

Author's response to referee #2's comments to "Chemical and aerosol characterisation of the troposphere over West Africa during the monsoon period as part of AMMA" by C. E. Reeves et al.

We would like to thank the referee for their comments and suggestions. Below, in italics, are our responses to each point and a description of how we have revised the manuscript to take these into account.

This is a well written overview of the experimental results of aircraft measurements during the monsoon season 2006 over Africa. As the authors remark there are not many such measurements in this region- and I appreciate the particular efforts to provide a coherent synthesis of the results from the various aircraft. This alone warrants publication in ACP.

Nevertheless, I feel the paper is a little short on the interpretation of the results, in particular: were there particular hypothesis tested (beyond 'characterisation'), and did the results confirm these or not. While I understand that the goal of the paper is not to perform a full model analysis, I would appreciate if the measurement results could be brought out in terms of challenging (or confirming) literature results. Some of this has already been done (e.g. section 4.6), but unfortunately rather superficially. Some of the key results of the paper (S-shape ozone mentioned to be consistent in the text but not in the abstract, intrusions from stratosphere, relationships with biomass burning, large dust even in the wet season: is this something to be expected or rather novel. I would appreciate if the revised version would provide some more context, while recognizing that other papers (which ones?) will go in greater detail. I hope the authors will be able to address these comments.

At the end of section 1 "Introduction" we have added new text stating the scientific questions that were addressed by the aircraft campaigns and have indicated what the other papers are that address these in more detail. See response to referee #1.

Now that these aims are described and with the existing text in the introduction, it is clear, for the most part, how the results presented later in the paper confirm or challenge the original hypotheses. However, in a few cases we have added further text to strengthen this.

i.e. We have expanded the following 3 bits of text (the first 2 in section 4.4 and the latter in 4.5).

"Isoprene (Figure 14e), a biogenic species with a lifetime of only a few hours, had enhanced mixing ratios across about 6° of latitude (~700 km), showing its widespread impact on the composition of the boundary layer (Murphy et al., 2010). Its short atmospheric lifetime means that it is largely confined to locations close to its source, in this case the forested areas. Ferreira et al. (2010) confirm that this concentration distribution is largely consistent with the emission distribution predicted by MEGAN (Model of Emissions of Gases and Aerosols from Nature (Guenther et al., 2006))."

"The modelling study by Saunio et al. (2009) confirms the hypothesis that the ozone latitudinal gradient in the lower troposphere over W. Africa is a result of the rapid deposition to trees in the south and enhanced NO_x from bare soils in the north. Further, it suggests that partially oxidised VOCs, produced from isoprene oxidation in the south, may, following northwards advection, also contribute to the enhanced ozone in the north."

"To date, the integration of the observations acquired within AMMA has not advanced to the point of providing new highlights of the dynamics of the dust size distribution during its atmospheric cycle. Crumeyrolle et al. [2010] have pointed out to discrepancies between size distributions observed during a dust event in SOP1 and that expected from the Alfaro and

Gomes (2001) model. The parametrisation of the dust size distribution in chemistry-transport models should be considered a future-research priority.”

minor: p. 7118 l. 14 lifetime of SOME greenhouse gases.

Changed.

p. 7121 l. 2: which ones?

This is dealt with above by the new text on the scientific question where we have indicated which questions these other papers address in more detail.

p. 7122 section 2.3 what was actually the strategy behind the flight planning (science questions).

