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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Sources and chemical characterization of organic aerosol during the summer in the eastern Mediterranean" *by* E. Kostenidou et al.

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The manuscript by Kostenidou et al. presents chemical composition and sources of fine particulate matter in the Eastern Mediterranean based on AMS measurements in the suburbs of Patras and Athens and subsequent PMF analysis. There are not too many studies on air quality in the Eastern Mediterranean, which makes the present analysis an important addition. I recommend publication in ACP after the following comments have been addressed:

General comments:

(1) *Please add more info and clarifications to the site descriptions. Especially for Patras it is not clear to me at which of the two sites mentioned the instruments described in section 2.3 were measuring. Also add a map that shows the locations of the 2 sampling sites in Patras, and their position relative to the sampling site in Athens.*

All of the instrumentation described in Section 2.3 was located at the ICE-HT institute. We added a sentence at the end of this section 2.3 clarifying this point. In addition we added a sentence at the end of section 2.2 highlighting that this paper focuses only in the ICE-HT measurements. We also added two maps in the SI illustrating the locations of the two sampling sites in Patras and the positions of the Patras and Athens sites.

(2) *The measurements in Patras and Athens could not be done at the same time due to experimental limitations, I can see that. However, to still be able to make some comparisons between the two sites you could compare parameters that were measured simultaneously (meteorology? SMPS? Etc.). This would give an idea of how different the meteorological and pollution situation was for the 2 measurement periods, and how that potentially influences AMS mass loadings.*

A detailed description and intercomparison of the various measurements (including filter samples) performed in Athens and Patras is given in the companion paper by Tsiflikiotou et al. (in preparation). The present work focuses on the OA sources and AMS data. We added a brief summary of the conclusions of Tsiflikiotou et al. about the two periods and the OA levels in the two cities to provide additional context for the present work.

(3) *There are quite a few comparisons in the manuscript between PM1 and PM2.5 data, e.g. sulfate for the CE, and (section 3.1) OM and Org. This can be done, but discrepancies should specifically be discussed for the size difference. Also, what conclusions are drawn based on the comparison in section 3.1 (p. 3463, l. 23 – 26)? Section 3.2 (p. 3465, l. 8 – 12): How can AMS PM1 be higher than PM2.5? This needs*

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to be elaborated.

We have made sure that the different size ranges (PM1 for the AMS and PM2.5 for the filter samples) are taken into account during the various comparisons. A number of useful conclusions are also drawn by the agreement or lack thereof between the two techniques. For example the good agreement between the AMS sulphate and the PM2.5 sulphate from the filter samples probably suggests that almost all of the PM2.5 sulphate was in the submicrometer size range during these campaigns. The scatter observed when comparing the corresponding measurements is due to a number of factors including the positive and negative artifacts of filter measurements, the uncertainties of the corresponding measurements (e.g., due to AMS collection efficiency), and the different size ranges. The negative artifacts of the filter measurements due to evaporation of the collected OA can explain the occurrence of some measurements in which the PM2.5 filter-based OA is less than the PM1 AMS OA. These issues are now discussed in the revised paper.

(4) *What is the reasoning behind calling the 2 non-biogenic OOA factors V- and M-OOA, and not LV-, and SV-OOA, respectively? Are they significantly different from literature LV- and SV-OOA factors? If yes, this should be mentioned, otherwise I suggest renaming the factors.*

The names of the 2 non-biogenic OOA factors (very oxygenated OA and moderately oxygenated OA) were chosen based on their oxygenation degree. The PMF analysis separates these factors based mainly on their oxygen content and not on their volatility. Unfortunately, no volatility measurements were made during these campaigns. Recent (not yet published) work by our group suggests that the V-OOA factor also contains some relatively volatile components, and the M-OOA contains low volatility compounds. We understand that the LV-OOA and SV-OOA terms have been used in several past studies and we do explain the correspondence and compare our factors to the corresponding LV-OOA and SV-OOA ones (see for example Table 1). V-OOA was

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close to the literature LV-OOA spectra, but M-OOA had a moderate correlation with most of the SV-OOA spectra. We have added discussion of these topics in the revised paper but we do prefer to keep the existing names of these factors.

(5) *I am a bit confused by the interpretation of the two factors called HOA-1 and HOA-2. P. The factor profile of HOA-1 is similar to HOA factor profiles found in literature, however its factor time series does not correlate with traffic marker time series. The HOA-2 factor profile resembles COA, the diurnal pattern of the time series indicates contributions from cooking emissions, however its time series correlates also with BC. When you did ME-2, was HOA fixed? I wonder whether the different PMF solutions and their rotations could specifically be explored for a distinction of the different primary organic sources.*

In a series of sensitivity tests, we tried rotations in the fpeak range -1 to 1. In all of these tests the correlations between our factors HOA-1 and HOA-2 and traffic markers such as BC and toluene practically did not change. We also used a fixed HOA spectrum based on the Aiken et al. (2009) as well as the HOA mass spectrum found in Athens during winter (unpublished results). In these tests too the correlation between HOA-1 and BC did not improve. All these suggest that are conclusions are robust to the details of the PMF process. We have added a summary of the results of these sensitivity tests to the revised manuscript. Our explanation is that the correlations are affected also by the locations of the various sources around the receptor site and the corresponding wind directions. The fact that these area sources (gasoline cars, diesel cars, cooking activities) did not have a homogeneous spatial distribution can lead to these rather unexpected results.

