Answer to Anonymous Referee #1

We thank Reviewer 1 for his thorough reading of the manuscript (especially the numerous suggestions for English language improvements) and we apologize for the lack of explanation when discussing model results, we have tried to improve this in the new version. 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 writing for the OD simulation section (now section 4.4). One DLR Falcon flight was not available when writing the first version and is now added (August 16)

General comments:

"The values added by the model simulations are very limited."
We agree that publishing new modeling studies is not the main focus of this paper, but we feel that not doing any work of this kind will weaken the interpretation of the data. This is why we wish to keep these discussions in a single section called Model Results (section 4) : origin and age of air masses (FLEXPART), respective influence of Biomass Burning and city emissions near Cotonou (BOLAM), a sensitivity study of the O3 photochemical production for the chemical observations related to our ozone data (0D modeling with CittyCAT). We agree that the latter was the weakest part because of a lack of a full chemical description of the air masses. So this section was changed to limit our work to a sensitivity plot of O3 photochemical production for the possible ozone precursor concentrations and for a more realistic meteorological variability of the Planetary Boundary Layer (PBL).

Major comments:

"1. In the setup of the box chemical model, vertical mixing is neglected. This is not reasonable since turbulence vertical mixing would change the mixing ratios of chemical species significantly in the atmospheric boundary layer. Also it is not reasonable to using constant temperature and water vapor during a 2-day simulation (Even diurnal cycle is not considered). Thus the simulation using the box model does not really help explain the O3 production rate."
We agree with the reviewer that turbulence mixing would change mixing ratio. However, as meteorological condition suggest a very stable boundary conditions, this change should be limited. Moreover we have not enough data to estimate background concentrations to mix the plume with. Our work is comparable with lot of study where net O3 production is estimated with box model without simulated mixing (ex: Kleinman et al., 2002). Concerning the meteorological data, we agree with the reviewer that daily variation should be taken into account. Therefore, new runs have been conducted that take into account these variations.

"In Figure 18, the NOx mixing ratio over the downwind area is zero. This is contradictory with the reality and the text"
The figure and runs have been changed.

"2. The 3D model simulation is just evaluated using 3 profiles, I would not agree "the BOLAM simulation of the PBL structure is in a very good agreement with the meteorological observations" without more extensive evaluation."
Validation of the Bolam simulation is now carried out in section 4.3.1, before than the discussion of tracer transport. A figure has been added (figure 9), showing the time evolution of wind direction and wind speed for the whole simulated period over Cotonou for the BOLAM simulation and for ECMWF AMMA reanalyses. We choose to show wind speed and direction averaged over two layers: the monsoon layer (0-1 km) and the layer where the Ozone changes are greater (1.5-3 km).
As discussed in the paper, the agreement of simulated and modeled wind fields over Cotonou is satisfactory thus we think we can rely on this simulation to interpret and discuss the measurements performed above Cotonou. Simulated wind fields averaged over the simulated period have also been compared with ECMWF AMMA reanalyses in the area encompassing Cotonou and Lagos. The comparison also

show a good agreement between BOLAM wind and ECMWF analyses. This comparison has not been included in the paper but it could be provided as additional material.

“The biomass burning emissions simulation is longtime integration (one month). How can we trust the meteorology simulation over such a long time integration ? How can we trust the tracer dispersion driven by such meteorology simulation without any evaluation ?”

We agree that month-scale simulations may be not accurate in reproducing the single convective system. Nevertheless the distribution of pollutants produced by Biomass Burning emission in WAM region is driven by regional scale (Mari et al., ACP 2009). Such circulation is reasonably reproduced by Regional models and tracer transport experiments are validated and used in previously published work to characterize the distribution of biomass burning emissions.

The same simulation set up for BOLAM has been used in Real et al., ACP, 2010, AMMA special issue to interpret and characterize the observation of biomass burning in a latitudinal transect (3°N to 16°N) on August 13 2006 showing that:

- The model has a good agreement with the vertical structure of biomass burning at different latitudes
- This is in agreement with what previously found by Mari et al., 2009, ACP who studied the biomass transport on seasonal scale using flexpart model.

Furthermore, comparison with ozone soundings in Cotonou during August 2009 (see for instance Thouret et al., ACP 2009, fig. 9) shows a remarkable agreement on O3 increase around 4 km.

“3. Frequent convection is a key factor of determining O3 production in west Africa. Also the moist convection changes vertical O3 profiles significantly (Grant et al., 2008, Atmos. Environment). But the paper did not discuss the difference of the convection in terms of frequency and intensity over the three cities at all.”

The convection variability was monitored during the AMMA campaign using the 15-min MSG satellite observations. This information was used to produce Table 3 showing the days with significant convection and also to distinguish the effect of convective and non convective days in the ozone vertical profile data set from the Niamey balloon station (new figure). The topic of ozone vertical transport was discussed in details in the ACP AMMA special issue (Reeves et al. 2010, Ancellet et al. 2009). We also added a reference to the work of Grant et al. 208 who studied the effect of convection on surface ozone. For this study, discussion of convection is limited to the following point: period with reduced convective activity is necessary to observe O3 enhancement related to city emission (this is not contradictory with previous work including Grant et al. in Senegal). The main difference for the Sahel region is that rainfall maintains significant NOX soil emission after the convection days and thus also contributes to O3 production. This is better explained in section 3.3 and again in the new section 5 discussing the aircraft data representativeness.

