

Interactive comment on “Temporal variability and sources of VOCs in urban areas of Eastern Mediterranean” by Christos Kaltsonoudis et al.

Anonymous Referee #2

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Review of “Temporal Variability and Sources of VOCs in Urban Areas of Eastern Mediterranean” by C. Kaltsonoudis et al. ACP-2016-358 Anonymous Referee Comments

This paper presents the results of VOC measurements using a PTRMS in two of the largest cities in Greece (Athens and Patra) during the summer and winter seasons. The paper presents a good overview of the most dominant VOCs observed and applies a standard PMF analysis to discuss the sources of VOCs. This is a well-organized and nicely written manuscript. These types of PMF analysis on gas phase measurements can be a powerful tool for understanding sources and is a generally underutilized tool. This paper provides a nice framework for future PTR studies which will utilize PMF in a similar fashion.

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General Comments

In general I find the manuscript to be quite complete considering the extent of the data set collected. I have a few technical comments that will follow. My biggest comment is that a summary figure similar in design to Figure S20 would be highly beneficial in the main text. This paper ultimately focuses on using PMF to determine the impact of the various source on ambient VOCs. In this sense a summary figure using pie charts to summarize for each measurement location and season showing the relative impact of each factor would be a great way to relay the study's results in a compact manner that is easy to digest. The figures currently in the main text show the diurnal trends of the figures nicely, but that does nothing to show the relative impacts of each factor. Addition of a figure like this and a more distilled discussion of the results from that figure would really benefit to overall clarity of the analysis.

In general, there is a lack of discussion as to the potential for misidentification of observed m/z . The case that is most obvious is the treatment of isoprene, where the authors appropriately initially identify the potential for furan detection but end the discussion at that point. I would imagine that after performing PMF analysis and retrieving a BBVOC factor containing isoprene that the authors should discuss this ‘isoprene’ as potentially signal due entirely to furan, or at least partially. Continuing to label this measurement as isoprene is somewhat misleading, considering the potential overlap.

Section 2.2 on the experimental details is lacking some details on the method that are useful to the reader to understand the robustness of the measurements. Things like how long were the inlets overall in the various studies? Were the entire inlets heated to 60C or just a subsection where the PTRMS sampled. What has been done to correct the data for humidity effects? How was the normalization of the data done? How often were calibration performed?

Specific Comments

In the first sentence of the abstract, the phrase “urban background sites” is used. This

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is not a commonly used term that requires some explanation, it may be best to simply state and urban site. Also in that first paragraph a sentence should be added introducing the winter season measurements. The last paragraph of the abstract launches into discussion of the results of winter measurements without first indicating that they were made.

Page 2, line 12, you need a new paragraph indent

Page 3, line 18, 'emissions' should be singular

Page 4, line 30, It rather well know that activated charcoal filters induce changes in humidity. As the sensitivity of the PTRMS to various species, such as methanol, is dependent on RH, how well do the authors believe they have been able to capture the true instrument background.

Page 7, line 14, the comment about elevated ozone levels being a result of long range transport seemly has no support in this manuscript. How do the authors know that? Were back trajectory analyses performed? This kind of a statement needs data to support it.

Page 7, line 33, suggest editing the phrase "peaked during noon at concentrations" with 'peaked at noon with concentrations"

Page 8, line 2, suggest reversing the order of 'significantly' and 'influenced'

Page 8, line 6, what spikes are being referred to here?

Page 8, line 23, suggest reversing the order of 'traffic' and 'hour'

Page 10, line 16-17, suggest rewriting to read "These periods (Table S2) occurred during the nighttime (18:00-06:00 LT) and were associated with acetonitrile. . ."

Page 10, Section 4, I would suggest that you reverse the order of the discussion in this section to first introduce the concept and equation for calculating EF, then discuss the results. Basically swap what is on page 11 with the text on page 10.

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Page 11, line 5, suggest editing to read "For the CO₂ emission factor (EF_{CO2}) a value of 1600. . ."

Page 11 line 27, why was the time period of two hours chosen here? Is there a basis for this time duration, was the correlation optimized at two hours, or is there a time shift that possibly induces a better correlation?

Page 12, line 8, Edit beginning of sentence to read "The OVOC factor. . ." Page 13, line 31, edit to read "Mixing ratios of OVOC-2 species were elevated during the night."

Page 15, line 1-4, That do you suggest that the OVOC-1 factor is? In the next paragraph you state that the OVOC-2 factor is associated with the background of VOCs but no discussion here.

Page 16, line 12, Again to reiterate an above comment calling this isoprene in the biomass burning factor is likely misleading. If the author make a change here, an update to the manuscript tables is likely necessary.

Figures 2-4, I am not sure that the main text is the appropriate place for these figures. I would either move them into the supplemental or edit the figures to include only a few key species for all three measurement sites to compare the diurnals observed in each site/season.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-358, 2016.

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