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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.

B. Sauvage et al.

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Response to Referee T. van Noije

We thank T. van Noije for his careful and detailed review that was very helpful to improve our manuscript.

(Twan van Noije) In a previous paper (2005), Sauvage et al. presented similar back-trajectory calculations for a variety of locations including Lagos, using winds from ECMWF analyses (6-hourly as well as monthly mean). The transport routes identi-

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fied in that paper are qualitatively similar to those described in the present paper. The new aspects of this study are the use of a meso-scale model and the conclusion that there is a line of preferred location for air masses containing ozone precursors from fires to be uplifted into the lower free troposphere. The presented analysis is sound and clearly described. As a general comment, I think the authors should better motivate this study in the context of the work published in 2005. What is to be gained by using a meso-scale model rather than ECMWF analyses? I have the impression that the calculated back-trajectories are qualitatively similar, but give rather different transport time scales. To indicate the robustness and limitations of their calculations, a more quantitative comparison should be included.

We agree with Referee that it is important to clarify what motivates our study in the context of the previous paper by Sauvage et al. 2005 (thereafter S05). This is done in the revised version of the manuscript in the “Introduction” section, last paragraph “The processes of back-trajectories calculations”. We clarify that our main objective is to better understand dynamical mechanisms that induce uplift in the upper part of the lower troposphere and allows connection between fire events and ozone enhancement downwind. The present study and S05 are indeed complementary, with different goals and conclusions. Indeed S05 was a climatological approach to describe and to understand ozone monthly averages observed from 1997-2004 commercial aircraft measurements. The main goal was to interpret ozone monthly and seasonal averages, in relation with the biomass burning events and the dynamical mechanisms persistent on seasonal average (Harmattan, Trades and AEJ flows). The methodology consisted of (1) analyzing the ozone and dynamical observations monthly averaged for different sites, (2) using ATSR monthly averaged fire counts, (3) using monthly ECMWF analysis (validated with a statistical comparison using 6 hours daily analysis) to describe dynamical mechanisms seasonally persistent. The main conclusion was the regional description and the “suggestion of connections between the characteristics of the ozone monthly mean vertical profiles, the most persistent circulation patterns in the troposphere over Equatorial Africa (on monthly mean) such as the Harmattan,

the African Easterly Jet, the Trades and the regions of ozone precursors emissions by biomass burning.” (Sauvage et al., 2005, abstract). More precisely, concerning the studied region in the present paper (Gulf of Guinea, Lagos), it was shown in S05 that:

1. during the northern hemisphere biomass burning season (DJF), ozone monthly enhancements observed at Lagos and Abidjan, around 900-750hPa were related to biomass burning emissions transport inside Harmattan flow; and ozone enhancements above (750-550hPa) were related to air masses transported inside the AEJ.
2. during the southern hemisphere biomass burning season (JJA), ozone enhancements were observed at 750-550hPa over Gulf of Guinea when synoptic situation was showing trade flow from fires region reaching the coast of the Gulf of Guinea.

From this former study, two main questions were remaining: 1/ How pollutants can be uplifted to the AEJ altitude and injected into it during DJF dry season? 2/ How pollutants can be injected into the 750-550hPa trade-wind layer during the JJA wet season? These two points could not be understood using S05 paper methodology. Their approach was based on back-trajectories calculations with no consideration of turbulent and convective scheme, as noted by the Referee. Moreover most conclusions in S05 have been deduced from trajectories using monthly analyses, where meso-scale processes are smoothed. We then explain in the revised version the gain from using meso-scale model rather than ECMWF analyses (Sec. Introduction, beginning of the last paragraph “The processes É understanding of the injection”) for the present study. Therefore the present study was motivated by the previous two questions and its goal is then to better understand processes driving the injection in altitude (750-550hPa) of air masses loaded with biomass burning products. In order to investigate the meso-scale dynamical processes implied in those injections, we use here a meso-scale model to interpret two case studies. These case studies have been chosen to be representative of the ozone monthly average “characteristics” in terms of the altitude

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of the ozone enhancement, and in terms of the origin and dynamical mechanisms (ie AEJ during the dry season, trade winds during the wet season) allowing a connection between fires and ozone enhancements. As case studies have (by definition) their own specificities, we believe that it is not relevant to make a quantitative and detailed comparison between observation and wind analysis of the two present case studies on the one side, and seasonal averages of over the 1997-2004 period as in S05 on the other side. Indeed a great part of the small-scale variability visible in instantaneous pictures is removed in monthly means due to smoothing. This can explain most of the potential differences in the observed dynamical and chemical parameters, between the MOZAIC studied cases and the monthly averages. Moreover the goal of the present paper is not to quantify ozone production during the transport of fire products. This would imply simulations with chemistry, which were not performed. Furthermore that type of simulation would imply the use of correct chemical fields for initialization and inventory emissions which are missing for that region. Further work of that type is expected with the current analysis of the AMMA campaign.

