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***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 #1

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General Comments

This paper describes daily average concentrations of PM_{2.5} atmospheric aerosol at a suburban measuring station, in the Athens metropolitan area. For comparison and interpretation the authors also use parallel aerosol measurements of a background measuring station (Finokalia). Although the analytical measurements performed in the aerosol (mainly PM_{2.5} mass, organic carbon (OC), elementary carbon (EC), water soluble organic carbon (WSOC) and water soluble ions) are not innovative the fact that these measurements were taken almost continuously during 5 years (from May 2008 to April 2013) provides multi-annual trending information that is important to the un-

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derstanding of atmospheric processes involving particulate carbon. Therefore the data and paper merits to be published. I agree with most of the discussion and conclusions of the manuscript, but I have doubts about the interpretations and conclusions relating with biomass burning originated particulate carbon. The authors interpret the composition and seasonal/annual variability in aerosol/carbon concentrations as an effect of several processes namely the local impact of biomass burning for home heating during winter which they say has been of increasing importance during more recent years as result of the economic crisis that obliged the population to change to biomass fuels. As it is known biomass produces large amounts of carbonaceous particles (principally OC) when burned in less well controlled stoves and heaters. The reasoning of the authors is logical but in my opinion it is not demonstrated, being the winter /summer variability observed also alternatively explained by meteorological specific conditions such as winter more frequent atmospheric inversions and poor dispersion. The utilization of water soluble potassium tracer in Figure 5 did not confirm the predominance of biomass burning during winter periods (may be because there is interference of soil originated potassium, principally during summer). Therefore the subject should be discussed more thoroughly along the manuscript, not treating it as a clear evidence. The presentation of statistics of wood consumption for home heat burning in the Athens area, showing an increase during more recent years, would be a helpful contribution to the demonstration of the biomass burning effect.

Specific comments Page 17166, lines 15 and 24- 0.45 μm instead of 0.45 mm Section 2.33 – describe better how TC and IC are analyzed to calculate WSOC. Page 17167- lines 14 and others (average \pm standard deviation (?)) Page 17168line 26- (due to emissions from residential heating) – discuss. It can't be said as if it is demonstrated. Page 17169 lines 11 and 12- the number trends for 2008 and 2012 (1.21 and 0.59) given in the text are different from those in Figure 3 (0.99 and 0.43). Should not be equal? Page 17170 lines 7-13- The discussion and conclusions here are not very clear and relevant. A R2 of 0.49 is low for taking firm conclusions.

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Page 17170- In the text, discussion passes from figure 4 to figure 6. Figure 5 only later in the paper is discussed. Reorder the figures.

Page 17170- section 3.3- there is a discussion here relating OC with WSOC. Being WSOC more than 60% of OC, of course that there is a good correlation between both. More clear information could be taken from the relation between WSOC and WIOC which are independent (?) variables. Page 17171, lines 20-25- The formation processes of nssSO₄ are probably quite different from secondary OC; therefore the conclusion in this part of the text is not clear and relevant. Page 17171, lines 24-25- correlations between nssK⁺ and WSOC during winter only should probably be more clarifying than for the whole year. Page 17171, lines 24-26- Why not use Ca²⁺ to remove K⁺ from soil origin, in order to obtain only K⁺ from biomass burning? Page 17172 lines 16-18- To me this is not clear. EC is not very efficiently removed by rainfall; therefore the correlation between EC and nssK⁺ in winter can result also from higher regional emissions from biomass burning during this period of the year and advection / long range transport to the sampling site. Page 1716, lines 9-10- , . . . or more inefficient dispersion of local emissions in Athens during winter. Page 1716, lines 13-14- Add a reference demonstrating the maximization of wood burning activities during 2011-2013.

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

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