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Comment

***Interactive comment on* “Spatial variation of chemical composition and sources of submicron aerosol in Zurich: factor analysis of mobile aerosol mass spectrometer data” by C. Mohr et al.**

Anonymous Referee #3

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The manuscript by Mohr et al. presents an interesting study of the aerosol composition in and around Zurich. On-road and stationary measurements were made to explore the spatial variation in the aerosol composition and factor analysis was used to identify distinct organic aerosol factors that could be correlated with traffic emission sources (HOA), wood burning and heating (WBOA), and oxygenated organic aerosol (OOA). The measurement and analysis methodologies used in this work are of interest and I recommend publication after the following questions are addressed:

General Comment:

1) Discussion of Background versus local contributions:

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- The authors' state that they are using "urban background" interchangeably with "regional background", but this is confusing. The measurements that are presented can be used to assess the spatial variability of chemical composition and "local" sources in and around Zurich above the urban background value in Zurich. However, I do not think that this can be automatically extended to assess the "regional background" which would require more measurements both north and south of the city in background rural sites. The PM10 comparisons made between Zurich Kaserne and Payerne and Tanikon are not necessarily reflective of the much smaller PM particles measured with the AMS.

- Top of page 12342, the authors state "it seems that even though the city of Zurich is a major emitter of air pollutants, the beforehand mentioned meteorological conditions and the high, well distributed emissions of the densely populated Swiss plateau area lead to a homogenization of background air being it urban or rural"

Are the authors trying to say that the "urban background" in Zurich is due to regional sources outside Zurich? It is not clear that the case for this has been made by the authors.

- Have the authors tried using CO as the inert dilution tracer in place of SO₄ in equation 4? If so, what were the results? If not, it would be useful to mention why not.

Specific Comments

1) Since a significant amount of the analyzed data was obtained with mobile on-road measurements, one would expect that there was significant contamination from single vehicles. It is not clear from the manuscript how the authors dealt with these contaminations in the data stream. Were the contamination datapoints removed before PMF, for example? This would be an important step to describe in the preparatory data analysis section 2.4.2, particularly since the Q-AMS would suffer from particle counting statistics as mentioned in line 8 on p. 12338.

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2) p. 12331, line 19. The authors mention that they use a CE of 0.85 based on comparisons with other instruments. What value would be chosen if the AMS composition was directly used to determine a CE based on the previous lab results of Matthew et al.? What CE was used for the HR-ToF-AMS?

3) p. 12332, line 20. Since this study did not use ME, which would allow fixing of mass spectral profiles, it is not clear what they authors mean by “forcing one factor (OOA) in the solution to have the mass spectral characteristics of the fragmentation table dependencies for m/z 44 was just sufficient a priori information to yield an atmospherically relevant PMF solution“?

4) Figure SI-9 and associated discussion. The reasoning for why the $F_{\text{peak}} = -0.1$ solution was chosen should be clarified. Was the choice based on getting m/z 60 fractions to be in a range that is representative of burning spectra? How did the correlation between the components and component tracers (i.e. CO, NO_x etc.) change with F_{peak} ?

5) Figure SI-12: Why are the scaled residuals for m/z s greater than 200 so small compared to the rest?

6) Figure 1: It would be useful to see the overview values and statistics for all the aerosol species as well (SO₄, NO₃, OOA, HOA, BC, etc.)

7) Figure 5: The f_{44} and f_{43} values measured for 14.12.2008-16.12.2008 do not seem to be well reproduced by the PMF components for part 2. Several of the measured points have $f_{43} < 0.05$, but none of the components have low enough f_{43} values to account for this. Are these low f_{43} points noisier (i.e. weighted less in PMF) than the others?

8) Figure 6: It is hard to get an overview of how the absolute loadings vary with the type of site (residential, urban etc..) from this figure. It would be useful if the authors added a bar chart showing the loading vs. site with the sites arranged according to

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classification type. Separation of the bars according to primary and secondary organic and inorganic contributions will also be useful..

9) Figure 10: It would be useful to see how NO_3 , OOA, and SO_4 vary in this figure as well. If I understand correctly from the discussion in the text, most of the BC appears to correlate with the vehicle emission source. So, why is the BC trend with ambient temperature and wind speed different from the HOA trend?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12323, 2011.

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