Concerning the main flow characteristics, the transport is indeed quantitatively different between the present study and the S05 paper. As noted by the Referee, S05 were using monthly analyses (except for the polluted case in June-July-August season) and back-trajectories based on the latter, which are reflecting the transport mechanisms over Africa that are persistent on a monthly time-scale. Again the average process makes the wind field smoother, and as a consequence the time needed to transport air-masses can be over estimated in comparison to real cases (even if the obtained paths of the air-masses are representative on a climatological point of view). The present study uses 6 hourly ECMWF analyses to drive a meso-scale model able to simulate diffusion and convection processes (on contrary to back-trajectories calculations in S05). We thank T. van Noije for his suggestion on a more quantitative comparison. However we do not believe that a quantitative comparison between trajectories performed with the present meso-scale model and the ones performed in S05 would be appropriate and would match the goals of our study. First, differences are expected be-

tween 6-hourly and monthly ECMWF analysis, and also between a meso-scale (with turbulent and convective scheme) and a trajectory model without either turbulent or convective scheme. Second as we stated previously, the goal of the paper is not to analyze potential differences between case studies and monthly averages analysis.

Nevertheless, we explain in the revised version the reasons why a meso-scale model is necessary for the present study. We also discuss the expected quantitative differences between trajectories performed with the meso-scale model, and the ones performed with the trajectory model in S05, and between individual vertical profiles and monthly averages of the S05 study. We also describe more quantitatively the difference between simulations and observations. (Sec. Introduction, last paragraph “First case is representative ... climatological averages”; Sec. 3.1, last paragraph “Moreover the ozone monthly ... Newell et al., 1999”; Sec. 3.2; Sec 4.2)

1. As indicated in Figs. 3 and 9, there are large differences between the wind profiles over Lagos simulated by MesoNH and observed by the MOZAIC flights, especially during the monsoon season. Also, there are large differences between the wind speeds simulated with MesoNH and analyzed by ECMWF (Figs. 2 and 8). From the plots shown it remains unclear if MesoNH performs better than the ECMWF analyses. One would expect that MesoNH is able to resolve more of the fine-scale dynamical processes. However, the ECMWF winds are constrained by assimilation. How do the ECMWF analyses compare with the measurements in Figs. 3 and 9?

We have added the ECMWF wind profiles in the revised Fig.3 and Fig.9. Beyond the limitation of smoother profiles due to the coarse resolution, ECMWF analyses provide in both seasons a good agreement compared to the observed wind direction. The wind speed compares also well with the MOZAIC measurements during the wet season. However during the dry season, ECMWF analyses are not able to reproduce maximum wind speed at 3500m, contrariwise to the simulation. During the wet season, the simulation agrees less with the observations than the ECMWF analyses. This is not unexpected after a 4-day model run, especially during the wet season with the

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complex monsoon dynamics and the variable location of the ITF with respect to Lagos (lying alternatively on its northern or southern side), as noted in S05. However, as explained below and in the revised manuscript, we checked with various tests that the model failure near Lagos does not affect much the trajectory calculation and in turn the determination of the source location of the air-mass above Lagos.

2. The back-trajectories are initialized by ECMWF analyses at 1 resp. 2.5 h after the time of measurement. How does the initialization affect the calculated trajectories?

The sensitivity tests on trajectories to different initialization times did not show any meaningful difference in the trajectory paths during in the dry season case. During the wet season case study, the tests showed trajectories always originating from south-east, similar to those of Group 2 in Fig 10. This strengthens our suggestion concerning the role of the baroclinic meridional cell to uplift emissions as an important mechanism during that season. We add some comment in Section 3.3, end of paragraph one.

Given that MesoNH is able to resolve processes at finer scales, this apparent advantage is effectively lost for the early part of the trajectories. At least in some of the trajectories (especially in those labeled Group 1 in Fig. 10), uplift takes place right from the start. Does this mean that for those trajectories the relevant transport processes are actually prescribed by the ECMWF analyses? Please clarify in the text.”

