**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Elemental analysis of chamber organic aerosol using an aerodyne high-resolution aerosol mass spectrometer" *by* P. S. Chhabra et al.

Anonymous Referee #2

Received and published: 8 February 2010

In this work, Chhabra et al. present HR-ToF-AMS analysis of several types of SOA particles generated in laboratory chamber experiments. They then make comparisons to PMF components commonly identified in ambient datasets. These studies are novel, the experiments cover a wide range of SOA precursor VOCs likely to contribute to SOA formation, and the data are useful for interpreting field measurements and SOA formation mechanisms. The paper is well written, the experiments appear to be carefully conducted, and the data analyzed carefully. The paper is appropriate for the journal and should be published with some minor corrections.

General Comments: I have two general suggestions for improvements to the paper.

The strength of the paper is the quantitative measurement of the elemental ratios of the SOA particles produced from a wide range of VOC precursors. Therefore, the issue of measurement uncertainty is of crucial importance for this paper. Indeed, the authors recognize this by devoting a section to possible sources of error. However, this section is purely qualitative and the only quantitative error information is relegated to the supplementary information. The authors have a large data set backed by independent measurements of elemental ratios and it would be good to see them comment on the errors presented in the original Aiken papers. Related to the issue of error, I have several comments and questions:

1) The elemental analysis figures would benefit from putting a single representative error bar on the elemental ratios so the reader can understand the magnitude of the error.

2) Lines 8-9 of the abstract and page 27510. Looking at the supplementary material I come to a different conclusion from that stated by the authors. In particular, in the abstract they state that m/z 44 is a good measure of SOA oxygenation except for glyoxal. O/C 44 and O/C HR are clearly different for the glyoxal system. However, they also appear to differ significantly and outside of error (assuming 30%) for the low NOx isoprene experiments (S3), the xylene and toluene experiments (S5), and at least one of the naphthalene experiments. The authors should revise their statement. In many cases, the O/C 44 is half the O/C HR. Error bars on the measurements would help this discussion.

3) P 27496, line 5: The 31% uncertainty is probably accurate only when the aerosol is chemically diverse. In particular, the calibration factor and hence the error from using the Aiken calibration factor are may deviate significantly for single species aerosol. Given the glyoxal spectrum is so distinct from other species, it is highly possible the Aiken calibration may not represent glyoxal SOA. Have the authors attempted to calibrate with pure glyoxal or a similar system? Glyoxal was not included in the original calibration samples in Aiken.

4) Is the N content of the aerosol in the glyoxal experiments above the LOD? Given the difficulty of separating CHNO and CHN fragments from CH and CHO fragments, is the 22% uncertainty presented in the supplementary material representative of the true error? It would be good to provide a raw HR spectrum in the supplementary material showing what ions are responsible for the N.

The elemental figures are presented with only a time axis. In these experiments, both loading and exposure to oxidants are changing in time. Given that the particle chemical composition changes both with loading and with chemical processing, the figures can be difficult to interpret. Adding loading and/or exposure as a second horizontal axis would make interpreting the figures much easier. In addition, in some figures organic mass (4,7) is plotted while in others organic volume (13) is plotted. I suggest plotting all figures as a function of organic mass loading for consistency.

Specific/Technical comment, corrections, and questions.

P27489, line 6: The statement about the O/C ratio of LV-OOA is somewhat confusing. Earlier in the paper you state the O/C can be as high as 0.9. I suggest revising the sentence to 0.6 and above for clarity, assuming that is your meaning.

P27490, line 7. Typo in the units.

P27490 Second paragraph: Please state the physical state of the AS aerosol. At 60% it could exist as a metastable solution or as a crystalline solid.

P27500, line 14/Figure 6. The green points have a significantly higher degree of scatter. Is this due to the loading? In general, the green points make it difficult to interpret the values on the figure. It may be helpful to exclude this experiment from the paper.

P27500, line 10. Can you provide the observed signal split between the ions?

P27501, line 4: The authors rule out one possible source of discrepancy between their filter samples and the on-line measurements. Do they have a positive hypothesis for the difference?

P27503, line 2. Incomplete sentence.

P27509, line 2: Incomplete sentence.

P27509, line 5: The data can be corrected for particle wall loss. If the data are wall-loss corrected, do the authors come to the same conclusion?

Figures 2, 5,9,10, 14. It is very difficult to distinguish the N from C in the spectra. I suggest using a different color for one or the other.

Figure 8: Combine the legends into one.