“4. Most of the XY-plots in the paper are very hard to read.”

We apologize for this. It has been corrected. All the plots have been drawn with thicker lines and labels.

Minor comments:

Line 5, page 27136: "significant Ozone production (O3 increase of 40-50 ppbv)" took place in how long time ?

The FLEXPART study shows that the age of the air mass staying near the city is of the order of 2 days. So it is the time needed to produce the observed O3 horizontal gradient.

"*7 km-anthro" is used in the text for a few times. But the resolution of the simulation is "around 8x8 km".*

The name of the simulation has been changed. The resolution is between 7 and 8 km, this is why there is this mismatch in the first version

"Black and red asterisks in Fig. 11" on page 27151. No black asterisk is found in Fig. 11.

This figure was removed and replaced by Table 1.

On page 27151, the sentence "Close to Lagos high value for emissions are reported, while the overall area encompassing Cotonou and Lagos exhibits weak emissions" does not make sense.
We agree! It was corrected and values are now given for all the cities (see Table 1)

On page 27150, the paper introduced the biomass burning emission simulation at first, then the local anthropogenic emissions simulation. But After that the paper shows the results from the Local anthropogenic emissions simulation at first, then the results from the biomass burning emissions simulation. It is not a good sequence.
We agree. The description of the local simulation comes first.

In Figure 16, the horizontal distribution of biomass burning tracer is shown at 900 hPa, which is in the boundary layer. But the paper mentioned the advection of biomass burning from the Southern Hemisphere is at 3-5km. Does Figure 16 suggest the advection of biomass burning occur in the boundary layer?
The figure mentioned is used to show that the biomass burning plume (individuated by the highest gradient of the tracer) at 900 hPa is confined far from the coast during that day in the area of Cotonou.

What are the VOC species in the box chemical model? The initial values for those VOC species may be important for O3 production.
VOC concentrations for initialization are now given in Table 4.

Show Lagos in Figure 1 since it is very important to the O3 production in Cotonou.
This is done in the new version

On page 27149 Line 18. "O3 increase of the order of 50 ppbv in two days". What is the O3 value? Is it Daily average O3 or peak O3?
As explained before it is in fact the horizontal O3 gradient across the O3 plume edge at 2 km around noon, which is used as a proxy for this ozone increase. Thus it is a peak value but corresponding to the O3 accumulated over a two day period in the polluted plume. See new introduction for the model section.

On page 27148, Line 7, "While local ozone deposition and convective influence play a role in the differences between Niamey and Ouagadougou observations". What is the difference of convection over Niamey and Ouagadougou in terms of intensity and frequency?
This is discussed in section 3.3 and 5 and is based on the data presented in Table 3. The average intensity and frequency are not very different during the wet season at the two locations, but the data sampling make the Ouagadougou flights more representative of a period with convection while in Niamey it is not really the case.

Page 27138, Line 17, "O3 and its precursor gases, namely NOx and CO". There are other O3 precursors for example, VOCs.
We are aware of this. We meant to say that NOx and CO are often used to characterize the difference between aged air masses and air masses recently influenced by new emissions which are important for O3 production (biogenic, city plume, biomass burning). VOC concentrations are of course very important, and it is mandatory to use them for OD modeling. They are not available for our flights. We agree that it is the major uncertainty in our results of the O3 photochemical production in section 4.4. To address this, we used average BAe-146 VOC concentrations over the AMMA region, scaled to our observed CO values. We think it is the best way to take into account the speciation of the VOCs for our flight conditions. In the new version of the manuscript, VOC concentrations now vary in the sensitivity study in order to reflect this uncertainty. We see that the uncertainty is large if VOCs would be overestimated when based to the observed CO. This is not so large when we underestimate VOCs.

References are needed for "numerous mesoscale convective systems (MCSs) can developed over West Africa with a strong impact on vertical mixing of O3 and O3 precursors."
Reference to Reeves et al. 2010 and Thouret 2009 are added.

on page 27140 Mean O3 profile (total 24) is shown in Figure 2. Deep moist convection can modify tropospheric O3 profile significantly. How many of the O3 profiles were impacted by recent deep convection ? and How many were impacted by the advection from SH?

