

Interactive comment on “Characterization of ambient aerosols in Mexico City during the MCMA-2003 campaign with Aerosol Mass Spectrometry – Part I: quantification, shape-related collection efficiency, and comparison with collocated instruments” by D. Salcedo et al.

Anonymous Referee #3

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This is a well written manuscript in which aerosol chemical composition data from the MCMA-2003 (Mexico City) field campaign is presented. Specifically, two AMS instruments were compared to each other and to BC, soil, and PM_{2.5} measurements.

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I have one very major concern about this manuscript. I notice that this, described as Part I of a series, appears to actually be the 'Experimental' section of the subsequent papers. It is not a stand alone scientific manuscript. The paper could be considerable shorter but contains an extensive review of the AMS literature instead of appropriate use of references. After reading Part II of the series, found now in ACPD, I note that manuscript is rather short and relies heavily on Part I. The companion paper describes only the measurements of one of the two AMS instruments. This leads me to believe another paper is forthcoming from the other field site (a mobile lab). Furthermore, I notice from the footnotes that this group of authors have at least two other papers in preparation from the MCMA-2003 study.

I request a clarification from the authors in their response as to the need for so many publications from this mission instead of a single comprehensive work. This manuscript deals largely with field performance of the AMS and shows minimal differences from previously published studies (e.g., inlet performance and sensitivity). Both Part I and Part II have conclusion exactly a single paragraph in length. Further, the two papers 'in preparation' in the footnotes of Part II are studies of organic and inorganic components - both of which are already discussed, albeit slightly, in Parts I and Part II. One of these papers is said to describe combustion emission in MCMA which is introduced, but left as a hanging paragraph, in Part II. Without a clarification from the authors this appears to be a case of repeat publication. I encourage the ACPD editor to suggest the authors condense these five papers into a single entity.

Comments on Part I This paper, as written, is very clear. There are, however, several important points that should be addressed, 1) A central topic of this paper is the first time intercomparison of two co-located AMS instruments. The quantitative difference between the two instruments for several species is presented in section 3.2. These values are a central part of this paper and should appear in the abstract or conclusions or both. The authors suggest that the differences are due to different inlets on the instruments. Can the authors unambiguously show the inlets were solely responsible

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for the difference? Is the implication the AMS measurements are exactly equal and all errors due to the inlets? Why, if an intercomparison was planned, were two different inlets used? It seems counterintuitive that the authors would plan an intercomparison with a systematic error that they do not quantify and go on to use this difference to explain the observational difference. 2) Sections 2.4.2 and 2.4.3 appear to be almost entirely reviews of previous AMS literature. The values found for size and bounce for MCMA, 1 and 0.5, appear to be essentially what is suggested in the literature. These values (and the uncertainty of +30 -10 %) should be moved to the abstract as, like the intercomparison values, they are of central importance. Could not a single sentence in the abstract say 'the size and bounce terms were found to have values of 1 and 0.5, as suggested in [reference]'? Why the need for several pages of reiteration of previous work? 3) Please eliminate the use of subjective terms throughout the paper. For example the two AMSs are said to agree 'relatively well' in the Introduction and the BC+AMS+soil is a 'good approximation' for PM 2.5. In Section 2.4.1 transmission differences are said to be 'most likely due' to alignment. These should be replaced by the values in the text so as to let the reader decide about the level of agreement. 4) In the Introduction, line 24 page 4146 'does not measure non-refractory' should be 'does not measure refractory'. 5) With respect to statements in the Experimental section: Are there high volatility salts that the AMS does not measure? Are all volatile components on mixed refractory/non-refractory particles analyzed? For example highly irregular soot with an organic coating? 6) In the Results a reference is made to the companion paper supporting conclusions in this work (Line 22 page 4159). What isn't that data in this paper if it is used to support the conclusions? 7) Page 4160 appears to again be largely a review of the literature. Why are all these statements reiterated when they are previously published? 8) At the end of page 4164, continued 4165, a comment is made that LASAIR saturation may be a reason for discrepancies. What is the saturation point of the LASAIR? What is the agreement below this point? Again, as with the point made about a known systematic error of using two different AMS inlets why would the authors allow this systematic error and then use it to explain the differences? An

attempt should be made to quantify it. Is the reader to believe the only error is the LASAIR undercounting? 9) In the Conclusion: please replace statements such as ‘‘the two AMSs compared well’’ with the actual comparison values. The attribution of errors to the inlet difference here should be replaced by an attempt to quantify this difference instead of the insinuation that this is responsible for all errors. Likewise, please give the value for the difference of PM_{2.5} with the BC+AMS+soil.

Comments on Part II I notice that several of the most interesting topics are only touched upon (normally in a single paragraph). They should be expanded: 1) Paragraph 2 of section 1.2 should include additional detail of the methods used to characterize aerosol composition during previous field studies in Mexico City. Without this detail it is difficult for the reader to understand the difference between previous missions and this one. 2) One would assume there was fairly extensive intrusion of biomass burning aerosol during this field mission. This is touched upon in the paragraph between pages 4193 and 4194 by referencing an intrusion of biomass plumes from satellite data. This section should be expanded. How are biomass aerosols determined using the AMS and/or black carbon or soot measurements? Are there clear correlations? Is it possible to tell from the aerosol loading how much of the aerosol mass on these days is from biomass aerosol? What is the size distribution? 3) Another section of interest is described in only another single paragraph (line 15 on page 4192): What is the relation of aerosol to traffic patterns? Is it possible to offer a more comprehensive correlation of this effect? The authors appear to have at least a rough understanding of auto emissions. Can this be correlated in the aerosol signal beyond the phrases ‘‘more noticeable.’’ and ‘‘a considerable reduction was observed’’? 4) The next paragraph is used to relate meteorology, specifically a ‘cold surge’, to aerosol properties. A reference is made to a paper by Foy et al. [which is not in the reference list ??]. Are AMS data presented in this paper? If not is there an attempt to perform a meteorology correlation in one of the ‘in preparation’ papers? This would seem to be a central topic of this field mission. 5) A single paragraph is used to describe an observation of what is assumed to be ammonium chloride (last paragraph of page 4197). What is the formation mechanism

of these particles? Although it is a semi-volatile species what does it condense on? How was it produced? Is there lab data to support this finding? It appears the identification of ammonium chloride is dependant on a correlation by AMS size. Were single particle instruments present? Do they support this assertion? 6) The conclusion are a single paragraph in length. Is there a reason some of these important findings are not reiterated?

In conclusion, please consider the following: While these are well written papers the topics of interest to atmospheric science are all dealt with in single, short paragraphs. In part this appears due to the fact that many of the observations are repeated from previous work while others appear to be dealt with in more detail in the 'in preparation' papers. This appears to be repeat publication and it should be avoided. The topics which have been repeated should be eliminated and replaced by reference and those which are intended for later publication should be expanded. This is of obvious concern using the authors' own words: 'An overview is presented in Part II of this series, with more detailed analysis to follow in future papers.' Can the authors explain why this 'detailed analysis' is not done here?

As the number of AMS instruments and field campaigns increase the problem of presenting interesting new science will grow. I can not stress enough that two identical instruments located in the same city does not warrant 5+ peer reviewed papers. Likewise, repeated observations between laboratory and field sites are not reason for multiple publications if the measurements are essentially the same. With a lack of important science these papers begin to read like a report of aerosol chemical composition as a function of time which more correctly should be an archived data set, like temperature or winds. The second manuscript appears to contain some interesting science but it is not fully developed.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4143, 2005.

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