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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic aerosol evolution and transport observed at Mt. Cimone (2165 m a.s.l.), Italy, during the PEGASOS campaign" *by* M. Rinaldi et al.

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The manuscript presents measurements of aerosol chemical composition from a mountain location above the Po Valley, Italy. As a result, the site is influenced by both the local heavily polluted air in the valley below and more aged material from long-range transport within the free troposphere. The aerosol chemical composition is dominated by organic material and the authors focus primarily on this component, which is of great interest within the aerosol community and well within the scope of ACP.

My main comment regarding the manuscript is that the authors need to make a more convincing case that some of the stated differences in the organic component are statistically significant. This is particularly the case with regards to the elemental ratios. The information presented thus far implies that there is a large degree of variation in the OA component properties and it is not clear that the differences are significant (particularly for the OOAa and OOAb cases). Some additional analysis and perhaps some additional supplementary figures are therefore necessary. Please see the specific notes below.

Other than the above, one general comment I have is with regards to the naming of the OOA PMF factors: could a more descriptive nomenclature not be used? Adding 'a','b' and 'c' to OOA does not help in describing or understanding the analysis. It feels like every time a new AMS paper comes out with some PMF analysis, we introduce yet another naming method.

More specific comments are detailed below.

Section 2.2: Regarding changes in particle velocity within the AMS and given the pressure conditions at Mt. Cimone are approximately 800hPa based on Fig. S2, the authors should at least mention that this will result in a systematic change in the particle time-of-flight size distributions reported. Compared to sea-level, a reduction in atmospheric pressure to approximately 800hPa would reduce the particle velocity on the order of 10% in my experience. I would encourage the authors to report the results of any velocity calibrations that they have performed on their AMS (ideally this should have been done around the time of the experiment), so that the potential bias is reported.

In general, further information regarding calibrations (particularly relating to ionisation efficiency) and filter tests should be reported also.

Information on the sampling inlets, drying etc should also be included.

P14406, L18: 'measurement' should be 'measurements'

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P14410, L7: what do the +/- values refer to? Presumably one standard deviation? Please note in text.

P14411, L28: Rephrase sentence here relating to 'being the atmosphere. . .'

P14414, L6: Presuming the +/- values refer to one standard deviation, the variation here is very large at night. Is this driven by a few isolated episodes and/or very low values of NOx? Some additional figures in the supplementary to describe these trends and ratios would be useful. At present, it isn't clear how significant the differences are between the day and night given the large variability indicated by the standard deviation.

P14414, L24: replace 'resulted less' with 'resulted in less' or similar alternative.

P14414, L24: Missing 'O' in 'OM:OC'.

P14414, L23-28: Are these differences significant given the quoted variability and uncertainties in the determination of elemental ratios? I have my doubts on this and a more detailed discussion is required here. The answer to this point will impact the rest of the manuscript also, so any changes should be reflected later in the manuscript.

P14415, L 10-12: The language relating to 'direct sources of low oxidized OA' isn't clear to me here. Usually, direct sources would refer to primary OA i.e. directly emitted material, while oxidized OA is typically assumed to represent secondary OA. Some rephrasing here is necessary to improve the clarity of the explanation. Do the authors mean that the OA at the site has usually undergone significant atmospheric processing and consequently does not resemble recently formed secondary material?

P14415,L24-P14416,L3: This section would be more convincing with a little extra analysis regarding the size distribution peaks. Looking at Fig. 7, it looks like the peaks are all very similar and could be determined by e.g. a log-normal fit, rather than giving a fairly wide size range of 300-350nm in the text.

P14420, L15-18: This statement regarding heterogeneous reactions would be more

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convincing if the analysis suggested in the previous comment is undertaken. I don't disagree with the authors interpretation based on Fig. 7 but I'm not convinced that the AMS size distributions have the required sensitivity to rule out organic mass production based on the size distribution analysis as currently described in the text.

Fig. 5: As mentioned above, I have my doubts as to whether the differences in the averages for each air mass are different. Further discussion and analysis is required in relation to this. Also, I would suggest renaming the 'avrg_***' items in the figure legend.

Fig. 10: I would suggest using a more colour-blind friendly scale for these plots. Igor has several alternatives to the rainbow palette or see ColorBrewer.org for a wide selection of alternatives.

Supplementary material The authors state that there were not significant changes in the mass spectra and time series for different fPeak values in the PMF analysis - it would be useful to illustrate this in the supplement with either some additional figures or tables e.g. the correlations between the various factors and different fPeak solutions could be explored. By definition, changing the fPeak should alter the factor profiles and time series, so some evaluation of these changes would be useful and would give a bound on the use of 'significant'.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14403, 2015.