

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 #2

Received and published: 24 December 2010

This paper uses measurements of O₃, NO_x and CO in and around three African cities along with some modeling to assess the impact of urban emissions on the O₃ production budget of Africa, normally considered to be dominated by biomass burning.

This paper is interesting scientifically because of the generally small number of studies performed in Africa but I feel strongly that it needs considerable revision prior to publication in ACP.

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. Also, 18 figures and 4 tables is WAY too much. There are a number of figures

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that should be combined and the authors should reassess every figure that appears to make sure that it is necessary and adequately discussed in the text. It should be possible to present this information much more succinctly and with accompanying textual edits this will improve the manuscript as a whole. As it stands it seems like you rush through each figure only to come back to it later when you show the same thing for a different location so the reader is constantly flipping back and forth between graphs and trying to compare them when it would be easier if the comparisons were shown together in the first place. A lot of the plots of model output could be easily left out. 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.

More specific comments include:

1. The abstract is too long. The detail is especially excessive when discussing the modeling part of the work. The modeling work basically gives you confidence to make the statements you make at the end of the first paragraph so, in theory, this could be communicated by saying simply "Modeling with FLEXPART, BOLAM and CityCAT confirm these findings".
2. p27137, line 11: "the simulation shows...in a period of 2 days"
3. p27138, line 5: "large amounts of ozone..." line 11: "Thus ozone production due to African megacities remains..." line 20: "During the wet season, photochemical O₃ production in the lower troposphere is limited...but it provides a better altitudinal decoupling..." line 24: delete "indeed" line 27: "recall"→ "introduce"
4. p27139, line 4: "the observed vertical structure of ozone...is compared to modeling work using a tracer..." line 7: delete "using the aircraft observations. which can be produced in a typical city plume" line 17: "streaking"→"striking"

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

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 interhemispheric transport but you liken it to AEJ-N because that is the feature that shows up in your figure? But it is unclear and probably overly detailed.

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. 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?

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. Also, 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?

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 more

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generally how often you think these conditions would exist?

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?

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 NO_x level that can be attributed to soils, a city adds NO_x, 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. I had to read section 4 several times before I could figure out what your overall conclusion was.

11. In your conclusions, you say that you attribute half of the observed 3ppb NO_x increase in the Niamey city plume can be attributed to soil emissions using NO_x 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 NO_x even when you're not near a city? If so, you could state that more clearly.

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

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