The numbers are now given in the new section 5 and indicate the maximum number of events where O3 enhancement could happen above 3 km (30% of the 24 soundings in Thouret et al. 2009)

Page 27142, Line 4, "30 June sounding". What is the time of day for the sounding ? It is important to explain the O3 profile.
The sounding took place at 10 UT. It is mentioned now in text. The diurnal cycle is indeed important to discuss the positive vertical O3 gradient between O and 500 m when the PBL develops during daytime. However, the polluted plume maximum is observed near 1 km and thus it suggests as for our case study that high O3 values can remain in the residual layer even during the night. The main reason for showing the June 30 profile (now in section 5) is to stress that the low number of polluted days in the Thouret data set is not a good parameter to discuss the statistics of the pollution events, as it requires an easterly flow to be detected. The number of days without convection is more adapted to a statistical use of the Thouret data set.

What is "coast line emissions" on page 27143?
We meant the emissions of the cities located along the coast (corrected in new version)

Page 27144, Line 15, "ozone depleted" is not appropriate for a O3 level of 35 ppbv.
It is true. We wanted to mention the importance of the surface sink. The ozone observations in Niamey are now presented in a different way in section 3.3 which was rewritten to discuss Niamey and Ouagadougou data in parallel (see comments of reviewer 2 and 3). All the profiles are now given in Fig.3.

Page 27144. "there is a significant variability in the CO concentration with 25% of the distribution above 200 ppbv and below 120 ppbv within the PBL. This indicates that the city emission is not large enough to maintain a constant high CO level as observed near the coast". Significant variability in CO does not indicate "the city emission is not large enough".
We meant the occurrence of low CO values even at low altitudes (see new text for section 3.3 where CO and NOX are discussed in parallel for Niamey and Ouagadougou).

Page 27144, "Nevertheless flights within polluted plumes occurred as shown by the occurrence of high CO values". How high CO values should be in the "polluted plumes"? What is the threshold value to define a polluted plume?
The maximum CO level in the altitude above 2km (see Figure 3) is used as a proxy for the threshold needed to identify a polluted plume at local scale. The largest value above 2 km can be related to large scale transport and it has to be exceeded to refer to a city plume. This is explained in section 3.3.

Page 27145, Line 29. "small variability around 30 ppbv" Actually the variability is larger than that shown in Figure 5 at some levels. So Figure 5 also indicate very few plumes at Niamey?
All the O3 profiles are now shown in Fig. 3 for Niamey and Ouagadougou. So the fact that there is no episode with O3 values > 40 ppb in Ouagadougou compared to Niamey, appears more clearly. The same conclusion can be derived from the Niamey balloon data (section 5). Yes there are few episodes in Niamey but they nevertheless exist. It is discussed in section 5 as requested by Reviewer 3.

Page 27146, "Differences in the anthropogenic emissions between Ouagadougou and Niamey should lead to more polluted case studies for the former", why?
We meant to say that for similar meteorological conditions an O3 production is expected to increase for the largest emissions factor as soon as the chemical regime in the air mass becomes NOX limited. There is indeed a factor of 3.5 between Niamey and Ouagadougou emissions in Table 1. Of course it also depends on the difference in the VOC/NOX ratio, but we expect to be more quickly in a NOx limited regime in Ouagadougou where HOx production will benefit from larger VOC emissions (city and forest). The main problem we can think about, is the local reduction of NOx when increasing the biogenic VOC emissions (PAN formation). This could reduce the O3 formation near Ouagadougou. In fact, the average NOx value in Ouagadougou is lower even though peak values are similar in Ouagadougou and Niamey.

Page 27146, "the vegetation cover since the local ozone sink is more efficient to remove O3 over the more vegetated city (Ouagadougou).What is "the local ozone sink" ? deposition or chemical destruction? How do you know? There is no measurement or estimation of the "local ozone sink".
We refer to the deposition on the vegetation. It is stated more directly in the section 3.3 conclusions. This topic is discussed extensively in several AMMA papers, we gave these references in our work (Saunois et al. 2010, Reeves et al. 2010).

Page 27146, "no particular day with condition favorable to pollution plume formation and ozone production have been recorded." What is the "condition"? meteorology condition or emission? Low O3 variability means either unfavorable meteorological condition or low emission.
As explained before it is related to unfavorable meteorological conditions in Ouagadougou. This is why we discuss Table 3 (convection) and the large scale transport in section 4.2. This is more explicit in the new version.

Page 27146, What is the reason to pick up the particular levels of "2250m and 3750m"?
Particles are released in a 500 m box so it is not from a single altitude level. It is better explained in section 4.1 and 4.2.

Page 27146, "since the significant O3 production in the city plume occurs during a non convective period to reduce mixing and wet removal processes, but following a very active one necessary to increase NOx soil emissions." What reduce "mixing"? what is the "wet removal processes"? wet deposition? Dry deposition may play more important roles than wet deposition to remove O3.
It is well explained in Grant 2008 (reference suggested by the reviewer) how convection will increase mixing and will dilute quickly the city plume with the tropospheric air mass composition. We meant here the role of wet deposition to reduce NOy (and thus NOx) and HCHO (less HOX) and therefore to reduce O3 production. A reference to the work of Sanderson et al. 2008 is added.

Page 27150, Line 25, the horizontal grid resolution (216oX216o) must be wrong.
It is a typo in the latex code 0.216 deg *0.216 deg