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Answer to Referee #1

Thank you very much for your comments and suggestions on our study. Below we have inserted, in italics, explanations and clarifications to all the points you raised and describe how we have the revised the manuscript accordingly.

6929/12 (and 6932)– Please provide a description of the setup of the WRF simulation performed to force the model with (as is done with the MM5 model later in the paper). The paper that is referred to (Flaounas et al., 2010) is not yet published.

The description of the WRF simulation setup wasn't provided in the paper because we didn't perform this simulation. It was kindly provided by Emmanouil Flaounas and Sophie Bastin as mentioned in the Acknowledgments. Instead we provided the reference to paper describing this WRF simulation (Flaounas et al). When we submitted our paper the Flaounas et al paper was undergoing review, but is now published in Climate Dynamics so we have now updated the reference.

However, following the referee's comment, we have revised the WRF application description as follows:

"The meteorological data were obtained from a WRF meteorological model regional [URL3] simulation for a period of 6 months in 2006 (from April to September), initialized with 1ºx1º final global analysis (FNL) of the National Centre for Environmental Prediction (NCEP), and covering a 40 km horizontal resolution domain (Lat -5° to 20° and Lon -25 ° to 25 °) over West Africa with 3 hours time resolution (Flaounas et al., 2010)."

- One of the recurring issues in the paper is the EF and the mismatch between the vegetation present and the EF in the data set. Do the authors know what PFT/type of vegetation the EF around 15-16N is based on? What could be the cause of this mismatch in the data set?

Based on the PFT distributions (not presented in the paper), the EF around 15 -16N is largely a result of the shrubland and herbaceous distributions. We have altered the text in section 3.1 to provide clarification:

"High values of EF are given for some northern parts of this region, consistent with the LAIv fields, and as a consequence of the shrubland and herbaceous PFT spatial distributions".

6931 - The comparison between simulated emissions on the one hand and observed mixing ratios on the other needs to be done with caution, as is acknowledged by the authors. Whereas transport is probably indeed not very important with the short lifetime of isoprene, the reaction rate probably is, and changes in temperature and light could potentially affect the lifetime of isoprene in the atmosphere (and thereby the mixing ratios). How would this affect the outcome of the comparison?

As recognised by the referee, we acknowledge in the paper that there are limitations in comparing observed concentrations with simulated emissions. For the WA1 simulations where the monthly average results are presented and where our comparison is restricted to the large spatial distribution, we believe the impact of temperature and light via the lifetime of isoprene on our conclusions will be negligible. For the WA2 simulations, which are hourly, we do state on *page 6937: "Further clouds will affect the chemistry through the photolysis rates, which in turn will also affect the lifetime of isoprene" (lines 8-10) and "This demonstrates that, in addition to strong relationships between meteorological factors and emissions of isoprene, there are very complex interactions between the land surface characteristics, the dynamics of the PBL and thus on the transport and chemistry of isoprene. Not only does this limit any comparison between calculated emissions and observed mixing ratios and isoprene, but it also illustrates the complexity of the processes that need to be represented in chemical transport models when simulating the impact of isoprene on tropospheric chemistry." (lines 19-25). In doing so we believe we have already addressed, as far as we can, the issue raised by the referee.*

6932 – Spatial and temporal resolution of the applied WRF simulation are used as an argument to use MM5 instead for the high-resolution simulations. However, WRF could be applied in the same finer resolution as well, and the change performed here is not only a change of resolution, but a change of model as well. Why did the authors use MM5 instead of WRF to obtain a higher resolution?

When this work started our idea was to try to use meteorological simulation results already available within the scientific modelling community of AMMA Project and thus skipping the step of doing the meteorological simulation which was not the main objective, just a required input. Once we analysed the results of this coarser resolution application we realized that it was important to increase the spatial and temporal resolution to attain the goal of comparing observed and simulated data. We have run MM5 for this purpose. We realise that this adds additional variables to the comparison between the low and high resolution simulations, but this is as a consequence of the data that was available to us during the project.

- The high-resolution simulations for the time of the aircraft measurements (section 5) show clearly a cloud cover pattern in the radiation data (Fig. 9), that varies in time. How well is the cloud cover represented in MM5? Is this a major uncertainty for the comparison between simulated and observed data? It would be nice to have simulated (temperature and) downward SW radiation along the flight track represented in Fig. 10 as well (e.g. for different times, as done with the simulated emissions in the second panel) to compare these with the observed values in panels 3 and 4.

Concerning your question on simulated cloud cover, we have limited data to validate the MM5 behaviour for this variable. We have looked at MeteoSat infrared images and cloud classification for the specific days and times of flights and they are broadly in agreement with the MM5 radiation maps. But this is not true if one does a cell by cell verification. Simulating the development of convective systems and thus clouds over West Africa is difficult, with the results often being model dependent, so yes this is a major uncertainty. The simulated downward radiation for 14:00 and 15:00 UTC is already presented in the top panel of figure 10, along with the simulated emissions. They can be compared to the observed downward radiation in the bottom panel. We chose not to include the simulated temperature, since it was the radiation, rather than the temperature, that primarily drove the pattern in the simulated emissions.

We have inserted the following paragraphs into Section 5:

"In an attempt to evaluate the MM5 behaviour, MeteoSat infrared images and cloud classification for the flight day and times were analysed and are broadly in agreement with the MM5 radiation maps. However, this is not true if one does a cell by cell verification. Simulating the development of convective systems and thus clouds over West Africa is difficult, with the

results often being model dependent (e.g. Sijikumar, et al., 2006, Flaounas et al., 2010), and thus a source of uncertainty for emission estimates.

...

There is also little agreement, if any, between the observed radiation and that of the model. It should be noted that the model output is an average for the hour and that as noted above changes considerably between the two hours during which the aircraft data was collected. Clearly this will impact the comparison between the modelled emissions, which are strongly dependent on the model radiation, and the observed isoprene concentrations."

6925/27 – The sentence starting with "Thus the West African ..." is unclear, please rephrase

We noticed a typo in this sentence (a miscellaneous "the"). We have deleted this so that the sentence now reads:

"Thus the West African Monsoon provides a relatively clean environment in which to study biogenic emissions and their regional effects on tropospheric chemical composition."

6928/19 – The description of MEGAN (Guenther et al., 2006) describes six PFTs. Are croplands ignored in this study?

The standard MEGAN global classification scheme includes seven PFTs: broadleaf evergreen trees, broadleaf deciduous trees, needle evergreen trees, needle deciduous trees, shrubs, crops, and grass. However, the PFT database used for the simulations considers the herbaceous PFT including grassland and cropland. This has now been made clearer in the revised manuscript. In section 3, where MEGAN inputs are described (point 1) Landcover data) one can read now:

"Four different PFTs are considered by default in MEGAN – broadleaf trees, needle leaf trees, shrublands and herbaceous (including grass and crops)."

6930/125 – Please rephrase the part of the sentence on "a detailed multilayer 5 canopy environment model". Is it 5 layers?.

The sentence was rephrased and now reads:

"...using MEGAN and a detailed multilayer canopy environment model, i.e. the canopy has been divided into 5 layers, for the calculation of leaf temperature and visible radiation fluxes."

- The figures and figure captions would be easier to read if different panels (and the descriptions in the captions accordingly) were numbered a, b, c, etc.

Different panels of figures 3 to 10 were numbered a, b, c... in the revised manuscript.