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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What caused extreme ozone concentrations over Cotonou in December 2005?" *by* A. Minga et al.

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Authors are thankful to this reviewer. His/her comments will definitely help to improve the quality of the manuscript. Below are the answer to each specific points.

1) In section 3, you calculated TCO based on radiosondes for 20 December 2005 and compared it with TCO for other years. It would be interesting to compare it with TCO from satellite. Have you looked at any papers where TCO over West Africa is derived from satellite in order to know if such event is frequent at other location than Djougou?

We do not know which paper the referee is thinking about. As far as we know there is no published paper dedicated to TCO from satellite over West Africa with detailed analysis of retrieved TCO over this region. In Thouret et al., 2009 (this issue) we

have presented the comparison between TCO based on these ozone soundings data (RSO3) and the ones calculated by OMI-MLS, on a monthly basis. In December 2005, TCO from RSO3 is much higher than TCO from OMI-MLS. This is of course partly due to the very high ozone content on Dec, 20. On the other hand, the low sensitivity of OMI-MLS to the boundary layer do prevent the detection of such episode. However, as explained in our previous paper, December 2005 was also the anomalous month in the entire time series (2004-2007) we observed in OMI-MLS data set over the Cotonou region. Besides, we have tried to find such anomalous signal in the precursors in the different available data sets (NO2 from OMI or SCIAMACHY for example). Our research has not been successful. As suspected, it probably means that the event we report in this paper is at too small scale, spatial and temporal.

2) Lagos emissions seem very low compare to FF emissions or emissions from other cities over the world. How confident are your emissions from Lagos ? It would be useful to give uncertainties (I think the Hopkins et al., 2009 paper has been re-written with more emphasis on uncertainties).

The Hopkins et al., 2009 reference has been updated. Further commenting the uncertainties on their flux estimates is beyond the scope of the paper. The conclusion of our paper is not based on the reliability of the FF emissions over Lagos. Our objectives was simply to argue that only an exceptional event (like an explosion) may explain almost 300 ppb of ozone, never recorded before, and propose an estimate of VOC emissions allowing such high ozone production. We only used the Hopkins et al., 2009 FF emissions for the so-called "Lagos run". To be more accurate and because we needed mixing ratio, we actually asked Claire Reeves the concentrations measured by the BAe aircraft. This has been clarified in the revised manuscript. The resulting O3 concentrations from this run are consistent with the MOZAIC data (Figure 9b compared to Figure 5, as numbered in the revised version).

3) You mentioned that the purpose of the box model runs is only to formulate hypothesis scenarios to explain O3 concentrations and not to simulate the exact O3 concentra-

tions. Therefore the model was runned with no dilution which is physically inconsistent but you suppose it does not make a strong difference. I would be curious to see what kind of concentrations you obtained by diluting your plumes concentrations. You could simulate mixing in a very simple way by using background concentrations (from AMMA data for example) and a typical lifetime (between 5 and 10 days as in Arnold et al., JGR, 2007, or Real et al., ACPD, 2009 for example). You should reduce the loss of O3 you simulate at the beginning of your fire-only simulation. I am not asking you to re-done all your simulations and change all figures, but only to do a sensitivity test to see what changes it induces in simulated O3 concentrations.

The authors are very grateful to the reviewer for this suggestion. We decided to rerun all the simulations including the effect of dilution as suggested by the reviewer. New results are more realistic (no more CO constant concentration during the night) and the main conclusions of the article are unchanged. New Figure 7 shows the biomass burning run, the additional Lagos run and the additional petrochemical accident run for typical lifetimes of the plume: 5 days and 10 days.

The discussion on results in section 4 has been rewritten considering these new simulations. The paragraph describing the box model has been modified indicating how the effect of dilution is considered: “The dilution of the air parcel with the ambient air has been included in the model following the formulation of Arnold et al. (2007): mixing is simulated using background concentrations from MOZAIC and AMMA database and a typical lifetime of the air parcel. All runs are done with a 5 days and a 10 days lifetime as determined in Arnold et al. (2007) and the background concentrations used are given in Table 2.”

4) Flexpart gives information on altitude of the back-trajectories. Do these altitudes stay constant? This will justify keeping Temperature constant. Also, can you mention the water vapour content you used?

Flexpart is actually giving information on altitude. The objective of Figure 6 was mainly

to present the spatial distribution of the particles initialised over Cotonou between 800 and 1500 m altitude that were between the ground and 1500 m 24 to 60 hours before. A detailed analysis shows that most of these particles actually stay between 800 and 1500 m, especially for the last 24 hours which is the case we are investigating with the most interest, i.e., the hypothetical explosion. To make this clearer, the following sentence has been added in the text, beginning of section 4: “A further detailed analysis shows that most of the air particles stay, indeed, in the same range of altitude, 800-1500 m.” Within this altitude range, temperature is between 296 and 293 K (as shown in the Figure 4, in the revised manuscript). We are aware of the influence of the temperature on the ozone production efficiency (Figure 4 in Jacob et al., 1996 for example). However, we decided to keep it constant throughout the runs because it is not the main parameter leading the high ozone episode. The objective of the present study is clearly to investigate what kind (and quantity) of precursors are necessary to produce additional 200 ppb (from 100 to 300 ppb of O3 in a few hours). We have done different sensitivity tests with high (300 K as at the surface) and low (290 K at 2 km altitude, above the top of the layer) temperatures. The resulting O3 maximum concentrations were within 7 to 8% (close to measurements uncertainties). As expected, higher temperature leads to higher ozone. It means that the order of magnitude we are trying to characterize is still the same. Thus, to better focus on the role of the precursors, we decided to keep T and H2O constant in the run (values from the observed profile in the middle of the high ozone layer). To make this clearer, we have added the following sentence in the revised manuscript:”This is justified by the fact that air masses are travelling mostly between 800 and 1500 m as revealed by the Flexpart analysis (Fig.$\sim$8). Besides, even if ozone production efficiency varies with temperature, we rather want to focus on the role of precursors emissions.”

The water vapour content used in the simulation corresponds to a relative humidity of 70%, which is the value observed over Cotonou the 20th of December 2005 in the ozone enhanced layer (see new figure 4). This is now mentioned in the text in section 4: “For example, temperature and relative humidity are set, respectively, at 295K and

70%, and are constant throughout the simulation."

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