

Interactive comment on “Atmospheric aerosols in Rome, Italy: Sources, dynamics and spatial variations during two seasons” by Caroline Struckmeier et al.

Anonymous Referee #2

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The following paper presents a comprehensive study at two different sites of Rome and surroundings (called urban and suburban) for two different seasons, totaling four rather short datasets. Measurements of coarse and fine aerosol modes were performed, with focus on the latter, as well as several gases and meteorological parameters. Organic aerosol data were further analyzed with PMF and several different components were identified. Overall I recommend the paper if the comments below would be addressed.

Some specific comments, trying not to overlap with the previous Referee:

1) P7, L21-23: Why Canagaratna et al. (2015) was not used instead? 2) P7, L29-30: What would such variabilities be? Are they inside a 20% variation? 3) P8, L3-12: More

information about the PMF analysis would be very welcome. For example, what was the input matrix size, how many rows and columns? Were the outliers downweighted? Same seasons were run together or separately? 4) P8, L13-22: The Q/Qexp value is normally useful in the identification of the final solution. However, no information on that was provided. 5) P10, L15-17: It is true that absolute values of POA mass concentration are larger in the suburban area compared to the urban; however, note that the percentages are smaller. According to Figure 2 POA in the urban is still 58% of organics, while suburban is 46%. So it seems to me that POA mass is larger in the suburban because PM1 is ~ 2 times larger and not because BBOA itself. I think those lines need clarification. 6) Table 3 e Figure 2: Beautiful and instructive figure! Please verify the PM1 values, they do not agree with the Table 3. For instance, Figure 2 says that PM1 at suburban/2013 was $12 \mu\text{g m}^{-3}$, while the Table 3 says it was $15 \pm 10 \mu\text{g m}^{-3}$. Which one is correct? Same thing to the other 3 campaigns. 7) P10, L22-25: How can a fresher OOA (so called SV-OOA) be extracted if solar radiation and O3 are larger? Shouldn't it be the opposite? The larger amount of solar radiation and air T should fasten the aerosol ageing. Please provide further explanation. For example, could the fresher OOA be maybe related to different air masses reaching the region during this time of the year, further air circulation/dispersion? Also this time of the year may allow for larger ranges in air T and solar radiation, . . . 8) P10, L27-30: The reduced solar radiation makes a reasonable explanation at the urban site, where OOA increased from 42% to 76% from winter to summer. However, not at the suburban where almost no change was observed (53-56%). That is, if the radiation increased at both sites, why only at the urban such effect was observed? For sure solar radiation is important, but it is not the only aerosol ageing agent. There are locations where LV-OOA is dominant under almost no solar radiation (i.e. Finland). Moreover, was fog observed/measured during the Oct/Nov measurements? Particle water uptake associated with fog may promote aerosol processing through heterogeneous-phase chemistry. The presence of fog has been well studied in other regions of Italy during the same season (i.e. Po Valley). 9) P11, L4-6: The authors say that the lower fraction of sulphate on Oct/Nov could be

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related to the enhance of POA because of lower boundary layer heights (BLH). If the BLH was lower, that should affect all pollutants equally, right? In addition, a significant change in POA fraction, between seasons, was only observed at the urban site, not in the suburban. 10) P11, L22-25: The authors say that absolute concentrations of suburban home-made PM1 are larger than urban, probably due to meteorological conditions. Right. However, note that in percentage the urban is larger than the suburban (70 and 47%). In reality, home-made PM1 is smaller in absolute concentrations at the suburban because all PM1 is ~ 2 times smaller. Thus, the meteorological conditions are the main reason for such decrease in PM1. The home-made PM1 decrease is just following the PM1 trend. It seems to me that the use of absolute concentrations only, can slightly misguide the reader. 11) P13, L6-8: It would be very kind of the authors to point out in here which minerals were identified in Silvia Nara, personal communication 2016, since this communication is not easily available. 12) P18, L16-20: In fact, what Jimenez et al. (2009) state is that the larger dynamic range in solar radiation and air temperature may cause the different OOAs. 13) P19, L23-28: Those last sentences are slightly confusing and seem to contradict the “home-made vs advected” section of this study. That is, in the “home-made vs advected” section nitrate was considered home-made. Here it is said that nitrate is north-east advected. Please clarify. Why are those sections separated if they complement one another? 14) P21, L7-11: I’m surprised BBOA is still so significant in late spring. Unfortunately, the authors did not provide the MS for the reader. Are the two BBOAs (summer-like and winter-like) similar? If one BBOA was more related to agricultural and the other to agricultural and domestic heating, I’d expect to see some differences among them, was any observed? Also with respect to external tracers. 15) P24, L1-4: Very interesting! The MS of CSOA identified by this study look quite different from the previous (Fröhlich et al., 2015 and Faber et al., 2013), with prominent fragments at m/z s 84. Also the N-containing fragment at m/z 42 look prominent and present in NIST MS database. Why this fragment was not mentioned in this study? Was it because of interference from other fragments in the same nominal m/z ? 16) P24, L5-6: Just a comment: when comparing SV-OOA

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and LV-OOA MS from the AMS database (Ng et al., 2010), also a $R^2=0.65$ is obtained, and those are very different things! Thus, I'd be very careful in comparing CSOA obtained in this study to previous ones. I mean, there is no need to try to make them look similar, since the current study seems to have identified a much clearer CSOA factor. 17) P25, L3: The authors used C₅H₁₀N⁺ fragment as a marker for CSOA. Since the measurements were performed with and HR-ToF-AMS, which provides size distribution data. Did the authors try to identify the size distribution of the CSOA using this same marker? Or perhaps just UMR m/z 84, 42?

Minor comments: 18) P18, L4: The right citation is Alfarra et al., 2004 (Characterization of urban and rural organic particulate in the Lower Fraser Valley using two Aerodyne Aerosol Mass Spectrometers). 19) P18, L5-11: This sentence is too long and difficult to read. 20) P20, L14: Probably the “:” should be replaced by a “.”. 21) P23, L13: Replace “:” by “.”.

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