

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) Regarding to the initial condition used in the Master Mechanism, the authors used field measured ratios of biomass burning and urban plumes from Lagos. They should note that those measured concentrations cannot represent freshly emitted biomass/urban plumes but probably aged air masses which had already experienced a certain photochemical reactions. This referee suggested that the authors, probably, could try another method to get a better estimation. For example, the weak-diluted fresh urban plumes could be estimated by assuming that 8-10 hrs continuously emitted pollutants filled within the nocturnal boundary layer with a mixing height about 100

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or 200 m.

As mentioned by the referee, the measured concentrations above Lagos, used to initialize the "Biomass Burning + Lagos" run, cannot represent freshly emitted urban plumes. The objective of the authors in this second run is to simulate the passing of the biomass burning plume above the city of Lagos, before reaching Cotonou, through the "real" plume, i.e. the plume that has been emitted by the megacity, whatever its age. However, the authors agree that a more realistic simulation has to be done, and in response to referee #1 and to referee #3 (this question), we have made new simulations including dilution. Referee #3 is kindly asked to refer to responses to referee #1 (question 3).

2) In section 2.2, the authors used a title as "Synoptic situation". In the field of atmospheric science, "synoptic situation" generally means the overall weather condition. However, in that section there were no any discussions of weather but a general description about the site. It would be better that if the authors add one or two paragraphs to give a brief description of the general weather situation before and during this episode. The ozone data in Djougou suggests that a multi-day ozone episode occurred after 15 December, 2005, and large scale weather could favor ozone formation during that period.

We agree that the subsection "synoptic situation" was not well entitled in this version of the manuscript. It actually comes from a previous draft. In order to shorten the paper we finally cut this part and the corresponding figure. We thought that the general circulation over West Africa has been showed and detailed many times as in Sauvage et al., 2005 and as in many papers of this special issue. However, to satisfy this request, we plan to add in section 2.2 a few lines at the beginning of the second paragraph and a new figure (Figure 1 in the revised manuscript) to briefly present the synoptic situation. We have introduced in the revised version: "The confluence of the Harmattan and the monsoon flux is the main characteristic of the region in terms of synoptic situation. This latter has been described in previous papers from Sauvage et al., (2005) to many refer-

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ences of this special issue. A nice schematic zonal vertical cross section has also been proposed by Haywood et al., (2008) (their figure 12). In order to recall the main feature, Figure 1 shows the wind speed and direction at 900 hPa over West Africa averaged over December 2005, based on ECMWF analysis. At this altitude, in December, the coast of the Gulf of Guinea is clearly under the influence of the Harmattan from north-east and the monsoon flux from south-west as shown by the black arrows. It is worth noting the very low wind speed in the region around Cotonou and Lagos." Subsection 2.2 will also be renamed as "Observations sites" which seems more appropriate.

This new figure 1 gives the general description of the typical circulation for the month of December 2005, with the Harmattan from north-east and the monsoon flux from south-west. Details concerning the situation before, during and after the episode are better described with figures 4 and 5 (6 and 7 in the revised manuscript, last paragraph of section 3). The multi-day ozone episode over Djougou after 15 December, 2005 actually shows that the biomass burning is quite active at this time and the emission of precursors is high enough to produce 100 to 120 ppb of ozone, which is the maximum ever recorded in the region (as shown by the MOZAIC scatter plot). Large scale weather, like subsidence and very low wind speed, do favor ozone formation, through the accumulation of precursors and the weak dilution probably. However, because most the ozone precursors have a very short life time (a few hours), producing 200 ppb more ozone in a few days or hours necessarily implies having anomalous emissions of precursors.

3) Since the episode mainly occurred below 2 km, the author should try to give some analysis of vertical structure of the boundary layer. For example, the vertical profile of air temperature/water vapor could probably give some new insights into the sources/processes related to the ozone elevated layers. In addition, because petrochemical explosion can only have a local impact, it would be helpful if they use satellite retrievals to give a regional picture of the tropospheric ozone or precursors. These analyses may support/reject their hypothesis.

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As also asked by referee #2, we have added in the revised manuscript a new figure, figure 4 in the revised manuscript, showing the vertical profiles of O₃, T and H₂O between the ground and 3 km. It is clear that the plume is lying inside the boundary layer, capped by the temperature inversion and the strong decrease in humidity (the Harmattan layer above is drier than the monsoon layer below), characterizing thus the difference in stability and preventing the vertical mixing.

As also answered to referee #1, in Thouret et al., 2009 (this issue) we have presented the comparison between Tropospheric Column of Ozone (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 in the boundary layer prevent any 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 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.

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