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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of volatile organic compounds over West Africa" *by* J. G. Murphy et al.

Anonymous Referee #1

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General Comments:

Murphy et al. present airborne VOC measurements over West Africa (between 4N and 18 N and 3W and 4E) using a PTR-MS instrument deployed onboard the FAAM aircraft during the African Monsoon Multidisciplinary Analyses (AMMA) campaign between 17 July and 17 August 2006. Vertical and latitudinal profiles of acetonitrile, acetone, isoprene, the sum of methyl vinyl ketone and methacrolein, and benzene are presented and some processes that control the vertical distribution of these VOCs in the atmosphere are illustrated using four case studies, namely air masses sampled 1) in a convective outflow, 2) through a fair weather cumulus congestus cloud, 3) in biomass burning outflow and 4) over the ocean. These are then analyzed in terms of the chemical signatures of the aforementioned VOCs as well as carbon monoxide

(CO) and ozone (O3). This dataset obtained by flying for nearly a month over West Africa is a very important contribution and significantly improves our understanding of the VOC distribution in the atmosphere over this part of the world. I therefore recommend publication in ACP after the following specific concerns have been addressed by the authors.

Specific Comments:

Page 3864; Lines 2-5: The authors should use the updated and recommended rate coefficients as mentioned on the IUPAC website for calculating the lifetimes of the gas phase species with OH, in case the rate coefficients have changed compared to the Atkinson 1994 reference, which they have used. Also, if OH measurements were made during AMMA then why not use the measured OH concentrations for the lifetime calculations?

Section 2.2, PTR-MS: Please mention the drift tube pressure and temperature at which the instrument was operated. Information should be provided about the type of sampling inlet (e.g. forward or reverse facing etc..) and sampling system or at least a citation to it if it has already been described elsewhere.

Page 3866, Lines 10-18: What was the range of humidity range covered in the laboratory based variable humidity calibrations? Note that Equation 1 assumes a linear correction, and humidity dependent sensitivity curves are not always linear between 0 and 90% RH (e.g. Sinha et al., IJMS, 2009). Thus, it would be good to mention the estimated error due to this effect alone considering that you encountered very variable humidity during the measurements (extremely dry at 9 km and quite humid in the lower troposphere).

Page 3868, Lines 2-5: For methanol both sampled ambient air and “zeroes” were reported to have strong altitude dependence. Previous measurements over the tropics by Eerdekens et al., 2009, which the authors cite, did not seem to have such a problem for methanol, so this does appear strange. Could the authors at least speculate why

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their PTR-MS system was unable to measure methanol reliably? Based on the high backgrounds observed at other m/z signals, I am concerned that their scrubber which was heated only for a minute (see Line 2, Page 3866) may not have been working well.

Page 3869 and 3870: The authors have done a very good job in capturing the contrasting VOC signatures over different land use types (e.g. Sahel desert, forest, urban city etc..). However it is difficult to register this quantitatively from the Figures in the manuscript. Perhaps you could add a Table showing the average and std dev over these different land use types and the ocean for the VOC measurements done within the boundary layer.

Page 3872, First paragraph: The discussion about why biogenic VOC are not expected to impact ozone production significantly in the upper troposphere is not clear to me. The authors should explain this point more clearly.

Page 3877, Lines 1-4: The authors mention that they did not see enhancements in acetonitrile while sampling urban air inspite of widespread biofuel combustion in Lagos and Niamey. How do they reconcile this?

Figure 7: Below the regression line there are a number of points (coloured squares showing high values for benzene, low acetonitrile and high CO) that seem keep completely off from the rest. Can the authors clarify why?

Suggestions that the authors may want to consider for gleaning more information from their novel dataset:

1) The authors suggest that the main reason for low ozone over the forest is its dry deposition to the forest. Can they also provide an estimate of the magnitude of this flux, based on the ozone levels over the forested and non forested regions that they sampled, the forested area and the ozone vertical profiles?

2) With 85 hours of flight time over this region (well done) the authors would have sufficient data to construct diurnal profiles of some of the VOCs such as acetone and

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isoprene over the land and ocean within the boundary layer and overlay them. Note such information is obscured in Figure 6 and considering that the authors have kept the manuscript concise, an extra Figure would hardly be an issue.

Technical comment: In the reference list, Marandino et al., 2005 and Marandino et al., 2006 refer to the same citation which must be an error.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3861, 2010.