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Interactive comment on “The influence of biomass burning on tropospheric composition over the tropical Atlantic Ocean and Equatorial Africa during the West African monsoon in 2006” by J. E. Williams et al.

Anonymous Referee #2

Received and published: 9 June 2010

General Comments:

The manuscript by Williams et al. uses a number of models and observations to constrain the influence that regional fire emissions from Africa have on the chemical composition of the lower troposphere. The authors first perform a number of sensitivity studies to gauge what influence injection heights, regional emissions and temporal variability have on tropospheric composition. The authors imply, through these sensitivity studies, that higher temporal resolution of emissions better represents tropospheric concentrations of ozone and carbon monoxide. This section of the paper is

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comprehensive and straightforward, but comparisons with observations are lacking. The authors then compare profiles of O₃ and CO to AMMA observations over Equatorial Africa, concluding that the model fails to capture CO profiles or “extreme event” profiles of CO and O₃. This section is good, but could be more closely tied to the first part – relating how each of the sensitivity studies might help improve co-located comparisons between model and measurements. Finally, the authors use trajectory calculations and find that the accuracy of meteorological data may have an important effect on the model simulations. The conclusions from this section in particular do not seem to explicitly help the reader understand the specific role of biomass burning on tropospheric composition over equatorial Africa, as the title of the manuscript implies.

Overall the quality of scholarship in the abstract, introduction, and conclusions needs to be strengthened substantially prior to publication in ACP. The grammar and syntax need substantial strengthening. Further, more explicit comparisons with observations are needed in the figures and the overall quality of the figures needs improving. It's crucial in the sensitivity analysis section to make direct comparisons with MOPITT and if possible other data sources. Currently the authors describe qualitative comparisons but show no data. In addition, an overview figure is needed of Africa to show the locations of the transects, the different regions targeted in the sensitivity simulations, the location of the aircraft transects during AMMA and the MOSAIC observation sites used.

Specific Comments:

Abstract:

A broader motivating sentence is needed at the beginning to interest the reader. BB emissions, for example, have strong influence on the composition of the tropical troposphere – it would be good to elaborate exactly how – i.e. what compounds are introduced, why is this important for atm. chemistry.

All the acronyms need defining, including GFEDv2, AMMA, TM4_AMMA, ECMWF, and

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CTM.

The sentence starting with 'When adopting GFEDv2' was confusing to this reviewer. Please rewrite – what is meant by max concentrations? Over what time period, and with what vertical distribution?

Please consider rewriting the end of the abstract. Currently the reader is left with the impression that the model cannot capture patterns of observations in the region. Is this enough for publication in ACP?

Introduction

Pg. 7510 paragraph 1: The logic in second half of the introductory paragraph needs revisiting and the scholarship is not at the level required for ACP. Its not clear why interannual variability leads to uncertainty when assessing regional emissions (indeed this could allow one to isolate contributions from fires relative to other invariant components, for example). Its also unclear what the authors mean by 'events.' Does the IPCC explicitly document the case that fire emissions will increase in Africa. If so (the reader is not aware of this), the authors need to more concretely make the case this is the case and provide citations to the primary literature.

pg. 7511 In. 21: Studies are not yet conclusive on whether hotter fires have “significantly” increased injection heights. At best, studies are mixed (see Martin et al., 2010). Kahn et al., 2008 suggest that surface fire power (MW) is not a good predictor of injection height. Results from Martin et al., 2010 suggest that tropical emissions heights are not tied to fire power, but no studies have yet been completed on subtropical Africa. The authors need to provide a more balanced view of this literature.

Pg. 7512 In. 5-7: This sentence is unclear. What does “BB intensity” refer to? Also, in the second half of the sentence does not seem connected to the first half. What does “different conditions” refer to?

Methods.

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The authors have 3 separate sensitivity studies being discussed in section 2.2. 1) How do regional emissions affect troposphere composition? 2) How do varying injection heights affect troposphere composition? 3) How do temporally varying emissions affect troposphere composition?

Please consider reorganizing the text describing these studies. The first section should probably focus solely on discussing the regionally varying portion of the experiment, instead of all the other components, so as not to confuse the reader. Then move into temporal and vertical components respectively.

As discussed above, an overview figure of Africa showing the regions where BB are shut off would help the reader considerably in following the methodology of the authors.

The authors should strongly consider adding a section to their methods describing the different data sources used in their comparison. More information on MOSAIC, the field transects, and MOPITT (see below) is needed.

Results

Pg. 7516, pt 1 Please consider changing the title for this section that makes it easier for the reader to connect back to your sensitivity experiments described in the methods. For example, “The influence of biomass burning on tropical composition” could be replaced with “The simulated influence of different biomass burning parameterizations on tropical troposphere composition.”