We have expanded section 2.3 to include the following:

“As part of the coordinated flight planning several different flight strategies were designed to address different scientific questions. Each of these flight strategies was called an Intensive Observational Period (IOP). The types of IOPs flown during SOP1 and SOP2 are given in Table 3. IOP1.1 was aimed at exploring of the inter-tropical front (ITF) and surveying of the spatial and temporal evolution of the atmosphere in the coupled monsoon-harmattan-AEJ system. IOP1.2 focussed on the description of the role of mesoscale convective systems on the emission budget of mineral dust from the Sahel. IOPs 1.4, 1.5 and 1.6 were designed to investigate the impact of local (W. African) emissions on the chemical composition of the PBL. In particular, IOPs 1.4 and 1.5 aimed at investigating the impact of biogenic emissions from soils of different moisture characteristics from different vegetation types, while IOP1.6 targeted anthropogenic emissions from urban areas. IOP2 was aimed at investigating the impact of MCSs on the transport and transformation of pollutants in air as it was convectively uplifted. This included coordinated flights with some aircraft probing the PBL prior to uplift and others sampling the UT in regions of detrainment. IOP3 targeted air masses undergoing long range transport, either into the W. African region (e.g. biomass burning plumes from the southern hemisphere) or those in the UT following convective uplift some days previously. In addition to these targeted studies, data was also collected throughout the different flights to build up a large scale picture of the chemical composition and processing of air over W. Africa.”

p. 7124 While very important I think quite some material of section 3 could go into an appendix. For most readers it will be only important to know that crosscalibration has been carefully done.

We have shortened this section by removing some of the detail and putting it into an appendix (A).

p. 7128 Section 4; this section could mention which measurements will be covered in more detailed studies, and give some hints on agreement or not.

This is dealt with above by the new text described above.

p. 7134 l. 27; and what did the Barrett study tell?

The following has been added to the text:

“The role of convection and NO_x from lightning on the composition of the UT over W. Africa has been further examined in a study using 4 global chemical transport models (Barret et al., 2010), which showed that important differences between the UT CO and ozone distributions simulated by each of the models could be explained by differences in the convective transport parameterizations and, more particularly, the altitude reached by convective updrafts. Model sensitivity studies clearly indicated that the CO maxima and the elevated ozone concentrations south of the equator are due to convective uplift of air masses impacted by Southern African biomass burning, in agreement with previous studies. Moreover, during the West African Monsoon, NO_x from lightning over W. Africa is calculated to be responsible for 10-20 ppbv enhancements in UT ozone over the tropical Atlantic.”

p. 7136 l. 3 Is there also an issue how this long range transport interacts with convection?

This is most probably the case. There are clearly issues with the way models simulate both the long range transport, as discussed by Williams et al (2010), and convection, as discussed by Barret et al, (2010). However, we consider comments on how these interact or how their interactions are dealt with in models are beyond the scope of this overview paper.

p. 7143 in the conclusion it is mentioned that SOA might or might not be consistent with models, but I don't really find a clear analysis in 4.6

Looking back at the text, we can see that the conclusions in this section are not well presented and somewhat confusing. We have therefore changed the last part of this section:

“Further when they use a simple approach based on product yields of methyl vinyl ketone (MVK), methacrolein (MACR) and SOA from chamber experiments and measurements of MVK and MACR to derive an expected SOA abundance, along with an SOA abundance derived from selected monoterpenes, they show that this approach under predicts SOA abundance by a factor of 4-15 compared to measured concentrations. This result is consistent with findings from measurements of organic aerosol mass loadings in more polluted continental environments throughout the mid-latitude northern hemisphere (Zhang et al., 2007), which show measured SOA abundance can be significantly higher than model predictions based on extrapolated chamber yield information (e.g. (Volkamer et al., 2006). The under predictions of organic matter by these calculations and those in the mid latitudes are based on yields extrapolated from chamber data obtained at higher mass concentrations. However, Capes et al. show that more recent yield data obtained under atmospherically relevant mass concentrations (Shilling et al., 2008) gives much closer agreement with measurements than yields which have been extrapolated from chamber studies at higher mass concentrations. Capes et al. (2009) also compare their measurements with global model results for the region (e.g. (Chung and Seinfeld, 2002) and show that the concentrations are consistent within the large uncertainties associated with measurement and model outputs. However since these model results used the older yield rates this result points to further differences between global model estimates and measurements of SOA, most likely caused by use of incorrect BVOC emission rates.”