Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-356-RC2, 2018

Atmospheric Chemistry and Physics Discussions
Open Access

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 #2

Received and published: 13 June 2018

This manuscript presents a long-term dataset of near real time chemical composition of submicron aerosols in Athens, Greece. It is completed by two intensive campaigns during winter time. Statistical analysis was performed in order to apportion the sources of organic matter. The subject of this paper is of interest and falls within the scope of ACP, although in its current form neither the methodology (PMF) nor the results bring strikingly new outputs in this region. I am still favorable to publication after major revisions.

Major Comments 1) Overall, the result section is too descriptive. Describing the

Printer-friendly version

Discussion paper

angles between profiles impinges upon the actual results. The authors should re-focus the discussions on how this study slots into previous knowledge in Greece (Athens and their cities, such as Patras) and, why not, in the Eastern Mediterranean. Moreover, strong assessments are made regarding SOA, simply from diurnal variations. The authors should either tone down these statements or add much more discussion and figures. Then, organonitrates have been found to have significant contributions in Greece (Florou et al., 2017). Can the authors add some more knowledge about that? (especially regarding the role of primary combustion sources in their formation?) 2) The method used by the authors to select the appropriate solution is not clearly stated, although it was inspired by Crippa et al. (2014). From the first ACSM intercomparison, Frohlich et al. (2015) proposed a methodology of find optimal solution, and stated some recommendations. Why did the authors prefer Crippa et al.? 3) The local vs regional vs advected features are not well characterized, and I would strongly recommend the authors to perform a wind analysis, especially for the "local" sources (eg traffic & biomass burning).

Minor comments - P3 l78: the introduction mainly focuses on wintertime biomass burning, so why would you need long-term datasets? - P4 l120: please indicate the calibration values - P4 l103: the ACSM does not measure "aerosol mass" but only the chemical composition; it is not equivalent to a TEOM-FDMS. - P5 l123: it is not clear why a chemical-dependent CE has not been applied. Although it is discussed later on, it could be quickly stated here. - P5 l125: did the authors use denuders ahead of the PILS in order to prevent nitric acid, sulfuric acid and ammonia to be respectively confused with particulate nitrate, sulfate and ammonium? - P5: filter samplings are not presented. - P10 l292: See major comment. I don't think that the only fact that nitrate has a morning peak similar to BC is enough to link it with morning traffic. More discussion would be needed. - P11 l318-319: Datasets have been separated into cold and warm months prior to PMF. Why not seasonally? Not just cold and warm months influence the characteristics of secondary organic aerosols, it could also be related to air masses. So the approach chosen here is not well justified. - P11 l329:

Printer-friendly version

Discussion paper

why the HOA from Ng et al. has been used? Why not any other profiles, especially gotten from previous studies in Greece? - P11 l333: Do the authors have any hint of how representative the BBOA of Ng et al. is in Greece? - P12 l340-341: it could be appreciable if the authors provide a bit more details on the metrics used through the correlation of PMF timeseries with external tracers. - P12 l351: a correlation coefficient of 0.86 corresponds to a r^2 of 0.63, which is still a good statistical correlation. Ranges of r^2 are by the way not consistent throughout the manuscript. P13 l397 and p14 l400, r^2 of 0.32, 0.36 and 0.39 are considered as "moderate", which should rather be a poor correlation. Later, p16 l477, a r^2 of 0.53 is considered as "very well". I strongly suggests the authors to use a consistent description of correlation coefficients. - P13 l388-390: See major comment. linking SVOOA with primary sources only from mean diurnal variations is not convincing. Please add more discussion. - P14 l400: See major comment. Same comment, the statement "SVOOA may, to some extent, partially originate from a combustion source" seems random and hardly quantifiable. - P14 l407: "HOA emissions are very low", compared to what? - P14 l422: the authors would need to prove the link between SOA and regional biomass burning. - P15 l444-448: how does BBOA compare with BBOA profiles from other studies in Greece? Or in other Mediterranean sites? - P16 l477-479: HOA correlates moderately with CO, BC and NO. So is HOA representative of traffic?

Technical corrections and suggestions - P1 l22: replace "fine" by "submicron" - P1 l24: rephrase to "with concentrations during wintertime sporadically reaching up to 200 μg/m3". Please also indicate the time resolution for this (daily/hourly concentrations?) - P2 l50: replace "namely" by "such as" - P4 l92: "105 m above sea level" - P4 l105-109: these information are redundant and/or well known. It could be removed. - P6 l165-172: I don't think a thorough description of PMF and ME-2 is necessary here. Please shorten or remove this section. - P8 l237: rephrase to "The other striking feature is that" - P8 l241: rephrase to "average 8 of such" - P8 l242: please add "to our knowledge" - P8 l243: rephrase to "highlight the strong impact" - P10 l286: rephrase to "to the regional character" - P11 l231: one could cite here Canonaco et al.(2015): Canonaco, F., Slowik, J. G., Baltensperger, U., and Prévôt, A. S. H.: Seasonal differences in oxygenated organic aerosol composition: implications for emissions sources and factor analysis, Atmos. Chem. Phys., 15, 6993-7002, https://doi.org/10.5194/acp-15-6993-2015, 2015. - P12 l343-350: I think this has already been presented elsewhere, so I don't think it is necessary here.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-356, 2018.