheme and its validation.

Minor comments:

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.

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.

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Page 4935, lines 26-28, “. . . in correspondance to the enhanced BSR (triangles).” What do you mean ?

Page 4936, lines 2-4: Fierli et al. use of the reference Park et al., 2007 to confirm that convection uplifts air poor in CO₂ 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 CO₂ spikes were associated with local convection near Central America (their section 3.2.1) and for which strong low CO₂ 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 CO₂ 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.

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.

Page 4929, lines 13-14 and page 4945, line 7: The use of the expression “on average” tends to reduce the importance of exceptions to expedite the conclusion. A more careful statement would be needed.

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

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).

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

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

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

Captions: Caption of figure 6: "... where air parcels cross ...", "Dashed box indicates ...", "where trajectories ... originate". "Color indicate ..."

Caption of figure 10: change "mice" to "ice"

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