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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long term characterization of organic and elemental carbon in the $PM_{2.5}$ fraction: the case of Athens, Greece" *by* D. Paraskevopoulou et al.

Anonymous Referee #2

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This MS presents an assessment of OC and EC concentrations in Athens (Greece) over a 5-year period, providing a comparison for a shorter period of time with a background station in Crete. The results from the assessment are interesting for the scientific community, even if the methodollogies applied are not especially novel. However, the long and uninterrupted series of data is very valuable and makes up for this. I would recommend publication after a number of issues have been addressed. I am mostly concerned by the interpretation of wood burning as a source, as there seem to be some contradictions in the text (please see below).

- page 17163 *line 5: please add "road traffic" before "wintertime high energy con-

sumption", as this is probably the more relevant source. *line 6, please add "traffic but also" before "secondary organic aerosol" *line 13: please clarify this sentence, what does "in excess of other significant sources" mean? * line 18: "prevailing emissions" from what kind of sources? Please specify. * line 23: the authors could stress that this is the major contribution of their work, the extremely long and uninterrupted series. * line 27: "stalked", should this maybe be "occurring"?

- page 17164 *line 6: please define ACTRIS * line 20: is a website available for the station? If so, please provide * line23: "during most of the period", how long is this? I undertsand 2 different samplers were used?

- page 17165 * lines 10-15: please describe briefly the limitations of this sampling approach: it has been pointed out that the back-to-back filter configuration does not allow enough time for equilibration of gases on the second filter, and that therefore this approach may underestimate the positive OC artefact.

- page 17166 * line 2: was it a laboratory or a fiels Sunset instrument? * line 18: "IC", how was the IC concentration obtained? By decarbonisation with an acid of one sample, running another sample and calculating the difference? Please describe briefly.

- line 17168 * line 10: please add reference to Querol et al. (2013): Variability of carbonaceous aerosols in remote, rural, urban and industrial environments in Spain: implications for air quality policy; Atmos. Chem. Phys., 13, 6185-6206, 2013 www.atmos-chem-phys.net/13/6185/2013/ doi:10.5194/acp-13-6185-2013 * line 27: "local sources" should be "local emissions", given that it is the emissions and not the sources that are trapped. * line 27: "Particulate OC" should be "particulate "OC and EC", given that this process affects both OC and EC.

- page 17169 * line 11: "reproduced by OC": why does winter OC not decrease in 2012, as is the case for EC? If the economic recession is indeed the cause for this trend (which is likely), then the behaviour of OC and EC should be more similar during this period.

- page 17170 * line 9: "are proportional" should be "are mostly proportional"; an R2 of 0.49 is not especially high. * line 12: "suggest that OC and EC" should be "suggest that a large fraction of OC and EC", not all of OC and EC because the correlation is not too high. * line 23: Figure 6 is referenced here but Figure 5 was not discussed before

- page 17171 * line 19: please describe briefly what the SCiare and Pio methodlogies are based on * line 29: "improves during summer": this would suggest that during summer one significant origin of WSOC is long-range transport and aerosol ageing, given that nss-SO42- is a tracer of this kind of processes.

- page 17172 * line 2: "in winter", because in winter a significant source of WSOC is biomass burning * line 10: please add "water-soluble" before potassium * line 21: "calculated" could be "obtained" * line 22: "neither do they improve on a seasonal basis": this is unexpected: if biomass burning is a source of winter OC and EC, as described until now in the text, then nss-K+ should correlate at least partly with OC in winter. If this is not the case, how do the authors explain it? * line 24: "mainly fossil fuel combustion", I agree that traffic is the main source, but some correlation should be observed if biomass burning is a (although minor) source. Please explain.

- page 17173 * line 11: "influence of wood burning during the last years": here it is suggested that wood burning is a relevant source and therefore it contradicts the results on lines 21-27 on page 17172. * line 11: I think Figure 5 should be Figure 8?

- page 17175 * line 9: ratio =1: was the OC/ECmin also calculated graphically, to verify that the local OC/ECmin is similar to the literature value proposed? What values were obtained? * line 16: what was the % of SOC with respect to OC? It would be interesting to see whether teh Athens values are similar in relative terms to other Southern European cities, and not only in absolute values. * line 17: figure 9: an additional Figure (or an additional variable in Fig 9) would be useful, showing the variability of the % of SOC in OC throughout the year. This would allow to verify that the relative contribution of POC to OC is higher in winter

- page 17176 * line 5: please review the order of figures

- page 17177 * line 10: please add "mostly" or "largely" before "proportional" * line 11: please add "mainly" before "emitted" * line 21: it does, according to Figure 2, with winter maxima and an additional maximum in high summer.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17161, 2014.