

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 #2

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This manuscript summarizes wintertime observations of aerosol composition in downtown Paris and two suburban areas. Factor analysis has been performed on the aerosol mass spectrometer data to assess contribution of different sources to the submicron aerosol mass at these sites. The results are sound and consistent with the summertime measurements, indicating a significant regional contribution to the aerosol mass in the area. However, there are few points that need clarification or a better explanation. I suggest publishing this work after authors respond to the following comments:

Technical comments: 1. Regarding the CE in AMS: for the data obtained at the LHVP site a CE=0.4 applied at all times. This is lower than what is typically applied for ambient

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data and it contradicts the data shown in figure SI-2e. Based on the SI data, there's definitely a NO₃-effect apparent in the dataset, so applying a constant, low CE factor appears to not be justified. If the DMPS data are valid, authors should revisit the CE determination. Note also that the explanation that's given in terms of the higher density particles affecting AMS CE doesn't apply here- with higher densities, particles appear larger in the vacuum aerodynamic sense and it's more likely that they fall out of the 'good' transmission region of the AMS lens; in such case AMS mass decreases compared to other instruments whereas the data in SI-2e at high NO₃ fraction times indicate that the apparent AMS mass is actually higher compared to the other times. In general, I believe now that parameterization of composition dependent CE is available, it makes sense to apply such parameterizations and then evaluate the closure between estimated AMS mass and other measurements; if this is not done in this order, the estimated CE appears to be more of a 'fudge' factor to make sure 'consistent' results (but may be low or high depending on errors in the other measurements) are obtained.

2. Discussion on angstrom exponent values for traffic and biomass burning BC is circular- in section 2.3.2 it's mentioned that the choice of the parameters is discussed in section 3.3 while in sec. 3.3 it's referred back to 2.3.2! It appears these values come directly from another study. Maybe the best is to explain a bit in Section 2.3.2 how these factors were determined. Related to the BC mass- the concentrations reported here are awfully high! Are there any wintertime direct BC mass measurements (e.g., by SP2) to confirm such high concentrations in Paris or could this be due to the choice of mass absorption efficiencies used to estimate BC mass? Also, in the apportionment of BC only biomass burning and traffic related BC are considered. Isn't cooking also a source of BC? Do we know how small this source is to justify the assumptions here?

3. P. 22551: Comparison of MAAP and aethalometer results in a slope of 1.16- is this considered a good closure? In this case, reasoning of instrument differences for different BC mass estimates is not valid; I suggest removing that sentence.

4. P. 22555: indicate what the correlation coefficients are when comparing PMF BBOA-

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related factors and filter-based levoglucosan measurements .

5. P. 22559: Sciare (2010) analysis indicated OM/OC =0.9- does this OM include only HOA or other PMF factors as well? If it included other factors then it's not surprising that HOA/BC(tr) is lower than 0.9. However some explanation is needed as to why compared to Chirico (2010) work a factor of two higher HOA/BC is obtained in this work.

6. Why isn't OOA compared to sulfate? Since NH₄ can be associated with SO₄ and NO₃ and so it can have a diurnal temperature-dependent variation, it's better to compare the regionally influenced OOA factor with SO₄.

7. Figure 5- in COA factor, m/z 43, 55, 57 seemed to have been mis-marked. Please double check.

8. Figure 9- please clarify 'avg' lines are the 'reference' lines.

9. Figure 11- These fitted lines shouldn't be forced to go through zero. I believe the non-zero intercepts actually are an indication for the extent that the 'factorization' doesn't work. For example, at SIRTA, the positive intercept of HOA may indicate that BC(tr) is underestimated and that is consistent by getting a positive intercept for BC(BB) when looking at BBOA vs. BC(BB).

10. Figure SI-2d: is there an explanation of why comparison of SO₄ and NO₃ between AMS and PILS is so off during the 'high' period while NH₄ looks ok? Is this good agreement for the wrong reason (wrong RIE(NH₄)).

11. Figure SI- 3a-b: again- you shouldn't force the lines to go through zero. There are enough data points close to (0,0) that the line should be fitted freely.

12. Figure SI-4: how is OC calculated from AMS measurements? There was no reference to what OM/OC was assumed; this of course will change the comparison shown here!

13. Figure SI-5: there was no reference to this plot in the main text. I suggest either

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adding a ref. to it or removing it.

14. Figure SI 6.6.2- what is the explanation for not having high f55/f57 at peaks of COA time series at SIRTAs?

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