

Answer to Anonymous Referee #2

We thank Reviewer 2 for his thorough reading of the manuscript (especially the numerous suggestions for English language improvements) and his constructive suggestions to improve the readability of the manuscript.

In general the manuscript is long-winded and I still find myself not totally convinced that the conclusions the authors come to are the best explanation for the observations.

We have tried to remove unnecessary sentences and to reduce the number of figures. We are not sure which conclusion(s) is (are) questionable for reviewer 2. But we have changed the discussion about the data and model results to answer all the specific questions of the reviewers. 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 it is now added (August 16)

Also, 18 figures and 4 tables is WAY too much ... and it would be easier if all the comparisons were shown together in the first place. A lot of the plots of model output could be easily left out.

Some plots were either removed, grouped or simplified. However we feel that it is necessary to add 3 new figures to show new data (ATR-42 latitudinal section and balloon from Niamey) and a new BOLAM model validation in order to be able to answer the questions of the reviewer 1 and 3. The total number of figures is now 13.

Figures 9, 10 (did you discuss it in the text ?) I couldn't find it and I don't know what the symbols mean) and 13-17 should especially be considered for omission but this is certainly not a full prescription for fixing the figures, merely a statement that they don't work as presented and need to be rethought.

Former figure 9 and 10 are discussed in the FLEXPART model section (see p.27147 1.15 and p.27148 1.11 of acpd old version). The figure 11 and 13 of the acpd version were removed. Discussion of the Flexpart results is now in the modeling section. The section 4.2 discussing the Niamey and Ouagadougou air mass transport was simplified and more focused on the large scale transport conditions necessary for the development of an O3 plume.

More specific comments include:

1. The abstract is too long. The detail is especially excessive when discussing the modeling part of the work.

This was changed with a shorter summary of the modeling work

2. In general, someone with fresh eyes should proofread the text. I'm not going to highlight all of the linguistic issues.

We apologize for this and we thank Reviewer 2 for his English language suggestions.

5. p27139, lines 20-25: why are you talking about AEJ-S and comparing it to AEJ-N? It seems like maybe you care about AEJ-S because it is responsible for inter-hemispheric transport but you linken it to AEJ-N because that is the feature that shows up in your figure? But it is unclear and probably overly detailed.

The discussion was changed in section 2. It is simplified: AEJ-S interacts with monsoon flow and plays a role in the inter-hemispheric transport (see ref to Mari 2008) and AEJ_N limits the northward extent of the monsoon flux.

6. p27142, line 3: the influence of southerly flow is not shown by the single profile that does show O3 increase, it is shown by the fact that only one profile has appreciable ozone.

It is shown in Fig. 1 and also in the new BOLAM validation plot (Fig. 9.). The consequence is indeed a small number of layer with enhanced ozone in the Thouret et al. data set.

line 28-30: You barely show any data above 3km and the stuff you do show doesn't make it look like there is a marked difference in any measured parameter between below 3km and above. So your claim that the o3 from 1-3km is from the city and that above 3km is from biomass burning is entirely based on the model? Shouldn't there be a marked change in CO if that's the case?

The vertical profiles in Fig. 2 are now extended to 4 km. We are not sure what the reviewer meant because there are large O₃ and CO increases at 3.5 km in the west profiles corresponding to the lower boundary of the biomass burning layer south of Cotonou. The attribution to biomass burning is first based on previous work (Mari et al. 2008) and also on the fact it is seen simultaneously in the aircraft data and in the ozonesonde summer average, as expected for a large scale summer transport of biomass burning plumes. The results of the model study (FLEXPART and BOLAM model) are used to show that the biomass burning influence is weaker than the city plume at low altitudes. This is discussed in the new section 5 on data representativeness.

7. *Section 3.3: Why not show the Niamey and Ouagadougou data for O₃, CO and NO_x on the same plots? You talk about comparisons between the two locations enough that it might be helpful.*

We agree. This was changed in the new version and the text of section 3.3 was changed accordingly.

On your day-to-day variation plots, for the days with really low (<20ppb) O₃, it seems like that HAS to be titration from NO either from the city itself or from soil emissions. Though I think with the levels of CO you're seeing at the same time (in both cities) it is more likely from the city. So then if the cities are emitting enough NO_x to titrate 20ppb of O₃, how do you know you didn't just miss the ozone plume? Or is it that there aren't enough VOC's around so the chemistry is VOC limited?

