

Interactive comment on “Sources and processes that control the submicron organic aerosol in an urban Mediterranean environment (Athens) using high temporal resolution chemical composition measurements” by Iasonas Stavroulas et al.

Anonymous Referee #4

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The manuscript presents a one-year dataset (2016/2017) of near real time chemical composition of submicron aerosol particles measured in Athens and its subsequent PMF analysis. This dataset is complemented by 2 intensive campaigns carried out in winter (2013/2014 and 2015/2016). While these data are of prime interest, the manuscript is very descriptive and do not bring significant new results for the scientific community. However, I support the publication of this manuscript after major modifications.

1/ The PMF analysis and the constraints applied are somewhat confusing and the

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methodology should be described more clearly and in a more systematical way. A lot of different alpha values are selected (arbitrarily?) for the different factors. For a given source profile authors choose different alpha values for the different dataset. This must be explained and justified. Did the authors studied the influence of the alpha values on the sources contributions in a more systematic way? An alpha value of 0.1 is, from my point of view, too low for COA. Same for HOA, an alpha value of 0.05 is, in a first approach, too low considering the variability of the vehicular fleet (diesel/gasoline share, ...).

2/ Authors should convince the reader of the validity of the COA factor extracted from their analysis. The COA factor extracted here from the PMF analysis represents a contribution as high as BBOA in winter. It seems well correlated with the BBOA factor and other combustion markers (nssK+ for instance) and do not exhibit the classical midday hump. As the COA MS profile contains a slight contribution of m/z 60, I suspect a mix of both COA and BBOA factor. Also, the reference mass spectra chosen to constrain COA has been obtained in Paris. In Paris, the main site was located in the local Chinatown and was surrounded by well-known fast food brands. One could assume that the cooking emissions in Athens are slightly different than those of Paris for this specific study.

3/ The split of the data series between warm and cold period sounds quite arbitrary. Does it actually rely on temperature? If yes, this should be explicitly discussed in the text. While necessary for such long data series, splitting the dataset can induce a discontinuity of the sources contributions. Are such discontinuities observed here?

4/ If the data are available, I strongly suggest that the authors carry out a local winds analysis. From my experience such high nocturnal peaks are often mostly associated to local wind changes and in this case the occurrence of nocturnal breezes. In such cases (heavily polluted urban area), a local wind analysis is, from my point of view, much more relevant than a long-range transport analysis. Also, the influence of local wind patterns can induce strong correlation within the dataset which can not be related

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to sources intensities or atmospheric transformation processes.

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