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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone production related to West African city emissions during the 2006 wet season AMMA campaign" *by* G. Ancellet et al.

Anonymous Referee #3

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General comments:

This work by Ancelet et al. aims at studying the ozone production around three cities of West Africa and the relative role of anthropogenic and biogenic emissions in this production. For this purpose, they use aircraft measurements of NOx, CO and O3, as well as modeling tools (Lagrangian, mesoscale and chemical box models). Such analyses are rare in this region and this makes this paper scientifically interesting. However, significant changes are needed in the manuscript before it can be accepted in ACP. My main comment concerns the assumptions that are made to reach conclusions. Some of them are almost not justified and the reader cannot

know if these assumptions impact the conclusions or not. 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? Finally, I also strongly recommend reorganizing Figures in order to make the comparisons between the Niamey and the Ouagadougou observations easier. See specific comments for details.

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.

2) Page. 27141 lines 15. The statement "So the two dataset are quite comparable as far as the O3 sampling is concerned" is ambiguous. I propose instead "So the two dataset are quite comparable with respect to the O3 production cycle". Tables 1 and 2: I would merge both tables together and add a column to indicate the location of the flight.

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.

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.

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.

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?

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 or not.

8) Initialization of CittyCat: P. 27154 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. L. 14: What is meant by PAN? Namely CH3COOONO2, or all the “PANs” family (CxHyCOOONO2), including PPN or higher carbon compounds? Line. 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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27135, 2010.