Response to anonymous referee #2

We wish to thank referee#2 for his/her review of our manuscript. Modifications made to the paper as a result of the referees suggestions and remarks had resulted in a significant revision of the manuscript. As a result we have paid attention to the main criticisms of the review in that (i) the introduction, abstract and conclusions have received attention and been improved, (ii) a comparison with more measurements available from the AMMA measurement campaign are now shown, (iii) the visualization of the results has been improved and (iv) a discussion section has now been added.

Specific comments:

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.

We know introduce the following sentences: *Biomass burning in southern Africa is the largest emission source of CO and O3 pre-cursors in the region during the West African Monsoon (WAM) between June and August. The long range transport and chemical processing of such emissions thus has the potential to exert a dominant influence on the tropical troposphere over Equatorial Africa (EA) and the tropical Atlantic Ocean (TAO).*

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

We now remove most acronyms from the abstract to avoid giving lengthy definitions.

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.

We have now re-written this section thus:

Moreover, the intensity of fires and the total area burnt in the tropics exhibit a large degree of interannual variability linked to drought conditions imposed during the El Nino-Southern Oscillation. Observations from earth-orbiting satellites are now typically used when assessing the total regional emissions from BB and wildfires for any particular year (e.g. Duncan et al., 2003). Analysing a long term record thus allows such variability to be quantified. For example analysing the total burnt area time series from the Visible and Infrared Scanner (VIRS), the Along Track Scanning Radiometer (ATRS) and MODIS sensors reveals an inter-annual variability of ~9% for NH Africa and ~12% for SH Africa over the period 1997-2004 (van der Werf et al., 2006). Changes in land use and agricultural practices also introduce a degree of variability, although on longer timescales (e.g. Kull and Laris, 2009). As climate changes, wildfires are likely to increase in both intensity and frequency (e.g. Hoffmann et al. 2009; Flannigan et al, 2009). Hence, the importance of these emission sources for the tropics will potentially be enhanced in the coming decades providing motivation to investigate whether large-scale atmospheric models can capture the variability in tropospheric composition which has been observed in the African region in recent years (e.g. Sauvage et al., 2007).

pg. 7511 ln. 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.*

We thank the referee for bringing this new study to our attention of which we were unaware at the time of submission. We have redrafted the section concerning injection heights accordingly. Moreover, it has also motivated us to revise our simulation regarding injection heights and adopt some of the conclusions from Martin et al. (2010).

Pg. 7512 ln. 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?

We remove this particular sentence and provide the following paragraph as an overview of the studies presented in the paper:

This study investigates the influence of BB activity in southern and Central Africa during JJA on the composition of the troposphere over EA for the WAM during 2006. Here we differentiate the effect that various modelling parameters (temporal variability, "effective" injection heights, model resolution and emission fluxes) used to describe BB in large-scale CTMs have on capturing both latitudinal and vertical variability for this tropical region. Moreover, by performing a set of trajectory studies around EA, we also differentiate the constraints placed on a global CTM by the quality of the meteorological dataset used to drive the model for the African region. Finally we show the effect of assimilating a more statistically robust set of measurements into the meteorological dataset for August 2006 on the origin of specific air-masses in EA for periods where enhancements of MT CO and O3 were observed.

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.

Both an overview figure and an additional section outlining the details of the measurements used are now included in the revised manuscript.

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."

The title of this section is now modified to: The simulated influence of biomass burning from different regions and parameterizations accounting for BB activity on tropical tropospheric composition

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 could use MOPITT observations more directly

A composite of the MOPITT observations between 20th July and 21st August is given in Figure 6 of Reeves et al (2010) for a longitudinal transect almost identical to the 2D transect used for the analysis in this paper. Therefore we feel that referencing it is acceptable as the paper is also in the same ACP special issue as this manuscript. From the MOPITT composite it can be seen that the number of measurements for this period between 6-12°N are minimal. Moreover, the daily overpasses have many missing data points therefore it is not ideal for comparing with the daily variability in the 2D transect as shown in Fig 4a. Instead we include a composite of CO from all of the AMMA flights made during July and August and show a direct comparison with FULL and NOSAFR simulations in a new figure introduced in the section where model output is compared with the measurements. We also provide correlation co-efficients for the other simulations for different altitude regions of the atmosphere to investigate the sensitivity studies.

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?

We have now redefined the simulation with high injection height to account for the mean distribution given in Martin et al (2010). We reduce the maximum injection height from 4km to 3km and place no emissions in the first 100m. We also now show percentage differences of the daily means in the Hövmuller plot to strengthen our conclusions and aid interpretation. Taking differences reveals that the behaviour for both trace species is similar.

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

See response above related to the data coverage issues from MOPITT for Equatorial Africa during the West African Monsoon.

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

This is now adopted.

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?

We now replace the monthly mean comparisons for the FULL, FULL_8day, HIGH_IH and HIGH_CO simulations with weekly averaged comparisons at four different pressures levels: these being 850hPa, 750hPa, 650hPa and 500hPa. We thank the referee for this suggestion as it does provide further weight to our conclusions regarding each sensitivity study.

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.)

Although we discuss the comparison of model output with MOZAIC measurements, we do not show any comparisons of O3 using MOZAIC but rather CO. This is due to (i) the O3 comparisons being shown in previous publications and (ii) the differences between each sensitivity study being rather minimal. For brevity we refrain from including an additional figure similar to Figure 7 related to O3. However, we now provide an additional figure for a comparison of all CO measurements made during the AMMA campaign against both the FULL and NOSAFR simulations, and provide a table of correlation co-efficients for the other simulations (segregating into values below 800hpa and between 500-800hPa). This provides more insight into the effect of southern BB on the guinea region and strengthens the conclusions regarding the parameterizations for introducing BB into the CTM.

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.

We now introduce a brief section where we expand on our studies so as to link the findings. We discuss the differences in terms of tropical burdens for CO and O3 in order to quantify impact of biomass burning emissions from these regions. The conclusions section has been re-written to accommodate the main findings of the new figures.

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

Previous simulations performed for 2006 with the AMMA BB emission inventory (Liousse et al, 2010) essentially show the same distribution in both CO and O3 in the 2D transect as that shown in this study using the GFEDv2 emission inventories. Examining regional emission fluxes reveals that for JJA the emission flux of all gases in the AMMA BB emission inventory is nearly double that in the GFEDv2 dataset, as well have having a different temporal distribution of the emissions. Therefore the southerly position of maximum [CO] and [O3] is principally due to the ECMWF meteorological data placing the AEJ-S a few degrees too south in the Southern Hemisphere as described in Agusti-Panareda et al (2010).

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.

We now provide a more detailed set of conclusions and expand on this point.