

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

Anonymous Referee #1

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This manuscript interprets O₃ observations from radiosondes launched during the AMMA campaign in West Africa. Exceptional high ozone concentrations were measured on 20 December 2005, with values comparable to measurements made over very polluted cities or close to petrochemical industries. Using a chemical box model, the authors try to reconstruct air masses history and understand how such high ozone concentrations can build over this region. By using different hypothesis on emissions they show that neither biomass burning emissions nor urban emissions can explain such high concentrations and make the hypothesis of a petrochemical explosion in the region. This hypothesis is supported by the model which, in such case, is able to reproduce the high observed ozone concentrations.

This paper is based on recent data and presents the highest ozone concentrations ever

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measured in the low troposphere over Africa. The modelling work performed to explain the measurements is valuable and the paper is well written. For all these reasons I think the paper contribute to the discipline and should be published. However, some issues should be addressed:

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?

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).

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 concentrations. Therefore the model was runned with no dilution which is physically inconsistent but you supposes 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.

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 ?

Among these remarks I found the paper very pleasant to read and very interesting for

the community.

Typing corrections:

p 21013 – l.29 : by (Thouret et al) : remove brackets

Bibliographie:

Arnold et al., Quantification of mean {OH} concentration and air mass dilution rates from successive observations of non-methane hydrocarbons in single air masses, JGR, doi:10.1029/2006JD007, 2007

Real et al., Cross-hemispheric transport of central African biomass burning pollutants: implications for downwind ozone production, Atmos. Chem. Phys. Discuss., 9, 17385–17427, 2009

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