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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 #2

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The paper by Murphy et al describes the vertical and horizontal distribution of some VOCs measured by PTR-MS in West Africa as they were observed during the AMMA campaign in 2006. Measurements in this area are extremely sparse and the authors have done a great job collecting those data and summarizing them here. The vertical distribution of VOCs is described using various case studies from flights in deep and shallow convection, biomass burning layers and the marine boundary layer. The horizontal distribution is described by a latitude profile. The paper is generally well written and structured, all necessary references are given and the figures are mostly well done. I recommend publication after my comments as detailed below are addressed.

Specific comments:

1) page 3866: I don't really understand if the background was measured for 1 minute

or the catalyst heated for 1 minute. I assume the measurement was 1 minute. Please clarify.

2) Page 3867 line 26: Are the grey bars for the detection limits those for 1 second measurements?

3) Page 3868: I appreciate the clear acknowledgement of the measurement problems and the good explanation of the detection limits of the various VOCs, but I am wondering what the reason was for the high backgrounds on for example toluene. Did other masses have high backgrounds as well? It seems to me that the catalyst was not working quite right, which can be seen in the high background of mass 45. It also seems that the instrument noise was very different for different flights (see for example benzene in Figure 6). So overall there were some measurement problems, but it seems to me that the authors have carefully evaluated their dataset and used only data they have confidence in. I would still like to see more discussion on what the reasons for the instrumental problems and the varying performance of the PTR-MS were. How did the authors decide which measurements to use and what the possible uncertainties are that are caused by the problems with the catalyst. Also any explanation for the strange behavior of methanol?

4) Figure 2 shows nicely that different VOCs have very different vertical and horizontal distributions. While the vertical distribution is nicely explained by the case studies the latitude profile should be better illustrated. The minimum would be to at least show the different source regions in Figure 2, but I would suggest to plot the flight tracks in the boundary layer color coded with the mixing ratios on a map that also show the cities, desert and forest.

5) Page 3870 line 19: Please explain for people who are not so familiar with satellite measurements, what the MSG satellite is and why you show the IR brightness temperature.

6) Page 3871 line2-5: Why should the air in the early morning chemically be similar to

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the air lofted the previous evening. In the altitude profile CO and acetone are higher at 10 km than at the surface, which shows that the mixing ratios must have been higher the previous evening, when the air was lofted.

7) Figure 4: The standard deviation for benzene and acetone are missing.

8) Page 3871: Ozone is discussed in detail, but not shown in the Figures. Please add ozone to Figure 4 and Figure 5. The authors explain correctly that ozone is often low in layers observed aloft, because ozone probably was lower at the time and place where the airmass was lifted than in the free troposphere. It is therefore difficult to determine the actual ozone production in the plumes. What were typical ozone values measured during the boundary layer portions of the flight, especially during the time of lifting? Are there data available from some of the flights? And what values were observed in the layers?

9) Figure 5: Please also add ozone here.

10) Conclusions: page 3877 line 7: Here the authors say that low acetone "can be explained by ocean uptake", which is contrast to the text. This sentence should be changed to something like: Low acetone might be cause by ocean uptake or long residence times of the measured air masses over the ocean without significant acetone sources.

Editorial comments:

1) page 3863 line 8: please explain abbreviation of FAAM

2) page 3864 line 6: Holzinger et al 99 does not show that biomass burning is the main source of acetonitrile, better to use de Gouw et al 2003c here.

3) Page 3871 line 12: typo: close to those observed

4) Marandino et al is twice in the reference list.

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