

Interactive comment on “Tracing biomass burning plumes from the Southern Hemisphere during the AMMA 2006 wet season experiment” by C. H. Mari et al.

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This manuscript discusses the meteorological situation over central Africa and its west coast during the AMMA field campaign, and the resulting transport of pollution from biomass burning either out over the Gulf of Guinea in the mid-troposphere or north across the equator (accompanied by convective uplifting to the upper troposphere). This is studied with a Lagrangian dispersion model using input from satellite observations of biomass burning fires, along with ozone soundings and satellite CO observations. The approach and the tools in the manuscript are sound, and in principle it should be possible to make it acceptable for publication in ACP. However, the manuscript is

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lacking in terms of clearly indicating its scientific significance and impact, which needs to be either made clearer, or augmented with additional analysis before acceptance to ACP.

Major comment:

My only major comment is that the scientific significance of the study is not really clear, and based on what I could put together from the places where it is touched upon in the text, it seems like it is currently insufficient to justify publication. On the one hand, it is certainly somewhat interesting to know that ozone and other by-products of biomass burning sometimes end up over the Gulf of Guinea (in the mid-troposphere) and sometimes over central Africa (in the upper troposphere), and how this relates to the regional meteorology (especially the AEJ-N/S). However, it is not clear whether there is any larger significance to this difference. For instance, is there a substantial difference in the radiative forcing of the ozone depending on which direction it goes? Does it make a difference in terms of OH production and subsequently methane lifetime? Does it have an effect on regions further downwind? Is the net amount of ozone production per unit of burned biomass different for the two directions? Can we make use of the chemistry to learn something previously unknown about the meteorology? Is there an interference to be expected from the accompanying aerosols on marine or continental clouds? Or any other significant implications of this nature?

Although a motivation is given in the introduction, in terms of further information beyond the initial observation of Sauvage et al. (2005) of mid-tropospheric ozone maxima, it needs to be made clear why we would be interested in the further details on this interhemispheric transport of biomass burning emissions, especially when the study is somewhat restricted, not including a quantitative analysis for instance of the statistics of this kind of transport (e.g., interannual variability and connection to larger scale climatic conditions and its implications), or of the ozone origins and production (which is noted will be discussed more completely in a future manuscript). In short, these results are interesting on the surface, and seem to be robust, it just needs to be made really clear

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in the manuscript what makes them special enough to justify their publication already, separately from the greater detail of the proposed future companion paper.

Specific Scientific Comments:

- AMMA (African Monsoon...) needs to be defined in the abstract
- Define the acronym GIRAFE
- It would be very helpful to have a schematic diagram showing the main transport pathways; in principle, the information is largely available in Figure 2, but it takes a good bit of looking to decipher, and the value of the paper could be increased by a carefully-drawn schematic, with labels for the Gulf of Guinea and the biomass burning regions, and arrows showing the different transport pathways.
- On Figure 5 it appears that the dashed and dotted lines are reversed (relative to the caption and the text), please check carefully
- The proposed companion paper needs to be given an in-text reference (author and rough title)
- It is appealing that the ozonesonde profiles coincide well with the break and active periods, though it would be much more convincing to have a statistical analysis, and indication of the mean degree of enhancement, and a discussion of the significance (see above)
- "The coincidence of biomass burning and cloud convection was proposed as a key mechanism for the export of biomass burning [emissions] far away from the source regions..." - is there any clear evidence of this in the simulations (they may need to be reanalyzed or run to longer particle residence times before resetting), or in the observations considered here or taken on other platforms during AMMA? This would help with the significance (see above)
- The end of section 6 is confusing: "...the impact on O3 production over the ocean

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would be limited...", and in the next sentence, "...provides an additional scenario more favorable to ozone production..." - please clarify what is meant with these two different statements.

- The forecasting tool is noted several times - it would be interesting to have an indication of the quality of the forecasts versus runs with the analysis meteorology for this particular region and type of meteorological conditions

- Conclusions: "...biomass burning plumes are found..." - this is a bit misleading, because they are only simulated (found in the model), and not observed ("found" in the normal sense)

Language:

The English usage on the whole is very good, though there are many minor grammar mistakes. Since the manuscript will need a substantial revision, I will save commenting on these in detail for the revised version, and ask that the authors watch in particular for consistency in usage, such as capitalization of terms (Southern Hemisphere versus Southern hemisphere, Atlantic Ocean versus ocean, etc.) and any other mistakes which are relatively straightforward to correct.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17339, 2007.

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