**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Sources and atmospheric processing of organic aerosol in the Mediterranean: insights from aerosol mass spectrometer factor analysis" *by* L. Hildebrandt et al.

Anonymous Referee #1

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Review of Hildebrandt et al The paper describes a wintertime study at the Finokalia field site in Crete during 2009. The data are compared with those taken from Finokalia in 2008 during a summertime experiment. Data from an Aerosol Mass Spectrometer are compared with filter based measurements to identify the key chemical components contributing to coarse and fine mode aerosols during the experiment and show that organic, sulfate and ammonium are important submicron components at the site. The authors should also comment that dust is an important submicron component also, not only in the main dust events but at other times. Coarse mode aerosol were shown to

be principally dust, sea salt and nitrate, though there was also a contribution from nss Sulfate that the authors should recognize.

Positive matrix factorization was applied to the time series of the organic mass spectral data in a similar manner to the FACE2008 experiment and in other previous studies using AMS data. This seems to be performed thoroughly and with good practise, though I would like to have seen a bootstrap analysis to ensure that the factors are robust across the data set. The analysis reveals 3 factors, the dominant one being an aged oxidised organic aerosol (OOA) seen in many other experiments. The authors make the point that as OOA in this experiment were dominant as it was in FACE-2008 and no fresh OOA (OOA-2) was observed then this is a demonstration that OA at the Finokalia site is representative of the end point of oxidation of OA. However, the authors should note (Ng et al., 2010 and Morgan et al., 2010) that the factor in one campaign need not be chemically identical to the same factor in another campaign as the factor analysis simply picks out a factor in the mass spectral time series that best describes the variability in that time series. Hence the OOA in FACE2009 need not be as processed as the OOA in FACE 2008. This is an important caveat and one that should be included in any revised paper.

The remaining two factors are an amine factor and a biomass burning factor. The amine factor shows interesting behaviour that the authors discuss. One concern I do have is that amines can be surface ionised, a fact noted by the authors, but the same analysis as they conducted in FACE-2008 is not presented. It would be good if the authors could demonstrate surface ionization is not important. The biomass burning factor does not have the typical mass spectral fingerprint of many previous BB studies using AMS. The authors demonstrate that the factor is consistent with some burning events and state that olive branch burning is the most likely cause. It would have been good to have shown a representative source sample mass spectrum of olive branch burning to prove the hypothesis.

Overall the manuscript is well written and is clear. The factors are well explained and

the final results and analysis are fully justified. This is a nice paper. I have a few comments that the authors may wish to consider.

Pg 19641 Abstract: It might be good to include something of the duration and seasonality of the measurements in the abstract

Pg 19645 line 18: To what extent are the CE values consistent with the more widely used CE of Matthews et al which is based directly on the AMS component mass. This is quite important as it affects other studies.

Pg 19645 line 24: What was the CE during FAME-08?

Pg 19646 line 4: could the Matthews et al correction be used when SMPS data were not available?

Pg 19647 line 12 and Pg 19671 figure caption: There is a significant contribution to PM1 from dust that even away from the events that should be commented on. It is insufficient to say that sulfate, organics and ammonium dominate. There is also significant nss SO4 in the coarse mode.

Pg 19647 line 19: I see little point in quoting the slope of the OM filter/AMS regression given the OM/OA of the filters is based on the AMS.

Pg 19649 lines 19-21: "concentrations of air ions". This implies that ions charged naturally in the atmosphere were measured. Is this so? There is no reference to this in the list of instruments. If you mean UF particles you should say so.

Pg 19650 line 21: The authors suggest that OB-OA is converted rapidly to OA by implying that the sources are near to the sample site. However, the paragraph beginning on line 11 of Page 19653 clearly states that this is not necessarily the case. The arguments need to be mad to be consistent – I see no evidence for rapid conversion of OBOA to OOA from the data.

Pg 19654 line 22: "highly correlated" The R2 values do not imply a high degree of

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correlation, I suggest removing "highly".

Pg 19657: top paragraph: Was mz 28 excluded from the PMF? No mention is made but the mz 28 in the figure is derived from mz 44 in the fragmentation table as far as I understand it.

Pg 19673: It would be good to show a shaded region representing the range of particle size distributions observed - possibly using the standard deviation of the number at each size.

Pg 19675: The mz 28 is equal to mz 44 because it is set to be equal in the fragmentation table. This needs to be made clear.