Specific comments:

(6) *P. 3456, l. 15 – 16, and throughout manuscript: P. 3459, section 2.1.: Is there a reference about the larger study you could add? If not consider adding a few sentences*

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on this study here.

We have added the corresponding reference for the summer study (Tsiflikiotou et al., in preparation).

(7) *P. 3462, l. 1 -4: Add reference to the supplementary information. Also, add (here or in the supplement) information on the ME-2 settings you used – which factor spectra were fixed, how fixed were they, etc. You also mention in the SI several times that you chose fpeak=0 because for fpeak=0 Q/Qexp was minimized. Fpeak=0 will always produce Q/Qexp = min, you specifically use fpeak6=0 to distort the solution (compare Ulbrich et al., ACP, 2009).*

We added the Lanz et al. (2008) reference to the Supplementary Information. For the solutions presented in this paper we did not use constrained solutions in ME-2. For this reason we did not include such details. We tried constraining the HOA spectrum in one of the sensitivity tests (see our response to comment 5 above) and we have included the corresponding information about the spectra used as well as the corresponding parameter values. The fpeak selection was not based solely on the minimization of Q/Qexpected, but also on the mass spectra characteristics and the stability of the different fpeak solutions calculated by both the ME-2 and PET algorithms. We have modified the SI discussing in more detail the factor selection.

(8) *P. 3462, section 2.6: Concerning the CE in Patras, do you have any information on ambient RH? Can you see a dependency of the CE on RH?*

The R^2 between CE and the ambient RH was very low and equal to 0.02. This low correlation and the high CE values can be explained by the fact that the particles were acidic most of the time and probably contained water during practically all the campaign.

(9) *P. 3469, l. 18: What about the influence of boundary layer height? Temperature?*

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If there was an influence of the boundary layer height it would be visible in all factors. However while M-OOA and HOA-2 increased, V-OOA and HOA-1 decreased during the night. The average temperature at noon was around 32^{o}C and during the night it decreased to around 21^{o}C. The change in temperature could be one of the explanations for the increase in M-OOA. We have added this potential explanation and the corresponding discussion to the revised manuscript.

(10) *P. 3472, l. 15 – 16: Add more information on the meaning of this plot and your data in this plot. Just showing it is of no scientific value.*

We moved Figure 6 to the SI and added some brief discussion.

Technical comments:

(11) *P. 3456, l. 2: Should read "The concentration and chemical composition of non-refractory [: : :] (delete "the")*

Corrected.

(12) *P. 3456, l. 11: Should read "In both cases PM1 [: : :]" (delete "the", and check throughout manuscript for more of such "thes")*

Done.

(13) *P. 3456, l. 25: Should read "by causing cardiovascular [: : :]"*

Corrected.

(14) *P. 3457, l. 1: Update reference to latest IPCC report.*

Updated.

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(15) *P. 3457, l. 4: Should read "contributes to [: : :]" (check throughout manuscript for more).*

Replaced with "represents".

(16) *P. 3457, l. 6: Use HR-ToF-AMS for instrument abbreviation.*

Done.

(17) *P. 3457, l. 6: Should read "high time resolution [: : :]"*

Corrected.

(18) *P. 3457, l. 25: Typo, should read "spray"*

Corrected.

(19) *P. 3457, l. 27: Concretize studies, e. g. "Most of the studies on air quality [: : :]"*

Done.

(20) *P. 3457, l. 29: Delete "just"*

Done.

(21) *P. 3458, l. 11: Replace "higher with "more".*

Done.

(22) *P. 3458, l. 16: Should read "[: : :]was of industrial origin."*

Corrected.

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(23) *P. 3458, l.16 – 20: Very complicated sentence, consider breaking it up into 2.*

We broke that sentence into 2.

(24) *P. 3458, l. 29: Replace "very hard".*

Replaced with "quite difficult".

(25) *P. 3459, l. 7 – 9: Consider adding a couple of sentences more on the objectives of the paper.*

We added information about the objectives of this study.

(26) *P. 3459, l. 12: Replace "results" by "measurements".*

Done.

(27) *P. 3459, l. 18: Major anthropogenic activities?*

We added "anthropogenic" before "activities".

(28) *P. 3461, l. 2: Most densely populated?*

Corrected.

(29) *P. 3461, l. 12: The aethalometer measures absorption and derives BC concentrations.*

We replaced "measured" with "provided".

(30) *P. 3461, l. 13: Delete comma after "classifier".*

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There is no comma after classifier. However we deleted the comma after SMPS.

(31) *P. 3462, l. 6: Should read" throughout".*

Corrected.

(32) *P. 3469, l. 1: Delete "while"; "Similar" instead of "similarly".*

We rephrased these two sentences.

(33) *P. 3470, l. 3: Should read "while BC originated from [: : :]"*

Corrected.

(34) *P. 3460, l. 19 – 20: Replace "correspondingly" by ", respectively".*

Corrected.

(35) *Supplemental information, figure caption S3: "suggests" instead of "suggest"*

Corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3455, 2015.