

## ***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 #1**

Received and published: 17 August 2013

Overall Comment and Recommendation:

This manuscript examines the sources of submicron organic aerosol collected during summer 2010 in Hyytiälä, Finland, by applying positive matrix factorization (PMF) to Fourier transform infrared (FTIR) spectroscopy and aerosol mass spectrometry (AMS) datasets. FTIR requires the collection of integrated filters (6, 12, 24 hours) and AMS is a real-time analysis technique. From the PMF of FTIR data, the authors find 4 factors, including a biogenic SOA factor (BOA-FTIR), biomass burning factor (BB-FTIR), and two fossil fuel combustion factors (FFC1-FTIR and FFC2-FTIR). From the AMS PMF, authors assign the following designations for the 3 factors they resolve: OOA-

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1a, OOA-1b, and OOA-2. From these analyses, the authors identify the two largest sources being related to biogenic aerosol collected from a local forest and biomass burning aerosol from long range transport. The authors do a careful job in comparing their PMF results to collocated data sets. If I had to gripe about one thing, that would be the lack of SOA and POA tracer data. For example, the EPA group (i.e., Kleindienst et al., 2007, Atmos. Environ) analyzes filters by GC/MS with prior derivatization for known SOA tracers from isoprene, monoterpene, sesquiterpene, and aromatic oxidations. POA tracers could also be measured from similar filters. Tracer data would provide more support for the PMF results. I do think that FTIR and AMS are not very specific techniques, but in combination with tracer data, this would have provided more powerful approach for their source apportionment. With that said, I realize this was likely not available for this campaign due to a number of possible reasons and shouldn't be required for this manuscript. However, I think the authors should acknowledge that these tracers could have been helpful. Overall, this is a well written and interesting manuscript that carefully guides the reader through how the source apportionment was conducted. I recommend that this manuscript be accepted with minor revisions (suggestions) noted below.

Minor Suggestions:

1.) Introduction, Page 16155, Lines 13-15: The authors say that mass fragments from AMS are not specific to biogenically influenced OOA factors. I don't think this is necessarily true anymore based on several recent studies by Robinson et al. (2010, ACP), Slowik et al. (2010, ACP), and Budisulistiorini et al. (2013, ES&T). These three recent studies have identified a factor with a distinct fragment ion at  $m/z$  82 that is directly associated with isoprene SOA. Lin et al. (2012, ES&T) showed that this  $m/z$  82 fragment ion was due to the dehydration of isoprene epoxydiol (IEPOX)-derived SOA constituents in the AMS by using authentic standards and generating IEPOX-derived SOA and sending it into a HR-AMS instrument. Interestingly, all 3 studies showed that this isoprene SOA factor (more recently called the IEPOX-OA factor by Budisulistiorini

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et al. (2013)), increased with increasing sulfate, suggesting a potential anthropogenic influence on isoprene SOA formation.

2.) Out of curiosity, were the authors not interested in examining the anthropogenic influences on biogenic SOA formation with their dataset? I'm guessing this might be addressed in a followup study? This study was clearly more focused on the details of how the source apportionment was conducted and what was identified from this source apportionment.

3.) Introduction, page 16155, Line 4: Citations are needed for this sentence.

4.) FTIR analyses and organosulfates: Are the authors familiar with the Claeys et al. work from Hyytiälä? They have shown from PM<sub>2.5</sub> samples analyzed by LC/ESI-MS techniques that there are numerous organosulfates from monoterpenes and isoprene. This would suggest that FTIR isn't sensitive enough to resolve these compounds. Also, I think there remains some question how other components in the aerosol mixture affect the FTIR's ability to resolve organosulfates well and quantitatively.

5.) Page 16158, Line 24: You didn't define DMPS. Please define this acronym here. How accurate is the CE determination from AMS vs. DMPS comparison? I think you need to provide more detail here on how exactly this was done. Furthermore, for non-neutralized CE determination, I'm curious as to why the recent Middlebrook et al. (2012, *Aerosol Sci. Technol.*) composition dependent CE approach wasn't used?

6.) Have the authors considered comparing factor 4 from FTIR PMF with biogenic SOA tracers (e.g., Kleindienst et al., 2007)?

7.) I'm curious, how did monoterpene oxidation products from the PTR-MS compare to your factor analysis? You don't mention any of these products but mention isoprene products (e.g., MVK + MACR)?

8.) PMF Factor Designation from AMS: Why is OOA-1a, OOA-1b, and OOA-2 used for factor description? From the Jimenez et al. (2009, *Science*) paper, I thought the

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community was now moving to LV-OOA, SV-OOA, and BBOA designations. This makes things a little confusing for readers who are outside of the AMS community. Could the OOA-1a be simply called a biogenic OA factor and the OOA-1b called the biomass burning factor? I realize that the authors had a hard time pulling apart biogenic and biomass burning OA from each other, but this would be an example (I think) where molecular tracers for SOA and POA would help. To be picky, couldn't OOA-1 and OOA-2 be labelled LV-OOA and SV-OOA, respectively?

9.) It isn't clear how the STXM-NEXAFS data really add to the discussion in this manuscript. Also, since the majority of particles analyzed with this technique are between 1-3 microns, how comparable is this data to the AMS and FTIR data (which are collected as PM<sub>1</sub>)?

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

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