The particles of Group 1 are uplifted by wet convection that occurs in the model at the sub-grid scale (as shown in Fig.11b of the ACPD paper). If the conditions (vertical profiles of temperature and humidity) are fulfilled, the parameterized convection, and the associated upward transport of particle (allowed by our Lagrangian technique), may be triggered on as soon as the model starts. This is manifestly what occurs in our simulation. On the contrary, trajectory based on ECMWF analyses (e.g. Lagranto in the revised Fig.10) would only be sensitive to resolved vertical motions, so this process could be missed (at least partly, since it is also known that organized sub-grid convection can in turn induce resolved vertical motions in numerical models by adjust-

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ment to modified thermodynamical stratification of the atmosphere). This limitation of analysis-based trajectories is clarified in the revised text.

3. To obtain realistic trajectories for particular cases, ideally one would like to apply the initial condition and constrain the model by data assimilation. Can the authors clarify why they have not done so and why they think this is not necessary? A statement about this should be included in the text.

So far there is no simple way to assimilate MOZAIC data in MesoNH. This would imply a considerable work of development. In the framework of our paper however, the trajectory analysis does not aim at explaining specific small-scale features (this would require refinements in the trajectory calculation), but to explicit pathways that are to some extent representative of the climatology of air-mass transport. From this point of view the trajectories considered in our paper do not differ much from those of S05. So we consider the numerical device used here as sufficient for our goals. A short comment on this has been added in the revised version (Sec. 2, last paragraph “Unfortunately ... calculations”).

4. The back-trajectory calculations presented by Sauvage et al. (2005) do not account for convection and diffusion. Are these processes represented in the trajectories calculated online with MesoNH? Please specify in the text of Section 2.

Indeed back-trajectories calculations did not include diffusion and convection in the 2005 paper. This is among the reasons why we use the MesoNH system for the present study. In particular the way to compute the trajectories allow to some extent to capture the vertical turbulent transport in the boundary layer, or by the sub-grid convective updrafts. This has been added in the Section 2, paragraph 3 of the revised version, as requested by the Referee.

5. The peak in the African Easterly Jet observed over Lagos at 2300 m in the January case is at the low side of the range given in the 2005 paper (550-750 hPa, p. 319). Please explain why this is the case.

Indeed the wind speed maximum in the AEJ layer (at 2400m) is in the present study 50hPa lower in altitude than in the monthly average of the S05 paper (at 700hPa, ie around 3000 m). There is also an expected quantitative difference due to smoothing (15 m/s in the present study versus 7.5 m/s in the monthly average). This discrepancy can be explained as following. First as noted previously, a monthly average and a case study can not be strictly the same. Each monthly average involves more than 15 individual vertical profiles, and one standard deviation can be as large as 40th the monthly vertical profile has been calculated by averaging dynamical and chemical parameters into 50hPa vertical layers. This average smoothes the fine scaled maxima. Second the MOZAIC wind speed measurements are instantaneous values subject to high variability. This explains the lower location of AEJ wind peak compared to monthly average.

6. The measured ozone profile shown in Fig. 1 shows an extra peak in the upper part of the AEJ layer (L3), which is not present in the monthly mean profile shown in Fig. 5 of the 2005 paper. In fact, in that paper (p. 320) it was written that the ozone concentrations within the AEJ layer decrease with altitude. Can the authors include an explanation for this discrepancy?

We clarify this point in Section 3.1 of the revised paper (paragraph 8 “Moreover...case study”). As explained previously, this discrepancy is specific to the considered case, where fine scale structures are present (as highlighted in Newell et al., 1999 study). On the contrary a monthly average will smooth the different fine scale structures, especially with the 50hPa vertical average performed in the 2005 paper (response to point 5). In order to connect the vertical structure observed in Fig. 1 to the mean AEJ layer described in S05, the AEJ layer has here to be considered as the ensemble of the three ozone substructures, namely the ozone peaks between 2200 and 4200m. Then when considering the ozone profile globally between 2200 and 4200m (L3 layer), we clearly see that the ozone concentrations decrease with altitude above L3 (like in the monthly average). This decrease is visible in the present case study, from 3700m (70

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ppbv) to 4200m (40 ppbv). Moreover, the air in the three different fine scaled peaks has a common easterly origin and has been advected inside the AEJ. This is confirmed with trajectories between 2200m and 4200m every 100m that we have realized as a sensitivity test (not shown).

Reference

Ubiquity of quasi-horizontal layers in the troposphere”, Reginald E. Newell, Valerie Thouret, John Y. N. Cho, Patrick Stoller, Alain Marengo, Herman G. Smit, Nature 398, 316 - 319, 1999

7. In Fig. 2 the wind fields are shown at 3000 m. This is actually exactly the height at which the wind speed over Lagos shows a sharp drop (Fig. 3). Can something be said about the height sensitivity of the results presented in Fig. 2 over other regions?

