

Interactive comment on “Identification and quantification of organic aerosol from cooking and other sources in Barcelona using aerosol mass spectrometer data” by C. Mohr et al.

Anonymous Referee #1

Received and published: 1 December 2011

This paper covers the PMF analysis of HR-TOF-AMS data taken during the DUARE campaign in Barcelona, Spain. The factors identified are consistent with similar recent studies in urban environments and comprise of low- and semi-volatile oxygenated, biomass burning, hydrocarbon-like and cooking organic aerosols. Attempts are also made to extend the ‘poor person’s PMF’ (Aiken et al., 2009) to COA. Overall, this paper is very well written, clearly and concisely presented and relevant to the science. I would recommend that the paper be published in ACP after the following comments have been considered:

General comments:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



The discussion of the choice of solution set seems to be somewhat marginalised in the supplementary material. Given how much the later conclusions depend on the choice of the number of factors and p_{peak} value, it would be informative if the authors could include a précis of this process in the main text.

While the method for estimating the cooking fraction is potentially of much use (especially when interpreting ACSM data), experience has taught the AMS community that the ‘poor person’s PMF’ is not particularly robust between instruments and campaigns. It should be stressed in the text that it is possible (and indeed likely) that the derived coefficients presented will vary between datasets.

Throughout the manuscript, the authors use the R² statistic to compare the consistency between mass spectra. However, due to the nature of the mass spectral data, it is probably more appropriate to use an uncentred Pearson’s r (normalised dot product) instead. Note that R² is still appropriate for comparing time series.

Specific comments:

Page 27387: The final paragraph of the introduction should be restructured slightly. As it is currently worded, it would imply that the technical development of the COA estimation method is the principal focus of the paper, which I do not believe to be the case.

Page 27393: The authors refer to two factors as ‘LV-OOA’ and ‘SV-OOA’. While these are the commonly preferred terms, a caveat should be added that the AMS alone does not measure volatility. Rather, the factors identified can be related to low- and semi-volatile organics that have been measured elsewhere.

Page 27396: The method for calculating babs(880nm)traffic (and corresponding biomass burning product) should be briefly described.

Page 27397: Rather than remove spikes, why not average the AMS data down to the same time grid as the Aethalometer data? Better still, why not select the data corre-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

sponding to the times within the Aethalomter scanning cycle that correspond to the 880nm measurement?

Supplement, line 93: The reference spectra used in comparisons should be stated.

Figure S18: I would recommend that the authors try values lower than -1 for f_{peak} and try to identify the point at which the solutions begin to change.

Technical comments

Page 27390, line 6: The 1.107 and 1.108 are two different models of GRIMM dust monitor. The authors should check which instrument was used here.

Page 27390, line 15: The authors should give a typical relative humidity of the sample line if available.

Page 27390, line 23: References and/or web addresses should be given for the SQUIRREL and PIKA software.

Page 27392, line 21: Rather than 'a' and 'b', the authors should specify 'intercept' and 'slope'.

Page 27395, line 3: The C_nH_{2n-1} series does not necessarily come from alkenes and cycloalkanes as stated. Alkanes also contribute to these peaks.

Page 27396, line 12: The sentences "The time series babs(880 nm)traffic and the HOA time series show a similar trend. However, the R^2 value of 0.17 is too low to confirm correlation." Seem somewhat contradictory.

Figure 6: Correct 'Allan et al. 2009' to 'Allan et al. 2010'.

Figure S2: The parameters used to estimate PM1 from the GRIMM should be given.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27383, 2011.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

