

# ***Interactive comment on “Chemical characterization and source apportionment of submicron aerosols measured in Senegal during the 2015 SHADOW campaign” by Laura-Hélène Rivellini et al.***

## **Anonymous Referee #3**

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### Overview:

This manuscript presented the highly-time resolved chemical characterization and source apportionment of atmospheric submicron aerosol particles (PM<sub>1</sub>) in West Africa, along with field on-line measurements (including an ACSM and a 7-wavelength aethalometer) and offline model analysis. The campaign was deployed under the environment affected by anthropogenic emissions (e.g., traffic, cooking, and biomass burning) and natural sources (e.g., desert dust and marine air masses), etc.. The results showed that the ten times lower average concentrations of NR-PM<sub>1</sub> were observed

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here compared to the results from other megacities with persistent air pollution issues, i.e., Beijing and Paris. Sea breeze phenomena and Saharan desert dust outbreaks may lead to pollution events with high concentrations of PM<sub>10</sub> (up to 900  $\mu\text{g}/\text{m}^3$ ). Organic matter (OM) and sulfate could dominate the major fraction of aerosol particles when air massed could be associated to different influences, i.e., continental and sea breeze and oceanic region related. The authors also estimated the mass concentrations of particulate Fe from the Aethalometer data, for which an average contribution (4.6%) of Fe to PM<sub>1</sub> was obtained. A new organic aerosol factor (LCOA), Local Combustion Organic Aerosol, was resolved by PMF analysis, with relatively low contribution (3%). Both regional and local photochemistry processes could contribute the formation of oxygenated organic aerosols in this area. The results seem to be interesting. The manuscript is well written and organized. I would recommend this paper could consider to be published in ACP once the following comments are addressed.

Specific comments:

1. Page 4, line 30: Should keep the same abbreviations for those species throughout the manuscript. For example, what's the different between "NO<sub>3</sub><sup>-</sup>" and "NO<sub>3</sub>" (Page 5 and line 5), and somewhere else "SO<sub>4</sub><sup>2-</sup>" and "SO<sub>4</sub>", "Cl<sup>-</sup>", "Cl" and "Chl", etc.. If they are different for the discussion in this manuscript, please the authors give the related text to explain them.
2. Page 5, line 3: It was interesting to perform the chloride calibration with ammonium chloride particles. Could the authors also present the related calibration results in supporting information, as showing in Fig. S1, since your RIEChl is much higher than the default value (1.3). In addition, did the authors try to validate chloride data based on your calibration results? It's also interesting to know how it works about the chloride calibration.
3. Page 7, line 32: Would it be possible that the authors could give uncertainties of estimated Fe concentrations with this method for your study?

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4. Page 8, line 19: In this paragraph, I suggest that the authors could consider to also mention some brief information about ME-2 algorithm how it works for constraining organic aerosol factors (Canonaco et al., 2013), since this will be helpful and easier for readers to quickly understand the SoFi how it works in this study.

5. Page 10, line 20: The authors should consider to explain which kind of combustion sources for the ACSM  $m/z$  57 tracer is. When I am reading here, I immediately realized that why the authors did not perform the source apportionment of black carbon by using the Aethalometer model (Sandradewi et al., 2008). As described in Section 2.1, there are both traffic and biomass-burning emissions that would potentially contribute the ambient black carbon burden at this sampling site. So, is it possible to identify black-carbon aerosols related to traffic and biomass-burning emissions here? This will be very helpful for the source apportionment of organic aerosol.

6. Page 11, line 3: Here is a little bit confusion about the ratio of Fe/PM<sub>10</sub>, since your Fe concentrations were estimated by the PM<sub>1</sub> aethalometer, but not PM<sub>10</sub> Fe. Is this the case? If yes, the authors could consider to add “PM<sub>1</sub>” in front of “Fe” when you discuss this ratio in the main text of the manuscript.

7. Page 11, lines 27-28: Why didn't the authors consider to give the contribution of chloride to total NR-PM<sub>1</sub>? I suggest the authors could also mention it.

8. Page 11, lines 32-34: Is there evidence to support this discussion? Otherwise, the authors should give related reference(s).

9. Page 14, Section 3.2.1: It would be more convinced about the discussion on geographical origins, if the authors would also combine with some modeling methods, e.g., potential source contribution function (PSCF) that might be easily performed on highly-time resolved data (Petit et al., 2017). In addition, it could be also interesting to perform PSCF on organic aerosol factors.

10. Page 15, lines 24-25: Should “C<sub>n</sub>H<sub>2n-1</sub>” and “C<sub>n</sub>H<sub>2n+1</sub>” be “C<sub>n</sub>H<sub>2n-1+</sub>” and

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“C<sub>n</sub>H<sub>2n+1</sub>”, respectively?

11. Page 15, line 13: Change “Source apportionment” to “Source apportionment of OM”.

12. Page 15, Section 3.2.2: The biggest question I have in this manuscript is about the PMF-OA solution.

1) Page 15, lines 15-17: Please the authors could also present the results of 3 – 10 factors from the PMF-free runs in supporting information.

2) About the LCOA factor related to local open waste-burning emissions, did the authors try to compare it with any tracers? For example, please try to do some correlation analysis between LCOA with f<sub>60</sub> (an AMS/ACSM biomass-burning tracer), f<sub>36</sub> (as mentioned by the authors), and black-carbon aerosols from different sources, etc.. At the same time, the authors did not find any reasonable BBOA profile instead of LCOA factor. Did the authors try to compare the LCOA profile with any BBOA related profiles? What's different from them? And did the authors find any similar such mass spectrum of LCOA during your unconstrained runs and/or constrained HOA and COA runs, respectively? I am thinking whether the LCOA factor is a kind of splitted factors due to constrained HOA and COA? Could the authors give those results from your 3-10 factor runs and make comparison with your LCOA profiles. Based on the results of Fig. S6, the constrained five-factor solution seems also good. Why didn't the authors choice this one for the finally PMF-factors solution? Did the authors also try to constrain all BBOA, COA, and HOA factors together to check the five-factor solution? And the authors could also try to just constrain BBOA and HOA factors to check the results. There is in factor that the BBOA factor/profile could be affected when HOA and COA factors were constrained together.

3) In addition, could the authors explain why the median mass concentrations of COA and HOA show relatively similar diurnal variations (Fig. 9), and the similarly high peaks of HOA, COA, and LCOA around 8 am? I suggest that the authors could also make

correlation analysis among HOA, COA, and LCOA each other in supporting information. In addition, the authors should also consider to perform the source apportionment of black carbon as mentioned above. It will be very useful to evaluate the PMF solution by checking the relationships between HOA, COA, and LCOA with black carbon from fossil fuel combustion and biomass burning, respectively.

4) Why did the authors select  $\alpha$ -value = 0.6 for both HOA and COA factors (Fig. 9)? I suggest that the authors could also perform the sensitivity test of  $\alpha$ -values (e.g., from 0 to 1, with  $\Delta\alpha = 0.1/0.05$ ) on HOA and COA factors for your data sets. And why did the authors apply the reference mass spectrum of COA from HR-AMS (Crippa et al., 2013) for ME-2 constraining runs, but not from the ACSMs (Fröhlich et al., 2015; Ng et al., 2011)? Please check “Ng et al., 2016” in the plot?

5) Page 18, lines 3-23: It would be also interesting to discuss the different types of OOA factor, i.e., LO-OOA and MO-OOA, as showing in Fig. S6. Why didn't the authors keep both them for OOA factors in the final PMF solution? The authors would consider to try to take a look at the relationship between OOA and Fe concentrations. This might make sense to find something new. ãĀĀ

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