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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Single particle characterization using a light scattering module coupled to a time-of-flight aerosol mass spectrometer" *by* E. S. Cross et al.

E. S. Cross et al.

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Responses to Anonymous Referee #3

We have identified 12 comments made by Reviewer #3. We have labeled the comments alphabetically A - L. Each comment is listed below. Individual responses are provided directly below each comment.

Comment A: In summary, I do not see any way in which publication would be appropriate without a major rewrite, omitting technological details (or moving them to an appendix) and unsupported speculations while including all figures with stated uncertainties

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Response: The paper has undergone a major rewrite based on the suggestions from all 3 reviewers. A significant fraction of the technological details have been placed in appendices, as suggested here.

Comment B: The paper identifies important uncertainties and/or corrections needed to interpret AMS data quantitatively. However, in the data interpretation about Mexico City, there is absolutely no attempt to show how those uncertainties affect the results, and there is no attempt to address the way in which the uncertainties will impact the speculations that are drawn about work by other groups. It is almost as if the authors have ignored the findings of the first half of their work in presenting the conclusions of the second half.

Response: The reviewer raises an important issue, namely, given the results obtained with the LS-ToF-AMS, what are the broader implications for quantitative analysis with the Aerodyne AMS instrument in general? We have re-structured Section 3 of the revised manuscript to more clearly identify the implications of the single particle results on standard AMS measurements. We emphasize that the purpose of the current manuscript is not to definitively discuss ensemble average mass based collection efficiencies of the AMS. Instead, the goal is to illustrate that the LS-ToF-AMS can provide useful information about the mixing state and processing of ambient aerosol particles.

The reviewer also points out that the first half of the ACPD manuscript discusses important details about the collection efficiency and chemical detection of the AMS, while the second half of the manuscript largely ignores these findings. We agree with the reviewer that we have not presented the full scope of the issues entailed in the use of the AMS in the second half of the manuscript. Accordingly, the revised version of the manuscript is restructured so that all assumptions and operating constraints of the instrument's measurement are clearly stated in the second half of the manuscript.

Comment C: 21316, Lines 22-28: Typically a useful literature discussion states the relevant findings from the work, rather than simply providing a list of papers. The

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former can be both more substantive and relevant, even at the expense of fewer (more important) citations. 21317, lines 1-2: same comment.

Response: We agree with the reviewer on this point and have accordingly limited the number of references to those that are essential. For these references we have made an effort to summarized the relevance of the references used.

Comment D: 21317 line 2: "we do not use double parentheses" should be omitted

Response: The manuscript has been accordingly modified.

Comment E: 21321, lines 1-9: This is one example of a digression into a technical 'mine is better than yours' fight, which is certainly not enhanced by the personal reference to Moffett; as opposed to the relevant multi-authored publication ("Moffett et al., 200x"). This tone is inappropriate, and Referee 2 has already (rightly) taken issue with it. I'd prefer to avoid such discussions in the absence of an actual side-by-side comparison of each instrument's strengths and weaknesses (which has not been done here; but would make a much better paper). Let's try to be gentlemen and save these assertions for a fair fight. An attitude that strives to identify all of the useful information in complementary techniques is much more likely to provide a lasting advance in the field.

Response: We agree with the reviewer and have removed section 1.1 from the paper.

Comment F: 21328: is the collection efficiency found here consistent with 0.5? or not? ,What are the error bars on this measurement? Is it +/-1% as implied?, Everywhere, or just at T0 in April?

Response: In the present study, the standard ensemble AMS measurement for non-refractory, mass-based collection efficiency was found to be ˜ 0.5 by comparing the results with SMPS and black carbon measurements obtained at the T1 site. A certain amount of confusion can arise when one discusses the ensemble mass-based collection efficiency and count-based CE of the instrument. Accordingly, we have re-

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structured the discussion of mass and count-based CE in Section 3 of the revised manuscript.

Comment G: 21342: While the absolute magnitude of biomass burning sources may not be adequately represented in the current data set, our single particle measurements indicate that biomass burning was a relatively minor source for particulate matter at T1 during the sampling period under discussion. WHY?

Response: Our initial remark about the magnitude of the biomass burning aerosol particles was based on the low number of detected single particles with mass spectral signatures containing levoglucosan. A more careful examination of this issue leads us to conclude that we may have over-interpreted this result. Consequently we have modified the revised manuscript to include a description of the limitations of the single particle detection of BBOA signatures.

Comment H: 21346: This page sounds like a proposal not a paper. How is it relevant to your results? Or omit.

Response: We have rewritten this section of the paper to more clearly identify the relationship of our results to previous AMS observations and single particle measurements made by Moffet et al. 2008 at T0 with the ATOFMS. The relevance of the content of p. 21346 in the original ACPD manuscript is that it describes the LS-ToF-AMS detection of pure HOA single particles that originate from local primary combustion sources. We believe that this is one of the key strengths of the single particle method being featured in the manuscript. We have removed the portion of p. 21346 in the ACPD manuscript that discussed the atmospheric implications of aerosol particles containing black carbon as this section does sound more like a proposal than a paper.

Comment I: 21348, line 1-2: what implies what? The evidence of a trend should be presented as a scatter plot with a quantified value of correlation.

Response: We agree with the reviewer that the sentence as written is vague and in-

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conclusive and we have removed it from the revised manuscript. In accord with the comment, we have also modified Figure 11, omitting the upper panel.

Comment J: 21354-5: The assertion about Pb's lesser importance seems difficult in the absence of evidence that Pb particles don't result in null counts. That basic problem negates the relevance of the next 2 pages of discussion

Response: We agree with the reviewer, we have over-stated our observations of a low occurrence of Pb signal in the single particles measured with the LS-ToF-AMS. Due to uncertainties in the ionization efficiency of the AMS instrument to heavy metals such as Pb, we have rewritten the section on Pb to more clearly state these uncertainties.

Comment K: 21356-7: The bullet point format seems inappropriate for a formal journal publication.

Response: We have removed the bullet format of the conclusions and rewritten the conclusion in accord with the restructured format of the revised manuscript.

Comment L: Fig. 11 is missing. Fig. 10 is difficult to interpret and has a number of overlapping lines/labels.

Response: We don't know why Figure 11 is missing from the reviewer's version of the manuscript. It is in the original version of the ACPD manuscript and downloads without any problem. We have eliminated overlapping labels in Fig. 10 to ensure a more clear presentation of the data.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 21313, 2008.