We agree that there is one case in Niamey with quite low values of the order of 10-20 ppb (August 17). We also observed ozonesonde profiles in Figure 13 with low O₃ concentrations. Such low values are also observed south of Niamey above the forest (we added a new plot of the ATR-42 latitudinal cross section to discuss this point in section 3.3). On August 17 convection is strong in Niamey (see Table 3) and advection of southern air masses is significant (Flexpart analysis in section 4.2), so the best explanation we have is the advection of low ozone air masses from the south and no new ozone production during this day on which convection occurred. The effect of convection is supported by an overall shift of the ozone range between 0 and 3 km towards a lower value. A sink due to NO_x titration could be another explanation but the vertical profile shows low values up to 2.5 km while the very high CO values are below 1.5 km. We do not have VOC measurements to be able to discuss the chemical regime, although it appears to be NO_x limited in the AMMA area considering the biogenic VOC emission from the forest (Saunois et al. 2009).

8. *Why did you focus so heavily on a single day of data from Cotonou when you seem to have seen similar phenomena in Niamey on one of your flights there? You have the statistics in Niamey to say that this is just an intermittent event (which you attribute to soil NO_x emissions although I'm not convinced you've proved that. . .) so how do you know that you didn't just happen to be in Cotonou on a day with perfect conditions for ozone production? If it was just happenstance can you use the model to say generally how often you think these conditions would exist?*

The Cotonou case is interesting because first we have NO_x, O₃ and CO, on the same flight, second there is the question in Cotonou of the respective influence of biomass burning and city emission. The role of NO_x soil emission has been demonstrated for the 12N-15N latitude band in previous papers (Stewart et al., Delon et al., Saunois et al., Reeves et al. in the ACP special issue). In this work, the point is to show that the addition of city emission and soil emission makes more likely the formation of an ozone plume with values > 50 ppb near Niamey. We agree with the reviewer that very specific meteorological conditions are needed and it is difficult to observe ozone plumes with a limited number of flights. We discuss this in the section 5. It is out of the scope of this paper to derive the statistics of the days with the good conditions for ozone production (indeed this could be done with a regional model like BOLAM provided we can verify first its ability to reproduce the variability of MCS formation during the wet season). Our goal is to identify these conditions for the case of Africa based on the analysis of aircraft data.

9. *Similarly, in section 4.2: It seems like at the end of this section your conclusions are that you observed one ozone enhancement in Niamey and none in Ougadougou but that those conclusions are only useful for the specific days you were there. Is there a way you could use the model to say something more general like how often you would expect favorable ozone productions to exist?*

Yes the reviewer is right. We cannot derive a general conclusion about the ozone pollution related to Ouagadougou, but we can discuss the factors limiting the ozone production: convection and the proximity of the vegetated areas. Again the purpose of the paper is not to discuss the statistics of the days with the good conditions for ozone production (see answer to comment 8).

10. It seems like, when discussing the model results, you sometimes lose the forest for the trees and make the explanation more complicated than it needs to be. The scenario is really pretty simple, you have some background NOx level that can be attributed to soils, a city adds NOx, which, if there are enough VOCs and sunlight, will make ozone. The model and the observations either do or do not agree on whether there was ozone production and, if they agree, you can state what in the model is driving the result you saw.

Yes we agree with the scenario but we believe that we must check that (1) the air mass transport is consistent with this scenario (FLEXPART, BOLAM) (2) and that the O3 daily production rate is within the correct range to explain the occurrence of an ozone plume in two days considering the expected values of NOx et VOCs (even if the uncertainty on the VOCs is large). This is why the discussion in section 4.4 is now based on a sensitivity plot with variable concentrations of NOx and VOC.

I had to read section 4 several times before I could figure out what your overall conclusion was. We added a small conclusion in this section (4.1 and 4.2) which is now included in the model section. The FLEXPART simulations are very useful because they show that (i) in Cotonou the age of the air masses remaining near the emission region is of the order of 2 days (ii) in Niamey and Ouagadougou, conditions on the large scale transport are first a 1-2 day stagnation near the city and second the advection of biogenic emissions from the South 2-3 days before the O3 episode.

11. In your conclusions, you say that you attribute half of the observed 3ppb NOx increase in the Niamey city plume can be attributed to soil emissions using NOx concentrations observed outside the city plume but you've never shown a comparison between the city plume and the background air. Is this conclusion simply based on the fact that the background air has 1.5ppb of NOx even when you're not near a city? If so, you could state that more clearly.

Yes it is exactly what we meant. In air masses not directly influenced by the city emissions, the NOx level is estimated by the average NOx value of the BAe-146 (see 2nd paragraph of section 3.3) and by the NOx vertical profiles with no NOx enhancement in Fig. 4.e (see section 3.3 5th paragraph). The sentence was changed in the conclusion.