Atmos. Chem. Phys. Discuss., 9, C7427–C7430, 2009
www.atmos-chem-phys-discuss.net/9/C7427/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence of the impact of deep convection on reactive volatile organic compounds in the upper tropical troposphere during the AMMA experiment in West Africa" *by* J. Bechara et al.

Anonymous Referee #1

Received and published: 24 November 2009

Bechara et al. describe a new set of aircraft observations of VOC collected over West Africa during the AMMA experiment in August 2006. The observations show a strong influence of deep convection on the non-methane hydrocarbon (NMHC) budget. Observations of NMHC taken in the upper troposphere were categorized (by CO, O3 and RH) into being either background or convectively influenced. Air masses that were deemed convectively influenced showed both higher mean NMHC concentrations and were more variable than observations made in air that was determined not to be influenced by deep convection. The authors also describe their observations in the lower

troposphere and comment on the spatial variability in NMHC that is linked to heterogeneity in the surface fluxes. These observations are an important contribution to the growing data set in the tropical upper troposphere. The authors continue to show that: i) OH reactivity is a factor of 2 higher in convectively influenced air masses, which is largely attributed to convective enhancements in isoprene, ii) the fraction of lower tropospheric air transported by deep convection is of order 40%, and iii) vertical transport timescales can be estimated by the observed isoprene in the UT. In general, the paper is well written and presents novel observations that should be published in ACP. However, I have several comments that need to be addressed prior to publication.

General comments: 1. Both CO and O3 are highly variable in the lower troposphere and when lofted to the upper troposphere, they maintain their initial condition for several days. When combined they may be indicative of a "recent" convective event, however that signature will persist much longer than that of many of the short lived NMHC this analysis attempts to characterize. This could potentially contribute to the observed variability seen in the samples determined to be "convectively influenced". It is mentioned that NOx was measured on the aircraft, was NOy measured. If so, the ratio of NOx/NOy provides a more robust indicator of the time and air mass has been in the UT since being convectively lofted and permits the determination of freshly convected air.

2. Recent direct measurements of OH reactivity by Mao et al [ACP 9, 163, 2009] show OH reactivity in background UT air that was of order 1 sec-1, where over 60% of the reactivity was attributed to CO? The values presented here indicate that R(CO+OH) was significantly less. I am surprised by the statement that CO contributes a negligible amount to OH reactivity? What about methane? Also, I am confused by the origin of the isoprene reactivity numbers given in line 25, page 20324. These numbers (0.9 and 1 sec-1) appear to be larger than the total reactivity?

3. In section 6.4, please include the individual numbers for each of the compounds used to calculate f. How many compounds were used? This analysis will likely be

more robust for compounds that exhibit little variability in the lower troposphere and have a strong vertical gradient. Where did the uncertainty come from in this analysis? Is this the standard deviation of the mean?

4. The assumptions made in using equation 4 (section 6.5) are that isoprene is not lost in deep convection, that OH is 2x106 in cloud, and the air mass is sampled immediately after being detrained into the upper troposphere. There is little discussion of the uncertainty in each of these approximations. For example the uncertainty in the assumed OH has to be at least 50% if not 100%. I would expect the time since detrainment would be at least 20-30minutes unless the aircraft was flying directly through the anvil region. I would expect in cloud OH levels to be much lower than 2x106 and the UT processing time to be significant. It is likely that these values cancel one another in some complex fashion. I expect that the uncertainty quoted (10minutes) is grossly smaller than the true uncertainty in this determination.

5. Throughout the analysis it would be helpful to have an indication of how many samples are included in each mean. For example, in Figure 6, is the variability in the C-UT simply because there are only 5 data points? I would suggest adding the number of points to these figures.

Specific comments: Line 5, page 20310 – define AMOVOC

Line 26, Page 20310 – give an altitude range for this or convert to m sec-1

Line 22, Page 20312 – While these observations certainly add significantly to a poorly sampled region of the atmosphere, I would not say that AMMA “exhaustively explored” West Africa.

Line 22, Page 20319 – typo (in situ produced in situ)

Line 3, Page 20321 – Is the size of the box in figure 5 the spatial extent to which the samples were integrated over? It would be helpful to know spatially how big these boxes are.

Line 11, Page 20325 – typo (specie)

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20309, 2009.