We thank the Referee for his suggestion. In the revised manuscript we nevertheless kept the altitude 3000m for Figure 2 for the reasons exposed in a footnote, as well as a discussion on height sensitivity.

8. To what extent are the differences in the modeled and observed wind profiles over Lagos (Fig. 3 and 9) related to spatial and temporal variability that is not well captured or to systematic biases in the model?

It is a good question but difficult to answer, since this would require to establish a “climatology” from a great number of MesoNH runs. Nevertheless we think that this knowledge is not essential for our goals since we checked that those differences do not affect the robust transport mechanisms we point out in our study.

9. The circulation pattern shown in Fig. 6 indicates that the AEJ at 22 E is located to the south of the Harmattan. The situation is different over Lagos, where the AEJ is located above the Harmattan. Please explain in the text how this can be understood in relation to the location of the ITF.

The circulation pattern in a meridional vertical section at Lagos (not shown) is actually

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very similar to that at 22°E (Fig.6). Both cross sections show the Harmattan, where blowing near the ground, being located northern of the ITF and the AEJ, but below the latter and above a surface layer of southerly wind to the south of the ITF. Over Lagos, the Harmattan layer L2 is actually the continuation in altitude of the Harmattan (ground) layer to the north that “takes off” in the ITF then forms the upper branch of the baroclinic cell (particles released from a box to the northeast of Lagos support this statement, see response to Referee1 regarding the origin of layer L2). The comment on Fig.1 has been clarified in the revised paper.

10. As explained in the 2005 paper, during the wet season two different situations may occur that lead to qualitatively different ozone distributions over Lagos. The profile measured on 14 July 2003 is of the polluted type. However, the ozone peak presented in Fig. 7 is much higher than the JJA average profile presented in Fig. 15a of the 2005 paper, which also corresponds to polluted situations. Can the authors give an explanation why this is the case?

We agree with Referee, our case study present much higher ozone mixing ratio than the JJA average presented in the 2005 paper. The reason is that the present case study can not have the same ozone mixing ratio than for a seasonal average, by definition. The JJA average represents mean of 15 individual profiles, with vertical average every 50hPa (as explained above in points 5 and 6). The JJA average also shows standard deviation in that layer up to 85 ppbv. Then the case study represents an upper limit of the seasonal average presented in the 2005 paper. This is better explained in Sec 4.1, last paragraph of the revised version. We believe this difference of ozone concentrations is not important for the present study, as explained previously. The case study is chosen to be representative of the seasonal mean in terms of the altitude location of the ozone peak, and of the origin of the air-mass - the same origin (south easterly) as for the polluted cases described in the 2005 paper. This is now clarified in the Introduction, last paragraph of the revised version. The quantification of ozone production and the quantitative difference between the present case study and seasonal averages is

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out of the goal of the present study.

11. According to the 2005 paper, more than 8 days are necessary to transport air masses from the regions of fires to the Gulf of Guinea during the wet season (p. 326). However, the trajectory calculations presented in the present paper go back only 4 days. Why would this be sufficient then?

The Referee has well noted that S05 argued that “most of the time, more than 8 days” were necessary to transport air masses from southern hemisphere to the Gulf of Guinea. However in the present case study, the connection between ozone enhancement at Lagos and biomass burning events is realized with 4 days trajectories. This is both true with trajectories calculated with the meso-scale MesoNH model and with trajectories calculated with the Lagrangian model used in S05 (not shown), both using same 6 hourly ECMWF analysis. In the present study 4 days are sufficient as trajectories reach surface levels after that time frame, collocated with biomass burning events. We clarify this difference in the revised version (Sec. 4.3, end of first paragraph “ In the present \tilde{E} (Sauvage et al., 2005) ”).

12. In Fig. 12 data are shown at four different height levels. I find this rather confusing and would prefer to see the same levels as in Fig. 5.

The four different levels where chosen to illustrate at best the key dynamical features of the southern hemisphere ITF area. However, as it was found confusing, the levels in Fig.12(b) have been changed into the common altitude $z = 2000\text{m}$, which is also the one considered for air-parcel vertical displacement in panel (a). The comment remains unchanged since the shown structures are similar. Nevertheless we keep in (a) the wind field at $h = 20\text{m}$ above the ground layer (ie at the first (terrain-following) model level) in order to show the southeasterly wind near the surface, that is similar to the Harmattan in the northern hemisphere.

Minor comments

They have been taken into account in the revised version.

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