

## ***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 #3**

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The manuscript by Chhabra et al. presents an extremely useful and comprehensive compilation of HR-ToF-AMS elemental analysis results from smog chamber studies. The manuscript presents O/C, H/C, and OM/OC measurements for SOA produced from model biogenic and anthropogenic VOC systems. These ratios are compared with measurements by other on-line and off-line techniques. The smog chamber results are also compared with PMF components obtained from HR-ToF-AMS measurements in ambient environments. Finally, the empirical relationship between the fraction of organic signal at  $m/z$  44 and O/C is also examined for all of the SOA. This work is new and will be an important reference for elemental analysis of SOA. The material discussed in this manuscript is well suited for this journal. The manuscript is clear and

C10760

well written and I recommend publication after the following comments are addressed.

General Comments

1) Many of the figures show changing elemental ratios as a function of irradiation time. The authors clearly state that the elemental ratios in smog chamber experiments are a function of particle mass in addition to level of oxidation. So, it would be a good reference for the reader if the trends in organic mass loading with irradiation time were also presented as an additional panel in those figures.

2) Table 3 shows a nice comparison of O/C ratios observed for various laboratory systems. The range of ratios observed in this work is presented. However, for some of the other measurements only single values are entered and it is not clear what the uncertainties on these values are and whether these single values represent an average, lowest, or highest value for the ratio observed during the cited experiment. Since the degree of photochemical aging and organic mass in the cited experiments is not discussed or shown in Table 3, it is hard to know how to interpret differences that are observed for the same system. Particularly obvious in this context is the comparisons between the Aiken et al. (2008) O/C values and those presented in this work. The authors should provide some information either in the table or in the discussion to help the reader understand how to compare the numbers and what the differences may be due to.

3) In Table 3 and Figure 16 only O/C values are compared. Why not add in comparisons to H/C as well as OM/OC? Isoprene and Naphthalene have similar O/C values, but very different H/C values. So, understanding how the H/C values of the chamber SOA compare with ambient components will also be important. The addition of OM/OC values will allow for comparisons with other traditional filter based OM/OC methods as well. OM/OC measurements for the same SOA systems studied in this work have been published in other manuscripts (see some references below). Comparisons with those results would be useful.

C10761

Kleindienst et al., Estimates of the contributions of biogenic and anthropogenic hydrocarbons to secondary organic aerosol at a southeastern US location, *Atmospheric Environment* 41 (2007) 8288–8300

Kleindienst, et al. The Formation of Secondary Organic Aerosol from the isoprene + OH Reaction in the Absence of NO<sub>x</sub>, *Atmos. Chem. Phys.*, 9, 6541-6558, 2009.

4) The statement in the abstract that “m/z 44 is generally a good measure of SOA oxygenation for all systems except for glyoxal uptake” should be reworded to more explicitly mention the details of the observed variability in the O/C estimation for the various species. While glyoxal definitely has the worst agreement, O/C estimates for the aromatic systems agree within 50% of the measured values. Also, in some of the systems (i.e. isoprene low-Nox and Naphthalene high-Nox) the agreement between the f44 based prediction of O/C and the actual measured values gets better with irradiation time for some systems. For the isoprene low-Nox case, as described by Ng et al. (*ACPD*, 9.27745-22789,2009) and references therein, the f44 can have significant contributions from C<sub>2</sub>H<sub>4</sub>O. If this contribution was removed using the information in the HR data, how would that affect the O/C estimate?

#### Specific Comments

P 27488, Line 2. The O/C range should be .06-.1 instead of .06-.01.

P 27494, Line 4. I suggest getting rid of the first part of the sentence “ To distinguish between the O/C ratios determined from V- and W-mode”. This is awkward as it suggests that the O/C ratios were explicitly determined from the HR data in V-mode and compared to that from W-mode.

P 27494, Line 22. Can you be more explicit about what you mean by contributions from H<sub>2</sub>O being estimated? You could do this by including H<sub>2</sub>O/CO<sub>2</sub> fragmentation table values in Table S2.

P 27500, Line 25. How much of the mass do the glyceric acids and their corresponding

C10762

esters account for? It is useful to know this at this point to put the AMS measurements in context.

Fig. 1. Why is the N/C ratio in the low- NO<sub>x</sub> cases not closer to zero, particularly at low uptake times ?

Fig 5b. Two different m/zs are labeled with the same formula (C<sub>3</sub>H<sub>6</sub>O<sup>+</sup>)

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 27485, 2009.

C10763