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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An overview of the Amazonian Aerosol Characterization Experiment 2008 (AMAZE-08)" *by* S. T. Martin et al.

Anonymous Referee #1

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There are three purposes to the paper: 1) provide a technical, meteorological and physicochemical overview of the AMAZE-08 project,

2) summarize some important findings from already published work, and

3) present new data regarding the aerosol microphsyical, chemical, and cloud-forming properties

This paper is very well written and contains material of interest to ACP readers. It will be an oft-cited work since it provides an essential overview of the AMAZE-08 experimental setup and an important summary of the aerosol properties. I recommend it be published in ACP with some minor improvements, most important of which are a clearer discussion of the aerosol size distributions.

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Interactive Discussion

Discussion Paper

Major comments:

Although the vast majority of the manuscript is exceptionally clear, the paragraph in Section 3.2.1 immediately preceding 3.2.2 needs some improvement.

1) The Hatch-Choate relationships are not defined or referenced, and it is not clear what is meant by saying that the “parameters for the surface and volume distributions satisfy” the relationships. They must, since they are lognormal descriptions of the data, and the Hatch-Choate relationships are defined for lognormal distributions.

2) In the following 2 sentences, the goodness of closure is defined as the “residuals of log10 values”. I don’t know what this means, and an equation (or at least a reference) would be extremely helpful.

3) In the next sentence (lines 5-7), “the parameterization is constrained within 50% of the measured median”. Does this mean than the parameterization and the median agree within 50%, or was there some constraint in the fitting process?

4) Figure 12, which accompanies this section, shows number, surface, volume, and residual plots, nicely displayed. However, I am not at all in favor of the logarithmic ordinate. It masks the proportional contribution of each size class to the total number (or surface, or volume). The objective should not be to “show all the data”, which a log-log plot allows, but to show which data are important to that moment. Furthermore, the area under the curve is not meaningful in a log-log plot, while it is in a lin-log plot.

5) Also in Fig. 12, there is a big discrepancy for particles with diameters > 2 um between the APS and the OPC. The OPC shows a remarkable coarse mode extending beyond 10 um, while the APS shows a peak near 3 um and declining concentrations for larger sizes. These are very substantial differences, and one would arrive at two different conclusions regarding the magnitude of the coarse mode from these 2 measurements. Which is correct? How big are the uncertainties for diameters > 3 um? Clearly the OPC data do not fit the bimodal lognormal model presented as a parame-

terization to be used for modeling, so this issue needs to be discussed.

6) In section 3.2.2, page 18158, beginning line 9, there is a discussion of how the fractional abundances of PBAP markers compares to laboratory results, producing an upper limit of 5% primary particle contribution toward the accumulation mode. This is an important point, yet the data supporting this statement are not presented. Could this be shown graphically, or at least expanded upon a bit with references and some more meat so that we understand how this findingâĂŤwhich contradicts past published reportsâĂŤis determined?

7) Page 18158, lines 25-28. The reconciliation of past PBAP measurements and the current findings is the size being evaluated. For the present study, the diameter is given as 0.06-0.8 um. Is this vacuum aerodynamic diameter, in which case the true diameter range might be ~0.04-0.55 um? A quantitative discussion of the AMS size classes is needed in the Measurements section, since this ends up being an important part of one of the key findings of the paper.

8) Section 3.2.3, p. 18159, line 25. There is an opportunity here to compare results with the non-biomass burning, non-dust cases from the AMMA campaign, for example Capes et al., Atmos. Chem. Phys., 9, 3841-3850, 2009. I am struck by the consistent, relatively low amounts of SOA produced over both the African and Amazonian rainforest. This suggests a very short lifetime in the rainy season.

8) Section 3.2.4, p. 18161, lines 17-19. The suggestion is made that the volatility spectra from AMAZE are consistent with SOA formation, while those from polluted environments (California and Mexico City) are consistent with POA emissions. I think this is a misstatement of the findings from polluted regions; most of the organic mass there is SOA. It may have a different structure, and thus a different volatility distribution, than the biogenic SOA in AMAZE-08, but it is largely secondary nonetheless.

Minor comments/errors:

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Interactive Comment

9) p. 18142, line 3. “conveyor belt” is confusing terminology, given the meteorological phenomena of “warm conveyor belts” etc.

10) p. 18146, line 23. Was this a tethered balloon, and if so, what was its altitude range?

11) p. 18147, line 11. A switch was made from Celsius (earlier in the text) to Kelvin.

12) p. 18147, line 21. Was the inlet cutoff calculated or measured?

13) p.18148, line 20-21. Was there any condensation possible in the gas sampling lines in the 23C trailer (could be slightly cooler than ambient)?

14) P. 18152, line 1. Figure S4 was not readable in the PDF; all other figures were fine.

15) p. 18179. Add list of acronyms for “organization” at bottom of table.

16) p. 18181. Table 3 is very nicely presented.

17) p. 18185, Fig. 4. Would you consider adding a regional map showing the same trajectories at a scale to see the regional transport patterns vs. location of cities?

18) p. 18186. The figure caption does not describe the wind roses adequately. Do the rings indicate fraction of time (e.g., 0.1, 0.2, 0.3) in each sector?

19) p. 18192, Fig. 11. The black line (PBL top) is described in the text but not in the figure caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18139, 2010.