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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The manuscript by Struckmeier et al. analyzed four datasets that were collected at two sites in different seasons in Rome (suburban vs. urban). This study contains various real-time online measurements including aerosol chemical composition, gaseous species, particle number concentrations, and meteorological parameters. The sources of organic aerosols (OA) were also analyzed with positive matrix factorization. While rich chemical information was provided in this work to address the sources, dynamics, and spatial variations, the discussions e.g., composition, dust event, new particle formation, OA, and CSOA are scattered and lack of focus. Also, I have several major concerns on data analysis and the interpretations: (1) each campaign lasted less than two weeks, and most importantly, the measurements at the suburban and urban

sites were not simultaneous. This clearly increases the uncertainties in comparing aerosol chemistry and sources between the two sites. In addition, it is difficult to see the dynamic variations of aerosol species in Rome if the authors didn't present time series data. (2) the data quality was not validated adequately, particularly the AMS measurements. A simple comparison between PM1 measured by EDM and that measured by AMS and MAAP (NR-PM1 + BC) will help. (3) the AMS data analysis needed to be expanded. For example, which approach (Aiken et al., 2008 or Canagaratna et al., 2015) was used to calculate the elemental ratios the calculation of elemental ratios? If there are elemental ratios, why did the authors still use f43 and f44 to discuss the oxidation states? (4) the PMF analysis is a big weakness of this study. The authors didn't have a full evaluation of the PMF results. At least, the authors need to present the mass spectral profiles and times series of all OA factors, and also the comparisons with collocated measurements. The diurnal correlations the authors mentioned in page 21 did not mean much. Figure 2 also showed substantial differences in HOA/BC ratios at the two sites in different seasons, and surprising BC contributions, which should be well interpreted. The PMF uncertainties lead to another major concern of the cigarette smoking factor. Although the authors concluded this as a major finding and presented a long discussion on it, it is still not convincing due to the limited resolution of V-mode ($C_5H_{10}N^+$) and the absence of the measurements of molecular markers for cigarette smoking. I am also suspicious that the diurnal profile of CSOA did not reflect cigarette smoking that is expected less affected by boundary layer dynamics (if the authors claimed it as a point source). Showing the times series of CSOA factor will help. (5) the new particle formation in this study appears to have problems too. At least from the average diurnal cycles in Figure 5, we didn't see "banana" shape. On the other hand, the diurnal cycles appears to indicate strong local sources at both sites. (6) the classifications of "home-made" and "advected" might also have large uncertainties. For example, OOA can be from both sources since SV-OOA and LV-OOA cannot be separated. Although nitrate has a shorter life time than sulfate and LV-OOA, many studies have shown that regional transport can be important. I understand the authors can judge this based on the polar plots in Figure 8. In fact, I suggest that the authors re-analyze the polar plots by considering the influences of the number points in each cell. For example, the wind rose plots in Figure 1b shows a small frequency from the northeast, the polar plots in this direction can be significantly biased by sporadic spikes. With this, I cannot recommend it for publication on ACP.

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