Answer to Anonymous Referee #3

We thank Reviewer 3 for his very good suggestions to improve the manuscript. Major changes are new data produced for the Niamey analysis (role of convection and deposition on vegetation), a new section to discuss the data representativeness and a new focus and rewriting of the OD simulation section (now section 4.4). One more DLR Falcon flight was not available when writing the first version and is now added (August 16)

General comments:

"My main comment concerns the assumptions that are made to reach conclusions. Some of them are almost not justified"

We guess reviewer 3 means the interpretation of the OD simulation. This section was indeed weak in the first version and was entirely modified with a new focus: a sensitivity analysis of the possible O3 photochemical production to the level of NOx and VOC encountered during AMMA. The OD simulation plot in the first ACPD version suggested that we were able to reconstruct the observed O3 profile while the goal now is to show that a 40-50 ppb ozone enhancement is possible given the range of concentrations observed over West Africa and provided that the right meteorological conditions occur.

"There is no real discussion about the representativeness of the O3, CO and NOx collected data and pollution cases. How often could we expect that this ozone enhancement events occur for each region? Can statistics from the BOLAM model help in this? I wish a discussion could be added about this point. Furthermore, there are only 2 flights available for the Cotonou area. What to conclude about typical chemical regime of O3 production in this region?"

This is a very good point and we are aware of this. However we have to deal with the small number of data available, especially simultaneous observations of ozone, NOx and CO. Also the main focus of the paper is to identify the good conditions to observe ozone plumes related to city emissions in West Africa during the monsoon season. The statistics related to how often these conditions occurand their extrapolation to the seasonal O3 budget is not within the scope of the paper since it needs to be addressed using 3D models (e.g. see recently submitted Bouarar et al., 2011). Nevertheless, we added a new section (section 5) discussing the representativity of the data. In this section, we mainly use two ozonesonde data sets (the Thouret et al. data set for Cotonou and the DMI data set for Niamey, J. Nielsen being now a co-author). We have distinguished, in this section, the days with or without convection since it is the main condition controlling the formation of O3 in a plume in this region. This comparison with the ozonesonde record helps to put the small number of aircraft flights in a larger context.

"I also strongly recommend reorganizing Figures in order to make the comparisons between the Niamey and the Ouagadougou observations easier."

We agree and the same suggestion was made by reviewer 1. A new figure with all the vertical profiles for both Niamey and Ouagadougou is included (Figure 3). The section 3.3 was reorganized to discuss the Niamey and Ouagadougou data in parallel.

Specific comments:

1) Page 27141 lines 7 to 10. Is the 20-30 % uncertainty in the NOx values due to the hypotheses to derive NOx from NO or does it also include measurements uncertainties? Please justify the value for the photolysis rate of NO2 or add for which conditions this value corresponds to.

The NO2 photolysis rate is measured in the DLR Falcon. The NOx uncertainty given is mainly due to the hypotheses. The measurement uncertainties are in Baehr 2003 (accuracy of NO is 10% at 1 ppb, in 1s and detection limit is 2 ppt)

Tables 1 and 2: I would merge both tables together and add a column to indicate the location of the flight.

This was done.

3) Page 27145 line 1. "We can expect even higher NOx values". Please justify why you can use the August 19 and 20 NOx measurements to estimate the NOx amount on August 16. Please explicit why higher values are expected.

Reviewer 3 is right, it is the main problem we have with the NIAMEY observations compared with the Cotonou case study. All we can say is that a city plume can contain 3 ppb of NOx near Niamey (August 19 and 20) and even 5 ppb near Ouagadougou (August 16). Since CO was less than 250 ppb in Niamey for the flights on August 19 and 20 and since soil emission were also decreasing after August 17 (no more rainfall), our hypothesis is that the NOx level could have exceed 3 ppb value for flights on August 15 to 17. This is also consistent with the occurrence of the high ozone on August 16.

4) Fig. 3: please change the scale for the NOx, so that the East profile can fit into the plot between 1.5 and 2 km. I am not favorable to plot H2O and NOx with a scaling factor. Instead, I propose to plot a H2O scale and a NOx scale at the top of the corresponding panels, and keep the O3 and CO scales at the bottom.

This was changed (see figure 2)

5) Figs. 4, 5, 6, 7 and 8. I propose if possible to merge most of the panels of the figures in only one figure. On the left side, you would plot the panels for Niamey (H20, CO, O3, NOx), on the right side, you would plot the corresponding panels for Ouagadougou. The variability of CO and O3 would be plotted in another figure (left panel for Niamey, right panel for Ouagadougou). This would make the comparison between the Niamey observations and the Ouagadougou observation easier.

This was changed (see figure 3)

6) FLEXPART: the model was driven by ECMWF analyses interleaved with operational forecast. For other modeling tools in this study, the ECMWF reanalyses using AMMA soundings are used. Why not using the reanalyzes then? How would this impact the conclusion given in section 4?

In fact it is used. The text is changed in section 4.2 and 4.3.

7) Fig. 9 shows that the upper troposphere (UT) contribution can be neglected. There is no comment about that in the text. A few words should be added p27147 from line 16 since it could influence O3 concentration and its precursors. The approach is different for the Sahelian city FLEXPART analysis. No UT fraction is computed here but this could make the analysis more complete. Since the Ouagadougou measurements are made during a convectively active period, if the fraction of UT air parcels is non negligible, one could imagine that NOx produced by lightning could be transported down to the lower troposphere (LT) and modify the NOx amount in the LT (however this is not in favor of relatively low NOx amount), or more simply, that, low ozone from the convective outflow would be advected down to the LT. I do not know if this could be significant or not, but at least, this fraction should be computed to possibly rule this process out

We agree that this can be discussed in section 4.2 and 4.3, since O3 can be modified by downdraft bringing air with higher O3 from the UT. For NOx the lightning production takes place mainly in the updraft and not the downdraft and it is very important for the upper troposphere (see Huntreiser et al. 2008). The low UT fraction (Z> 8 km) for Cotonou is now discussed to rule out the transport of O3 from the UT. The UT fraction was also calculated for Niamey and Ouagadougou as suggested by reviewer 3. The same results are obtained, i.e. a low fraction less than 10%. It is not included in figures which have already a lot of information, but it is mentioned in the text.

8) Initialization of CittyCat
line 5: Please give the value of the initialization for CO and NOx for a few altitudes. Corresponding VOC values would be interesting and important to understand the O3 production regime.

New CiTTyCAT runs have been made and the figure was changed. Concentration of all precursors are now given Table 4.

L. 14: What is meant by PAN? Namely CH3COOONO2, or all the "PANs" family (CxHyCOOONO2), including PPN or higher carbon compounds?

PAN named for CH3COOONO2 only.

L. 17 about H2O and temperature initialization: how sensitive are these hypotheses on the modeling results? Did the authors perform sensitivity tests? I doubt that a constant temperature is realistic. The author should mention what chemical regime (NOx Vs. VOC with respect to the O3 production) is expected from the simulation.

On the new runs performed, meteorological data are varied on a daily cycle. Moreover, VOC and NOx concentrations are also varied in order to see the chemical cycle.

9) Figure 18. Why is the downwind NOx profile zero? I do not see any explanation in the text.

On the new runs, it is not question of profile anymore but just to estimate O3 net production in the polluted plume.