Pg. 7519 It seems that the injection height changes mattered very little, and I wonder if they are even worth mentioning here. You assert that your injection heights estimates represent a “maximum” effect, and I wonder if they are statistically significant. Also, because the effect is opposite between CO and O3 does it go against your initial hypothesis?

Pg. 7519: In 3.2 the authors the authors qualitatively compare their results with MOPITT observations. The manuscript would be strengthened considerably if the authors

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could use MOPITT observations more directly to evaluate the different sensitivity experiments. Please add a figure on this.

Pg. 7520, Section 4 – Please consider renaming this “A comparison of model results with observations” or something similar.

Pgs. 7520-7521: In section 4, the authors show comparisons with vertical profiles of CO from Windhoek Nambia. These comparisons are important for the paper. Is it possible to also show time series at a higher temporal resolution at several different altitudes? The temporal variability differences in the different sensitivity simulations are substantial (e.g., Fig 3) and are not well evaluated using the mean monthly profiles. For example, could a figure like Fig. 4 be generated but with overlaying snapshots of MOSAIC observations from Nambia?

Also more observations sites are shown for O₃ from MOSAIC than for CO. Are CO observations available from the other sites and a symmetric comparison be made for the two gases at the same set of sites? (This relates to a more complete description of the observations in the methods and to providing higher resolution time series data on CO as described above.)

Conclusions:

Overall, your conclusions are good, but brief. Please consider adding a “Discussion” section before your Conclusions where you connect the results from this study to the broader set of expanding work on fire emissions and tropical atmospheric composition, outside of the AMMA campaigns. You might spend more time building out some of the mechanisms behind your findings. For example – please consider expanding on why you found that increased temporal resolution (in the sensitivity study) improved your results. Also – it might be helpful to qualitatively describe the relative impact that biomass burning emissions have on tropospheric chemistry – and why this is important.

Pg. 7530 In 13-18: I wonder why the maximum concentrations of CO and O₃ from

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the model simulations were more southerly than observations. Is this a meteorological difference that the model fails to capture, or is it an issue with the GFED emissions themselves? Please consider addressing this conclusion more thoroughly.

Pg. 7531 In 16-20 – You might elaborate on why there was an underestimation of middle and upper tropospheric ozone as compared to radiosonde profiles.

Minor comments

pg. 7511 In. 15: should say “have difficulty capturing”

2 Model description – please consider renaming this “Methods” since your first section is about the experimental setup.

2.1 I would rename this section “Model description” since this is actually where you describe your model.

pg. 7512 In 20: TM4_AMMA – what does this acronym stand for? Your readers may not be familiar with this particular model.

pg. 7512 In. 22: Should ‘ECMWF analysis’ be changed to ‘ECMWF reanalysis.’

2.2 I would rename this section “Experimental design” or something similar.

Pg 7514 In 20: “sequentially” should probably be replaced with “respectively”

Pg 7515 In 7: This part about regridding should be moved to Section 2.1 – it’s not really relevant here.

Pg. 7518 In 25. Sauvage et al., should be in parenthesis.

Pg. 7516 In. 7-8 – do you take the averages for the entire column, or just the lower troposphere?

Pg. 7520 In. 21-25 This information probably belongs in the Methods section

pg. 7521 In. 25 it might be helpful to define HCHO for the reader.

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Pg. 7522 ln. 11 – you mean the modeled emissions of CO are too low?

Pg. 7523 ln 4 – e.g. Laat et al. should be in parenthesis.

Pg 7523 ln 6 – Do you mean profiles of O3 concentration?

Pg. 7526 ln 2 – I wonder why the anti-correlation between the model and measurements during enhancement events? Is the model otherwise ok, and just failing to capture these stronger episodes?

Pg. 7529 ln 19-20 – Please consider rewording this sentence, using “relevant differences” instead of “interesting differences”.

Pg. 7530 ln 13-18: I wonder why the maximum concentrations of CO and O3 from the model simulations were more southerly than observations. Is this a meteorological difference that the model fails to capture, or is it an issue with the GFED emissions themselves? Please consider addressing this conclusion more thoroughly.

Pg. 7531 ln 16-20 – You might elaborate on why there was an underestimation of middle and upper tropospheric ozone as compared to radiosonde profiles.

Figures

Figs. 1 and 2 – The color scale needs to be revised to bring out more detail. A color-bar would also help the reader (same for Fig. 2)

Fig. 3 – perhaps split into 2 figures – the individual boxes are very small.

Fig 4a and Fig 4b. This should probably just be two individual figures, or – trim down some of the plots and make into a single figure. Also – it would be helpful to have labels for each color line in the figures themselves rather than simply in the figure caption, especially Fig 4b. The caption refers the reader back to 4a, and makes it difficult to interpret which line is which simulation. Also, please label the trace gases within the figure itself. For example: (a) CO (b) etc.

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