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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biogenic and biomass burning organic aerosol in a boreal forest at Hyytiälä, Finland, during HUMPPA-COPEC 2010" *by* A. L. Corrigan et al.

Anonymous Referee #2

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General comments

This manuscript presents compact time-of-flight mass spectrometer (C-ToF-AMS) and Fourier transform infrared (FTIR) spectroscopy data from the HUMPPA-COPEC campaign in Hyytiälä, Finland, during July and August 2010. Different statistical analyses (Positive Matrix Factorization (PMF), Potential source contribution functions (PSCF), clustering) identified biomass burning and biogenic emissions to be the dominant sources of submicron organic aerosol (OA) in Hyytiälä during summer time. The paper addresses important scientific questions related to OA, namely chemical specification and source contributions (from two globally significant sources such as biomass burn-

ing and biogenic volatile organic compound (BVOC) oxidation products). One of my main points of criticism is the interpretation of AMS PMF factor OOA-2 (see specific comments below). In general, the manuscript is well written and the analysis and presentation of the results including the graphs are done in a careful manner; however the paper would profit from a bit of streamlining. I recommend publication in ACP after the comments below have been addressed.

Specific comments

P. 16155, Line 1-10: Compare also "Minguillón, M. C., et al.: Fossil versus contemporary sources of fine elemental and organic carbonaceous particulate matter during the DAURE campaign in Northeast Spain, Atmos. Chem. Phys., 11, 12067-12084, doi:10.5194/acp-11-12067-2011, 2011".

P. 16158, Line 14: The AMS time resolution can also be higher and depends on the mass spectrometer. If 5 min was the time resolution of the data presented here then this should be stated explicitly.

P. 16158, Line 15 – 18: Please give also information on the resolution of the C-ToF-AMS

P. 16158, Line 24: Define DMPS. Was the comparison of AMS and DMPS done including black carbon data?

P. 16159, Line 27: Was there no off-gassing from the HEPA filter observed?

P. 16160, Line 22 -23: Please specify what is meant by "baselined" spectra.

P. 16162, Line 22: How do you explain the higher sulfate fraction during the sawmill events? And didn't you state on P. 16157, Line 20 – 22 that organosulfate was below LOQ?

P. 16163, Line 24 – 26: The AMS CE was established earlier to be 0.43 for all AMS species, a not particularly high value. The authors suggest here that one of the reasons

for the strong agreement between FTIR OM and AMS OM is a high AMS OM CE. This seems contradictive.

P. 16164, Line 16 -18: This statement is not necessarily true. Even in cities other OA types than HOA can be more important (compare e. g. “Jimenez, J. L., et al. Evolution of organic aerosols in the atmosphere. Science 2009, 326, (5959), 1525-1529”).

P. 16164, Section 321: It would be helpful for the reader if the clusters were given names. On P. 16166, Line 6, I cannot see how this very strict conclusion – that there was no sawmill activity identified in FTIR clusters – can be drawn. Cluster 1-T is clearly connected to sawmill activities, and Cluster 1-T and 1-IR are mentioned to be overlapping. And couldn’t the high carboxylic acid fraction of 1-IR hint at sawmill activities? Are the mentioned high NOx values grouped into Cluster 1-T related to traffic to/from/at the sawmill?

P. 161167, Line 10 -22: It would be helpful if the percentage of FFC2_FTIR was stated as well.

P. 16170, Section 3.2.3: It would be helpful if the percentages of the AMS PMF factors of AMS OM were stated.

P. 16172, Line 16 – 18. I cannot really see a high mass fragment 57 in OOA-2. Do the authors mean 67? But more importantly, m/z 55 and m/z 57 are commonly tracer mass fragments for HOA (and, more recently, COA, and thus primary) and not OOA (see e. g. “Zhang, Q., et al. Deconvolution and quantification of hydrocarbon-like and oxygenated organic aerosols based on aerosol mass spectrometry. Environ. Sci. Technol. 2005, 39, (13), 4938-4952”). Also from looking at OOA-2, and its correlation with AMS nitrate, it seems to me this PMF factor might rather be interpreted as HOA-related, or OOA/SOA related to fossil fuel emissions.

P. 16173, Section 3.2.4: Whereas the identification of biomass burning and biogenic emissions OM is supported by both AMS and FTIR spectroscopy finding similar results,

I am less convinced about the interpretation of AMS PMF factor OOA-2. Apart from the reasons stated in my previous comment – FTIR identifies 40% of OM as of fossil fuel origin (FFC1_FTIR and FFC2_FTIR). The AMS factors, however, are all interpreted as OOA, even though HOA is usually a PMF factor that is most likely to be identified in AMS PMF analyses. How can this discrepancy be explained? Is it possible that OOA-2 might also be related to coal burning emissions? Statements on P. 16174, Line14 – 23 concerning SOx emitting regions would support that theory.

P. 16178, Line 3: Are there really no FTIR spectra of emissions from burning vegetation growing in the boreal forest?

Technical corrections

P. 16151, affiliations: “Lausanne” is spelt with 1 s and 2 n

P. 16203, Fig. 2: Has a box on the x-axes of top panels C) and D)

P. 16205, Fig. 4: AMS PMF spectra have a wrong y-axis label

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16151, 2013.