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## ***Interactive comment on “Fine-mode organic mass concentrations and sources in the Amazonian wet season (AMAZE-08)” by Q. Chen et al.***

**Anonymous Referee #4**

Received and published: 6 November 2014

The study presents new information of chemical composition of organic aerosols in the Amazon. The results are important and interesting, especially since there are few previous studies in this environment.

My main concern is that the results in some parts seem to be over-interpreted, and some conclusions are not well-supported by the data analysis. This regards the interpretation of the PMF factors OOA-2 and OOA-3, which only differ slightly in average mass spectrum and time variation. The associated uncertainty should be better reflected in the abstract, discussion and conclusions. OOA-2 is “implicated as associated with reactive uptake of isoprene products... to haze, fog or cloud droplets”, while OOA-3 is “consistent with fresh production SOM by a mechanism of gas-phase oxidation of BVOC followed by gas-to-particle conversion of the oxidation products” (from abstract).

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These strong statements should be modified to better reflect the data, uncertainty and analysis.

The supplemental material is quite extensive. 34 pages supplement to an 18 page manuscript seem like too much for a journal like ACP. I suggest moving some of the most important sections and figures to the main text.

The results and discussion section should include at least some information on meteorology (boundary layer heights and wind patterns) at the site, especially in the discussion of time series and daily patterns.

Previous studies in the Amazon have observed primary biological aerosols – were there any indications of primary biological aerosols in the present data?

Specific comments:

Page 16154 line 15: Oxidation of BVOC correlates with other factors beside sunlight – please be more specific.

Page 16157 line 5-8: The statement on density seems misplaced here.

p. 16158 and supplement: The authors spend quite some time to discuss that they do not observe organosulfates in their data. The sulfate levels are quite low, and since organosulfates are only expected to constitute a fraction of this, their concentrations should be very low. What is the detection limit for organosulfates using AMS?

P. 16160 L4: Would biomass burning in Africa give a covariance of BC with sulfate at your site?

P16161 L17-22: The argument is not clear here. First you say that the charges nearly balanced and then you state the relative proportions within each group, which does not provide information on the degree of charge balance. In several places you mention that the aerosols were acidic. How did you arrive at that conclusion based on your data? P16161 L. 8-11: statements 1) and 2) seem somewhat redundant.

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P. 16163 L26: The correlations are not very strong, probably due to a mix of sources for the tracers. A term like “correlated somewhat” would describe this better.

P. 16164 L. 11: The peak at  $m/z$  82 is not prominent in Fig. 4c. I agree that is present and visible, but it is far from being prominent. Furthermore the relative intensities of  $m/z$  53 and 55 in OOA-2 and OOA-3 are too similar to state that 53 is “elevated”. The text should better reflect the data here.

P. 16164 L25: What is number of observations included in calculation of the correlation coefficient? How many days did the correlation include?

P. 16164L27-29: Should be rewritten to reflect that your OOA-2 factor somewhat resemble the spectra of Budisulistiorini et al, not the other way around.

P16165 L1: Unless the reference Kuwata et al., has been updated, the statement should be removed, since it does not add to the readers understanding of the present work.

P16165 L5: How did you measure/calculate that the particles were acidic? How acidic were they?

P16165 L7-22: The paragraph is quite speculative and ends with a conclusion that OOA-2 represents both degree of emission and oxidation of isoprene, as well as uptake of these products in particles, in addition to uptake and aqueous phase processes involving other precursors. It would be nice if the latter part could be supported by data or more precise references. I agree that these are possible explanations for the factor, but it is important that the text (here, as well as in the conclusion and abstract) reflects the associated uncertainty.

P16165 L23-: The figure (4d) does not support the statement that  $m/z$  55 and 91 are “distinct”.

P16166 L1-2: “a linear combination of the three chamber spectra largely reproduced the OOA-3 factor”. The ion of highest intensity  $m/z$  29 is not well modeled with the

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chamber spectra – why?

P16166 L 10-11: For how many days did OOA-3 track the BVOC concentrations?

P16167 L18: “the OOA-2 factor was consistent with the reactive uptake of isoprene oxidation products”. Consistent is a very strong word here. It is more fair to say that it showed similarities or it was interpreted as uptake isoprene oxidation products on aerosol particles.

P16166 L22-24. Was there changes in the boundary layer height during the day?

Figure 6: The legends should be more clearly described in the figure text. What is “the OH family”? It is very interesting that these experiments have been performed at different concentrations. The results show the variation in the relative intensities, and thus give information on the uncertainty in these studies.

Supplement

Please check that the references to figures and tables are in chronological order.

P5: Please update and add the reference Canagaratna et al. 2013. “The contributions of organonitrates and organosulfates, detected as inorganic nitrate or sulfate ions, to the elemental ratios were negligible because their low mass concentrations.” The statement should have a reference to the discussion later in the Appendix.

P6: “The two types of filters show reasonable agreement”. The statement should be further elaborated or at least give a reference to data in the manuscript or published papers.

P12: “The prominent C7H7+ accompanied with a negligible signal of C6H13+ at m/z 85 is also unique for the three biogenic SOA studied in the chamber”. The reader cannot judge the statement since only data for BSOA is given. Give at least a reference here. The last sentence of “prominent” m/z 82 should be corrected as discussed above.

I suggest to move section D to the main manuscript. Furthermore, I suggest that Figure

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S9 is included in the main manuscript.

Please extend the figure texts for figures S12-S14, so the reader is able to understand them without reading the full text.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16151, 2014.

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