

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 #2

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I recommend this manuscript for publication in ACP.

The manuscript presents interesting results on the submicron aerosol in Senegal. The datasets are new, the data analysis is very complete and thorough, and the manuscript is well structured and well written. There is one main issue to be addressed (first one listed below) and some additional comments to be addressed before the work can be published in ACP.

1. Please consider revising the calculated Fe concentrations and/or the PM1 concentrations and/or the ACSM concentrations (RF for NO₃). These data together have

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inconsistencies that need to be addressed or commented and the possible sources of uncertainty should be stated. The authors report a mean value of 4.6% for the Fe/PM1 ratio. This means 16% for the Fe/RefractoryPM1, since 71% of PM1 is NR-PM1. (Or 20% for the ratio Fe/(PM1-ACSM-BC) according to Figure S2, considering 5% of Fe and 25% of PM1-ACSM-BC). Hence, it is this 16% (or 20%) which should be compared to the data in the literature, given that the literature data that the authors quote make reference to Dust (and not total PM, regardless of the size fraction). Please see some additional comments related to this one below.

2. The sampling period is actually 3 months (20 March to 22 June), and not 4 as stated.

3. Please homogenize the dry and wet period definition: in the introduction it says that the dry season extends from November to May; in section 2.1 it says that the dry season is from December to March; in section 2.3.1 it says that dry season is generally defined from November to April; in the same section 2.3.1 it is coherent within the section and it says that “Our study taking place from March to June allowed for the observation of both the late part of the dry season (March-April) and the beginning of the wet season (May-June)”; in the conclusions section it says “during four months of the 2015 dry season”. Given that precipitation data for the specific campaign is available, according to section 2.2.3, could you please provide this data, or make the classification based on these data? (Although the info in literature about the usual dry-wet periods can still be included).

4. Section 2.2.1. Please confirm that no major changes (such as filament replacement) occurred to the ACSM during the time between Feb 2014 and Jan 2015, for which the calibration constants were taken. This is necessary to be able to take the average of all calibrations. Especially between Dec 2014 and Jan 2015 there is a big difference for the RF of NO₃ (Fig S1). This is a source of uncertainty that should be acknowledged in the manuscript, especially considering that the absolute concentrations are used and that the differences with the bulk PM1 concentrations are taken as very valid and interpreted.

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5. Section 2.3.1. "During IOP-1 two main prevailing directions were found (Fig. 2a). The first one [...] and North-West to South-West (315-225°, dominant in May-June)" Maybe you could indicate which wind direction prevails for the first and second periods (Mar-Apr and May-Jun), since the wind directions for the periods are given based on literature info, but the data for the specific campaign is available and then the wind roses are commented for the entire period.

6. Section 2.3.1. "In summary, among the 91 days of IOP-1, 19% were classified as continental days, 32% as sea breeze days and 49% as marine days". You could say these percentages for the dry and wet periods? Or the rain for each of the 3 types of days? Somehow the info of the day types and the info on rain (dry-wet) should be linked. This is related to Fig 2 as well.

7. Section 3.1.1. "the fraction of unaccounted material therefore corresponded to DD and SS contributions". Note that if Fe is 2-5% of DD, then according to Fe concentrations, DD > unaccounted mass.

8. Section 3.1.1. "The unaccounted fraction (determined as the difference between the gravimetrically measured PM1 mass concentration and the sum of chemical species from ACSM and aethalometer measurements) corresponds to 27%, 26% and 16% of the PM1 mass for continental, sea breeze and marine days, respectively (see Figure S2)". Please specify that for these numbers you already applied the model from Fialho for this calculation, so that you derived already BC and Fe concentrations from the aethalometer measurements.

9. Section 3.1.1. Related with comment 4, please comment on the uncertainty of ACSM measurements since you took RF of NO₃ as an average of previous calibrations and not determined on site.

10. Section 3.1.2. "Although a weak correlation ($r = 0.55$) was found between Fe and total PM1 concentrations, Fe concentrations showed higher correlations with PM10 ($r = 0.70$, see Figure 4)". You could check the correlation between Fe and PM1-ACSM-BC.

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11. Section 3.1.2. "the highest Fe concentrations ($> 8.0 \mu\text{g m}^{-3}$) are generally associated with continental and sea breeze days. These maxima also coincide with PM10 highest concentrations ($> 400 \mu\text{g m}^{-3}$)". Note that $8 \mu\text{g m}^{-3}$ of Fe corresponds to $160 \mu\text{g m}^{-3}$ of DD (if Fe is 5% of DD on average according to literature values). Even if we assume a high percentage of refractory PM1 for this data point (higher than the average 29% according to data in page 9, line 24), let's estimate 60% of PM1 is refractory, this would mean that the PM1 is $267 \mu\text{g m}^{-3}$. If PM10 is $400 \mu\text{g m}^{-3}$, then the ratio PM1/PM10 for this event would be 67%, much higher than the average 10% reported. Is this the case? Please check for consistency. Either Fe is overestimated, or PM1 is underestimated, or both.

