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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and seasonal variations of fine particle water-soluble organic carbon (WSOC) over the Southeastern United States: implications for secondary organic aerosol formation" *by* X. Zhang et al.

Anonymous Referee #1

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Summary: This study reports the concentration of water-soluble organic carbon (WSOC) and sulfate in 900 24-hr PM2.5 samples from 15 sites in the SE USA. The main conclusion is that these secondary aerosols are spatially homogeneous throughout the year, once samples with [levoglucosan] > 50 ng/m3 were removed. While these results are not surprising and represent only an incremental increase in our knowledge of secondary aerosols in the SE USA, the extensive size of the data set supports the main conclusion to a greater extent than a single field campaign could. The presentation is excellent and the topic is of interest to ACP readers. I recommend publication

after the following comments are addressed.

Major points: P9623, L10-11: "suggesting the temperature effects were mainly on the photochemical processes" - Are you saying that faster photochemistry is more important that synoptic meteorology in causing the correlation between temperature and SOA? If so, I think this statement needs to be supported. I also think it is likely that the relationship between temperature and SOA is driven by temperature dependence of BVOC emissions as well as photochemistry, and no clear evidence has been presented to separate those two effects.

Minor points: P9627, L6-8: You mention that the CSN filters were not adjusted for sampling artifacts, but what about the FRM filters? As written, the implication is that the FRM filters were adjusted.

P9628, L17: What was the time resolution of the PILS samples? Also, was a PM2.5 impactor used? How about a denuder?

Section 3.2.1 - could be condensed to one paragraph (as written, both paragraphs have the same conclusion)

P9637, L1-3: What about other biogenic compounds (besides isoprene)? This comment applies to the manuscript in general - is the qualitative relationship show for isoprene not seen, e.g., for monoterpenes? Do the authors believe that isoprene is the dominant WSOC SOA precursor throughout the region?

P9639, L1-3: Could there be a possible artifact associated with the PILS? For example could there be gas-phase WSOC that is absorbed into droplets or other liquid water in the instrument?

P9639, L7-17: This paragraph is a nice summary of previous work, but seems out of place in the results section of this manuscript. I would recommend omitting, or transferring to the introduction.

P9639, L19-21: Please give some details on the chemical composition used in the

PMF analysis - if there are too many to list, at least mention the number of inorganic compounds and the type of organic analyses (OC, EC, WSOC, specific compounds, etc.).

Section 3.4 - was PMF factor 3 correlated to RH? This could be additional evidence that it arises from aqueous processing.

P9642, L2 - The use of the word “non-volatile” to describe the WSOC is confusing - it implies that the samples were subjected to heating and/or dilution. Please clarify.

Figure 3 is unnecessary - and in a sense confusing because it is the spatial correlation (not the correlation to PM2.5) that I think is the most important contribution of this manuscript. I would omit, and shorten the discussion in the text.

I also find Figure 9 unnecessary - perhaps it could go in the SI, but it seems too minor a point to include in the main portion of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9621, 2012.