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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancing non-refractory aerosol apportionment from an urban industrial site through receptor modelling of complete high time-resolution aerosol mass spectra" *by* M. L. McGuire et al.

Anonymous Referee #2

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General comments The paper titled "Enhancing non-refractory aerosol apportionment from an urban industrial site through receptor modeling of complete high time-resolution aerosol mass spectra" by McGuire et al. deals with the atmospheric aerosol source apportionment in Windsor (Ontario) making use of positive matrix factorization applied both to the organic and inorganic AMS mass spectra. 6 sources were separated using this approach, including factors related with inorganic components (like sulfate-OA, nitrate-OA and chloride), a HOA related with traffic, a site-specific component with amine peaks and an oxygenated OA fraction. The paper is well written

and describes detailed analysis especially concerning the comparison of PMF results obtained with and without the introduction of the inorganic fractions in the source apportionment matrix. This approach is still quite new although it has a lot of potentialities that should be systematically investigated in most of AMS-PMF analysis. Therefore I recommend the publication of McGuire et al. work after replying to some technical comments.

Specific and technical comments - When referring to the amine containing factor, the work of Hildebrandt et al. (2011) should be cited as well.

- There are many more papers than the one reported by the authors assessing the cooking source identification in urban areas. How were the selected references chosen? The authors should consider quoting also the work of Sun et al. (2011), Mohr et al. (2012), Crippa et al. (2013), etc.

- The discussion about the CE estimation should be extended reporting some graphs and evaluations (possibly in the supplementary material), although the choice of adopting a CE equal to 1 is in this case reasonable.

- Did the authors apply any downweighting procedure when combining together the inorganic and organic matrix in one PMF experiment? As for mz44 a downweighting procedure is required when running PMF because some organic peaks are calculated from mz44 (Allan et al., 2003; Ulbrich et al. 2009). The same applies to some SO4 peaks (e.g. mz 80-81-98 are calculated from the signal at mz48-64). Can the authors clarify their approach on this issue?

- At page 5093 (line 2), the authors discuss the Q/Qexp variation in the fpeak range -10,+10. Since this metric varies ca 1%, the authors are comparing mathematically equivalent solutions. They should investigate a broader fpeak range to possibly identify different solutions. Ulbrich et al. (2009) recommend analyzing an fpeak range corresponding to a Q/Qexp variation of at least 10%. This applies to all fpeak analysis reported in this paper.

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- Page 5112: the authors should report the R2 instead of R (although correlations coefficients are not very high). Can the authors additionally report the correlation of HOA with toluene?

- Table 2: F44 represents the fraction of mz44 to total org or the fraction of mz44 in a MS (see page 5015). Why is the ratio f44 to org reported in Table 2?

- It would be interesting to summarize with two pie charts/bar graphs the average source contribution to total OA (just taking into account the organic fraction from the coupled PMF) and to PM1 (from the coupled organic-inorganic apportionment). In this way it is easier to compare the PMF results obtained for this site with other literature works. These graphs represent complementary information to Table 2 and Fig.7. The authors should always report and discuss both the OA composition and the source contribution to PM1. A comparison of these results to analogous studies in the area in terms of organic fractions is needed (e.g. is HOA contributing to xxx% to total OA similarly to literature studies?).

- The authors discuss in the SI the impossibility to identify a BBOA factor. Do they expect this source to contribute to this site during wintertime? From the 5 factor solution of PMForg, the authors claim that the factor Other OA1 cannot be considered as a BBOA source because of the correlation of its time series with the one of Other OA2 factor (although the correlation is not very high as it appears from Fig. S-2.4). The authors should investigate the diurnal pattern of all sources in order to justify the interpretation of all the sources. If the authors expect a BBOA contribution, they could easily verify it looking at the diurnal pattern of this source.

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