

## ***Interactive comment on “Organic aerosol components observed in worldwide datasets from aerosol mass spectrometry” by N. L. Ng et al.***

### **Anonymous Referee #1**

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The paper addresses the classification of organic aerosol compounds in the atmosphere as inferred from aerosol mass spectrometry measurements. The influence of atmospheric aging on organic aerosol is analyzed by comparison of field and laboratory data. These are relevant scientific questions that are well in the scope of ACP.

The paper compiles a large amount of data sets the majority of which has already been published before (e.g., Zhang et al., GRL, 2007; Jimenez et al., Science, 2009; Lanz et al., ACPD 2009). While the authors give proper credit, it is not so easy to understand what the new and original contribution of this manuscript is. Here the authors should be more clear in the abstract and introduction.

To my understanding the new concept of this paper is the introduction of f43 (the ratio of m/z 43 to total OA), and to combine f44 and f43 as a diagnostic for atmospheric

aging, as in Figures 4, 5, 6, 7, and 8. This approach is new and it is shown here that the atmospheric organic aerosol compounds can be characterized very well (with respect to atmospheric aging, oxidation and volatility) by their position in the f44 - f43 space, or, as in Fig 5, in f44 - 44/43 space.

An additional very important point is the comparison of the field data with laboratory data from smog chamber studies (again, in the majority previously published data). This aspect is not mentioned at all in the title and in the abstract only with one (last) sentence. I would suggest giving more emphasis to the laboratory data in the abstract and introduction.

Summarizing: I recommend publication after the following comments have been considered:

### Major comments:

Change the title, for example: "Organic aerosol components inferred from aerosol mass spectrometry: Worldwide datasets and laboratory data"

Introduction, page 27749: Lines 24 to end: What is new in this study as opposed to Zhang 2007, Jimenez 2009 and Lanz 2009? This point has to be made very clear.

Skip Figure 1. There are been many papers showing mass spectra of HOA, LV-OOA (prev. OOA-1), and SV-OOA (prev. OOA-2), as Ulbrich et al., ACP, 2009, or Lanz et al., ACP, 2007.

Combine Figures 6, 7, and 8. It is not necessary to show all individual points and all individual compounds and experiments. It would be sufficient to indicate the areas of the graph where the data points are.

### Specific comments:

#### *Introduction*

Page 27749, lines 11-23: The direct measurement of the AMS is degree of oxidation,

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not volatility. Wouldn't it be more straightforward to call the subcomponents HO-OA (highly oxidized OA) and MO-OA (medium oxidized OA) instead of using volatility that is a kind of "secondary" information?

Page 27750, line 25: ME-2? If you mention it, you should briefly explain the difference to PMF. If it is not important for this data set, don't mention it.

### *Method*

Are the PMF results taken from the cited papers or have the datasets been newly analyzed by PMF, and if so (as I understand), why? Are there marked differences to the published results?

### *Results*

Page 27752: "the Pittsburgh data set". References are given, but please add the basic information (project name, year, season, location...) here. However, my suggestion above was to skip Figure 1 anyway.

Page 27753, lines 11-18: This discussion underlines the questionability of the naming convention LV-OOA and SV-OOA. The measured value is oxidation (O:C ratio or f44)

27754, line 22: Where is Chebogue Point (elevation, latitude, longitude, season and year of measurement)?

27755 lines 19 – 21. If Figure 1 is deleted, these two sentences can also be deleted.

27756 line 21 – end: The discussion on instrument-to-instrument variability is confusing. First, it is said "Some of the differences ... are likely due to instrument-to-instrument variability", later, the statement is " Thus, it is unlikely that this variability is enough...". Please be more precise.

27759, line 14, and Appendix: This is basic maths. I don't think the Appendix is necessary. Just giving the result in the text on page 27759 is sufficient. But please give in the caption of Figure 4 the slopes and intercept instead of coordinates for the two lines.

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Figure 5: According to the definition of the lines in Fig 4 and the transformed curves in Fig 5, the curves in Fig 5 approach a limit for  $f_{44}$  of about 0.03 (max  $f_{44}$  in Fig 4), corresponding to O:C = 1.2. Is there a further meaning? Does this value represent a atmospheric maximum oxidation state?

Fig 6: Some points are connected with lines, some aren't.

Fig 9: What the rationale in plotting only the highest  $f_{44}$ ? It is said that the reason is not to overcrowd the plot, but can't this procedure introduce a strong bias in the plot?

Page 27762, line 20: What is meant by "time-dependent" OOA data? I don't understand this explanation. If highest lab  $f_{44}$  values are chosen, wouldn't it be justified to use only the highest  $f_{44}$  from field data?

Fig 11: Plots a and b can be combined in one graph. You might consider reducing the number of points (increasing the averaging time)

Fig 4, 6, 7, 8: If you don't agree to combine the graphs 6, 7, and 8 into one graph, you should at least plot all 4 graphs on the same x-scale.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27745, 2009.

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