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Atmospheric Measurement Techniques Discussions

Interactive comment on "ACTRIS ACSM intercomparison – Part 2: Intercomparison of ME-2 organic source apportionment results from 15 individual, co-located aerosol mass spectrometers" *by* R. Fröhlich et al.

Anonymous Referee #2

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The manuscript describes the analysis of a data set with respect to source apportionment taken by 13 of the new Aerodyne ACSM monitors, which are supposed to monitor particle composition on ACTRICE sites. In addition an ACSM-TOF and a HR-TOF were simultaneously operated. The HR-TOF was used as guide to extent the statistical analysis, performed by PMF and ME-2. This is a very important study for methodology and quality assurance of aerosol monitoring.

The manuscript focus on the results achieved with PMF and variations, ME-2, and their

intercomparability, when applied to a data set from different instruments. Unfortunately the companion experimental paper was not available although the authors refer several times to it. Since the manuscript focus on methods of evaluation, this can be tolerated, but it would have been helpful to check for some boundary conditions.

The manuscript is published in AMTD, so, as mentioned before, the focus is not the result itself, i.e. the source apportionment, but the coherence of PMF and ME-2 analysis on a dataset monitored the same place at the same time with the set of instruments.

The manuscript is well written and interesting to read. Figures and Tables are ok and sufficient. The Figures are detailed and for that detail somewhat too small (if printed). Up to here, I have only minor points which will follow below. What is presented in the manuscript can be published in AMT after minor revisions.

However, before publishing the authors may want to consider the following major revision:

In my opinion the manuscript has a conceptual weakness. Instead of doing a careful analysis with ACSM data alone, push it to edge, and then compare with the HR-AMS data, the authors chose the opposite way and used the results of analysis of the HR-AMS data as a guideline to optimize performance of the analysis of the ACSM data. (The authors clearly state that procedure and this not my point. Although, even in the analysis of HR-AMS data already a lot of detailed work and know-how had to put in to clearly separate the four factors. And I asked myself in how far is this objective or already depending on the specific skills (or 'opinions' what to expect) by the operator.) I think the a posteriori analysis is not the real test for the ACSM source apportionment and the described results (even considering the careful discussion) do not really help the case. My question is, what can we expect if we have a single instrument at an ACTRICE site? Then Figure S6 is the truth and the question is what does this 3 factor result mean when compared to the more detailed HR-AMS results. Do I have a clear HOA and BBOA factor and the OOA factor is a mixture of OOA and COA? If I want

to refine the ACSM analysis, is there a general procedure (that is independent of the experience/'opinion' of the operator)? For example, can I consider always a certain set of anchor profiles and apply ME-2? Which anchor profiles for which site? If I use a profile related to a source which did not contribute in the considered period, will it be found to be unimportant? Will it change the analysis results?

What would happen to your comparison if you allow only 3 factors in PMF of HR-AMS ? I.e. skip e.g. the weak expressed COA factor?

Or do I need some HR-AMS analysis at each site? For how long - a few weeks, a full seasonal cycle? Of course these questions cannot be answered overall within this manuscript, but I urgently suggest to more discuss the consequences of Fig S6 in relation to the HR-AMS result and at least sketch generalized ways how to treat ACSM data in a self-consistent way - without input by HR-AMS results. The presented manuscript can be either shortened to make room for this addition, but can also stay as it is and the proposed aspect can be added on top of it.

Minor points:

p1564, l 4: "Overviews of wintertime. . .". This seems to be misplaced, it belongs to Site description.

p1570, l19: move definition of RIT from p1584, l22to here.

p1572, l14: This sentence sounds strange.

p1574, l27: move reference behind "setting".

p1575, l26: the word "classified as POA" seems to be strange, I would suggest "attributed to POA sources". Similar for "classified as SOA".

p 1578, l13: In the beginning the simple PMF is noted as unconstrained PMF, which is ok. From here on the notation "pure" PMF is used. I suggest a consistent use of "unconstrained" PMF, for the simple case.

p1578, l22: . . .subscript "Paris". . ., not subscript.

p1579, l12 & l24: I suggest to move those two paragraphs to the Method section, it feels misplaced here in such detail.

p1583, l10: high f44 in HOA of #13, is strange. But also #1 and #5 have significant contributions of f44 in the HOA profile, but do not have high f44 in general? By the way, the organization of the Figures S13 and S14 is unfortunate, as the same instruments are not appearing at the spot part in the figure. Move the explanations from the center of the figure to the beginning of each factor.

p1585, l15: Crippa et al.(2014) derived the HOA(Paris) profile from HR-AMS ? Why can it not applied in the HR analysis ?

p1586, l14: How can we be sure that the COA factor is real? If you would skip the COA factor in HR-AMS analysis would that unify the ACSM and HR-results? Compare to major concerns.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1559, 2015.