12. Section 3.1.2. Last paragraph. "Fe contributions to PM10 estimated in M'Bour (average Fe/PM10 ratio of 0.51% over IOP-1 and 0.89% for continental days)". The Fe concentrations determined in this study correspond to PM1, since the aethalometer was equipped with a PM1 inlet. If this is correct, then the authors are taking the Fe in PM1 with respect to bulk PM10, whereas the Fe concentration in PM10 corresponding to the Fe concentrations in PM1 determined in the present study would be much higher. Hence the comparisons with the ratios of Fe/DD or Fe/soil in the literature are not direct. Regarding the sentence "Nonetheless they (Formenti et al in PM40) measured for the same samples an averaged iron concentration of $10 \mu\text{g m}^{-3}$, in the same order of magnitude as our maximum concentration of $11.2 \mu\text{g m}^{-3}$ in PM1"; this is not directly comparable, Fe in PM40 with Fe in PM1.

13. Section 3.1.2. To compare with % determined in DD or soil samples, the ratios that should be taken from this study are the Fe/(PM1-ACSM-BC), assuming PM1-ACSM-BC a proxy for DD if we disregard sea salt, as DD or soil samples do not have the NR components that we have in the PM1 in this study. This ratio (Fe/(PM1-ACSM-BC)) is 20% aprox for IOP-1 and about 23% for continental days (according to Figure S2).

14. Section 3.1.3. "regional background sites such as MontSec, Spain". Consider replacing regional by continental, since Montsec site is defined as continental back-

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ground site in Ripoll et al, 2015.

15. Section 3.1.4. The wind is always from the North (NW-NE), so the variation along the day cannot be explained by transport only, since the transport takes place the entire daytime (during the night the wind velocity is lower, so this can partially explain some variation).

16. Section 3.2.2. "The HOA rose plot shows marked peaks in the directions of the two open waste burning areas and of the fish-smoking area located northeast of the site in the outskirts of M'Bour". Can you please give a tentative explanation for this?

17. Section 3.2.2. Could you comment on the 5-factors solution constraining HOA and COA? Do you get LCOA and 2 different OOA factors? If this is the case, you could see different origins for OOA, ideally locally formed versus transported? Or is the 5-factors solution resulting in a mix LCOA-OOA factor not well defined?

18. Section 3.2.2. The COA profile does not meet the 41>43 characteristic of the COA. Do you have any comment on this?

19. Section 3.2.2. "Our measurements over a large period of four months". Please re-write to state the 3 months period. Maybe 3 months cannot be considered a large period (true it is larger than typical 3 weeks campaign for AMS, but it is not very large).

20. Conclusions section. "during four months of the 2015 dry season". Please revise if you wanted to say dry season, or dry+wet. Please correct the duration to 3 months.

21. Conclusions section. "This factor (LCOA), although minor on average, could represent as high as 7% on a 30-minute time period when the air masses were blowing from the waste burning areas". Isn't it even more as a maximum? The average contribution of LCOA for marine events is 7%, so there must be some individual 30-min data points with a higher contribution. Or are these points you mention when air masses blow from the waste burning areas not taking place during marine-classified days? Maybe worth to clarify this. Moreover, maybe worth to clarify also the differences in absolute

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contributions, although the percentage is higher for marine days (by a factor of 2 or 3 with respect to other days), the absolute contribution is not so much higher. Still the absolute contribution for marine is higher (continental: $5.98 \mu\text{g m}^{-3} \times 36\%$ of OA $\times 2\%$ of LCOA = $0.04 \mu\text{g m}^{-3}$ of LCOA; sea breeze: $6.29 \mu\text{g m}^{-3} \times 40\%$ of OA $\times 3\%$ of LCOA = $0.08 \mu\text{g m}^{-3}$ of LCOA; marine: $6.09 \mu\text{g m}^{-3} \times 25\%$ of OA $\times 7\%$ of LCOA = $0.11 \mu\text{g m}^{-3}$ of LCOA) (calculations to be improved with the corresponding decimals and not rounded values taken from the plots). Why the absolute average contribution of LCOA is higher for marine days even that during marine days the wind is never coming from the identified waste burning sources, according to map and wind rose in Figs 1 and 2?

22. Figure 5. Maybe choose a different color for Fe (in print it looks same as sulfate).

23. Figure 7a. Consider choosing a different scale for OM and (SO₄, NH₄, NO₃ and Chl) to help seeing the variations, not very evident now for components different from OM.

24. Figure 7b. Fe correlates with BC. This is an indication that the Fe calculation should be revised. The explanation of the co-transport of BC and Fe may not explain completely this parallel behavior. You could isolate the dust events and see the differences in Fe and BC ratios.

25. Figure S6. Should legend in first plot read LCOA instead of WCOA?

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