

## ***Interactive comment on “Medium-range mid-tropospheric transport of ozone and precursors over Africa: two numerical case-studies in dry and wet seasons” by B. Sauvage et al.***

### **Anonymous Referee #5**

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Comments on “ Medium-range mid-tropospheric transport of ozone and precursors over Africa: two numerical case-studies in dry and wet seasons ” submitted for publication in ACP by B. Sauvage, F. Gheusi, V. Thouret, J.-P. Cammas, J. Duron, J. Escobar, C. Mari, P. Mascart, V. Pont

This paper is a continuation of the paper published by Sauvage et al. in 2005 where the vertical structure of the ozone profiles recorded in West and Central Africa by the Mozaic aircraft is discussed. In this new paper, the focus is on mesoscale model simulations (by the MesoNH model developed in France by Meteo France and CNRS) to

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explain how vertical uplifting of ozone precursors can occur in air masses advected by the AEJ during the Northern Hemisphere (NH) dry season and the trade winds from Austral Africa during the wet season. Indeed for many years, ozone photochemical production in the Harmattan layer has been the main scenario to explain ozone concentrations as large as 100 ppb above the monsoon layers and convective uplifting in the southern hemisphere has been the best scenario to explain high ozone values during the wet season. So the main originality of this work is to explain how a new dynamical mechanism could uplift polluted air masses along a large area where a low level flow related to a thermal cell at  $5^{\circ}$  from the equator can interact with the equatorward branch of the Hadley cell. Two case studies have been chosen to discuss this proposed scenario.

Consequently the paper potentially deserves publication in ACP. The work is clearly described and rather convincing when discussing vertical uplifting in the InterTropical Front (ITF).

However since understanding ozone production is the bottom line of this paper, I have some difficulties about the assessment of the respective role of the different mechanisms on the ozone values recorded near 3000 m over Lagos: ozone build up in the Harmattan layer during the dry season, ozone convective uplifting over SH during the wet season and uplifting along the FIT during both seasons. There are several reasons why I believe that the paper can be improved:

1) There are very few information throughout the paper on the availability of ozone precursors in the area described as influencing the Lagos MOZAIC profiles. Indeed it is not enough to identify an uplifting mechanism without showing that it occurs at the right place and the right time to produce ozone. Also some ozone precursors are very sensitive to cloud scavenging and this implies that the convection is accurately described. Could the authors discuss these two topics ?

2) The discrepancies between the observed wind vertical profiles and the MesoNH

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profiles is a problem as the Harmattan flow regime is not reproduced in the dry season case and the altitude of the trade wind maximum is not seen at the right place by MesoNH in the wet season (for the latter it would be useful to add the 18 UT MesoNH wind profile since the observation is at 16 UT). Since the air mass trajectories will be necessary sensitive to this, it should be more thoroughly discussed. Do trajectories in air masses with the wind profile closer to the observed wind regime show the same behaviour even if it is not exactly the measurement positions ?

3) Could the ozone content of the Harmattan layer advected over Nigeria be large and deep enough to explain the L3 concentrations ? In fact a process similar to the one described in Fig. 6 over Eastern Central Africa Republic (CAR) could occur over Nigeria near Lagos (see Fig. 5) . The reason the authors discussed it over CAR is related to the strong MesoNH easterly winds (ECMWF shows weaker easterly winds) and the weak MesoNH Harmattan. A figure like figure 5 for January 30<sup>th</sup> over Lagos would be useful. More generally, what did you see in term of local pollution uplifting or in term of mixing with free tropospheric air near Lagos in the Meso-NH simulation ?

4) Still for the dry season case, the authors ruled out interaction between L2 and L3 because CO is weak. Unfortunately CO values are missing at the layer location, so how can you completely exclude the same process being responsible for L2 and L3 ? This question is important as the author objective is to show that a new mechanism is leading to the ozone increase. Also CO gradients being larger than O3 gradient, do you think that it may reduce the CO concentrations more efficiently than the ozone values of the L3 layer when mixing with free tropospheric air occurs ? How the same uplifting process make less CO in the NH hemisphere uplifting (< 150 ppb) than in the SH uplifting (> 300 ppb) ?

5) For the wet season case, this work shows that convective uplifting plays a role. But is there a way to assess the relative influence of the IFT uplifting in ozone production compared to the convective uplifting ? If I understand well, the simulation allows an estimate of the fraction of particles coming respectively from the IFT region and the

convective region, would their comparison help to distinguish their relative influence ?

Minor remarks

- 1) The colorscale of the color figures are difficult to read.
- 2) Discuss the influence of the low horizontal model resolution on its ability to resolve the convection in section 2 by providing references on this topic.
- 3) How did you select the case studies assumed to be representative of the climatology ?
- 4) The plots of the potential temperature cross section are nice to discuss the baroclinic nature of the circulation but it is not the main point of the paper and could be removed if the paper needs to be shortened.
- 5) What is the meaning of the white boxes on Fig 2 and 8 ?
- 6) In section 4.2, last sentence on line 6 “ reasonable confidence can be put in the model” I am not sure what such a statement means as it only expresses an intuitive view and not the results of a quality assessment.
- 7) In the paper as it stands, l. 12 in the conclusion, the sentence should become “This study points out the POTENTIAL role of the baroclinic low-level circulations”
- 8) In conclusion line 2, the fact that the uplifting line coincides with emission of fire products is not demonstrated in this paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4673, 2007.

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