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Atmospheric Chemistry and Physics Discussions

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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(1) *The paper titled "Sources and chemical characterization of organic aerosol during the summer in the eastern Mediterranean" by Kostenidou et al. deals with the chemical characterization and organic sources identification in Athens and Patras, highlighting the importance of regional sources in that area. Positive matrix factorization has been performed on high resolution time of flight AMS data identifying both primary and secondary organic aerosol sources. This is a relevant topic in the atmospheric science field and therefore it is suitable for ACP. The overall quality of this work is good and the manuscript is quite well-written. I recommend publishing this work after the authors*

respond to the following comments.

We appreciate the positive assessment of our work

General comments

(2) *Despite most of the references cited in the introduction are pertinent, this section should be reorganized to give the clear context for this paper. The authors list many topics, like source apportionment techniques, organic sources, studies in the Mediterranean area etc., but this introduction appears to be independent from the topic treated in this paper. I would recommend to explicitly mention how your study is connected to this overview (you reported just one sentence at the end of the introduction).*

We have added in each subsection of the introduction information about the links of the present work with the topic discussed.

(3) *Did the authors expect to find marine related OA sources at their measurement sites? Being the two sites influenced by marine air masses, the S:C ratio should probably be higher than in continental urban sites. Can the authors further elaborate this concept?*

Given the location of the two sites, we would expect to find a marine OA (MOA) factor. The S:C ratio estimated by the AMS is often underestimated in the presence of organosulfates (Farmer et al., 2010; Docherty et al., 2011). To avoid such artifacts we investigated the contribution of MOA applying a constrained solution in the ME-2 using the MOA mass spectrum of Crippa et al. (2013) with a=0.1. For Patras the average MOA concentration for 4, 5, and 6 factors was around 0.04 μg m^{-3} corresponding to 1 percent of the OA mass. For Athens the MOA concentration for the 3, 4 and 5 factor solutions was approximately 0.25 μg m^{-3} (3.7 percent of the OA mass). So if MOA was indeed present its contribution to OA was very low. This information has been added to the revised manuscript.

ACPD
15, C2519–C2524, 2015

(4) *The conclusion section is quite poor. The authors should highlight the importance of their work also at the end of the paper.*

We have rewritten the conclusion section and added some highlights in the end of the paper.

Specific and technical comments

(5) *Page 3456, line 15: replace "HR-AMS" with "HR-ToF-AMS" (everywhere in the manuscript)*

Done.

(6) *Page 3457, line 4: replace "contributes" with constitutes.*

Done.

(7) *Page 3457, line 10: add the reference to Zhang et al., 2011.*

Added.

(8) *Page 3457, line 12: add the reference to Canonaco et al., 2013.*

Done.

(9) *Page 3462, line 9: I would mention that CE is usually around 0.5.*

Done.

(10) *Page 3464, line 18: reformulate "are not that useful".*

We replaced it with: "could result in erroneous conclusions"

ACPD

15, C2519–C2524, 2015

(11) *Page 3466, line 3: replace "types" with "sources".*

Done.

(12) *Page 3467: it would be interesting to show some of the PTRMS tracers correlations with the AMS SOA factors. I would also move Table S1 from the supplementary material to the main text.*

We added 3 figures in the SI showing the time series of the Patras M-OOA, Patras b-OOA and Athens V-OOA compared to PTR-MS tracers (2 tracers for each factor). We moved Table S1 to the main manuscript.

(13) *Page 3472, line 11: which m/z where so different from the HOA from Crippa et al. 2013b?*

The major differences were in the m/z's 44 and 28 which were higher than in Paris, m/z's 29 and 43 which were lower compared to Paris and m/z 39 which was absent in Paris probably due to the unit mass resolution spectra used as input for the Crippa et al. PMF analysis. Please note that there was a typo and the correct angle thetas between the HOA Paris (SIRTA) winter (of Crippa et al. 2013b) and HOA Patras and HOA Athens are 25 and 24 degrees correspondingly (not 33 and 31 degrees). We modified the corresponding text in the manuscript to explain the above points.

(14) *Figure 6 is expected to be used for SOA components. I would put it in the supplementary material.*

We moved Figure 6 to the SI.

(15) *Figures S3 and S6: include fitting parameters (slope, intercept).*

Done.

ACPD

15, C2519–C2524, 2015

(16) *Figure S14b: I would also highlight the extremely high contribution of m/z 44 for HOA-1.*

Added.

(17) *Figure S16b: I would also mention the lower contribution at m/z 44 for HOA-1*

In Figure S16a HOA-1 is actually HOA-2 and vice versa (there was a typo in this graph, which is now corrected). So, there is almost no difference between the m/z 44 for HOA-1 in Figure S16a and the m/z 44 for HOA-1 in Figure S16b.

(18) *A general recommendation for the SI: you should report at least few sentences describing the shown graphs to guide the reader, although a more complete description has been already reported in the main text.*

We have added 1-2 sentences in the SI providing additional information about the corresponding graphs.

References

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