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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wintertime aerosol chemical composition and source apportionment of the organic fraction in the metropolitan area of Paris" *by* M. Crippa et al.

Anonymous Referee #3

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This paper presents the AMS/PMF analysis from the winter MEGAPOLI campaign in Paris. This analysis has now become commonplace in urban intensive studies for the identification of primary sources in particular. The results are not particularly controversial, as they compare well with other datasets obtained in other studies in other cities. However, there is real novelty in how the authors have used the Aethalometer data and how they have compared the different measurement sites within the campaign. The end result is a largely well written paper that presents some useful, if not earth-shattering, statistics regarding primary aerosols for Paris. I would recommend that this be published in ACP subject to the following minor comments:

There is some crossover between this paper and Freutel et al., which I was asked to review in tandem with this. I refer specifically to the technical details such as the scaling of the organics by a factor of 1.3. But curiously, the authors do not refer to this paper, which is unfortunate because it covers details regarding the intercomparison in more detail. This paper would benefit from referring to this directly.

As a matter of taste, I do not see it necessary that equations 2-4 need repeating, as these have already been presented numerous times in the papers cited by this work.

Page 22543, line 16: 'Lee et al.' should be outside the parentheses.

Page 22544, line 20: I don't particularly like the explanation for the scaling factor of 1.3 applied to the data. Is it not more likely that this could be caused by something to do with the vaporiser (e.g. different temperature) rather than the ion transmission function of the mass spectrometer?

Page 22545, line 8: Rather than Weingartner et al. (2003), the authors should consider using the updated correction presented by Collaud Coen (2010, doi:10.5194/amt-3-457-2010).

Page 22547, line 23: How many variables were downweighted as 'weak'?

Page 22549, line 14: The use of a solution set with a nonzero fpeak for LHVP must be justified further. According to Paatero et al. (2002, doi:10.1016/S0169-7439(01)00200-3), while fpeak can be used to explore the amount of rotational freedom within a potential solution space, solutions associated with nonzero values can only be considered physically meaningful for certain systems. Unless there is a specific reason to prefer the fpeak=-0.1 solution, it would be safer to use the fpeak=0 version.

Page 22551, line 9: Replace 'pretty good' with something less informal.

Page 22557, line 9: Another explanation for the org60 could be that it reflects a portion of the BBOA that is prevalent during a particular burn phase, hence the separation from the BBOA factor.

ACPD

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Interactive Comment

SI-6.3: The authors did not explore a particularly wide range of fpeak values. It would have been informative to have seen at what point the algorithm failed to converge, produced unacceptable profiles or suffered a large increase in Q.

Figure SI-6.4.1: The scaling on the Q/Qexp graph is not much use, as it doesn't show how much variation there is in the red line.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22535, 2012.