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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal characterization of submicron aerosol chemical composition and organic aerosol sources in the southeastern United States: Atlanta, Georgia and Look Rock, Tennessee" *by* S. H. Budisulistiorini et al.

Anonymous Referee #3

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This manuscript reports the real-time characterization of non-refractory submicron aerosol in an urban and rural site in SE USA using the ACSM. Source apportionments of the organic aerosol were made at each site during different seasons. The results presented in this manuscript fall within the scope of this journal, but major revisions need to be made before this manuscript can be accepted.

General comments:

1. There are numerous grammatical and punctuation errors in this manuscript. This needs to be corrected. The inappropriate use of semi-colons in certain sentences in this manuscript makes it particularly difficult to understand the message that the authors are trying to convey.

2. The figures need to be bigger and clearer. Currently, it is hard to make comparisons between datasets obtained at the two sites. I suggest making separate figures for the two sites (especially Fig 2).

3. This is a two year study. This needs to be stated more clearly in the discussion section, and kept in mind when the authors discuss their results. Currently, the discussion reads like it is a direct comparison between the two sites during the same time period.

Specific comments:

Pg 22384 line 20: More details need to be provided on the two sites. For example, how far are the power plants away from the measurement sites? (especially since this is later discussed in the discussion)

Pg 22386 line 16: Please specify the months that are classified as spring, summer, fall and winter. A more detailed explanation on the choice of factors is also needed. This can be placed in the supporting information.

Pg 22387 line 6: More details on the parameters used in ISORROPIA is needed. For example, was forward or reverse mode used and why?

Pg 22387 line 16: Do you mean Eq. (1) in Budisulistiorini et al. (2015)?

Pg 22389 line 16-17: A brief description of the method used by Guo et al. is needed here, in order to better understand the limitations of the authors' calculation of aerosol pH and liquid water content.

Pg 22389 line 21: "pH prediction using ISORROPIA-II based on inorganic ions alone was found to give a reasonable estimate" Are you referring to the study by Guo et al.

ACPD
15, C9006–C9010, 2015

(2015) or this study? How do the findings reported by Guo et al. (2015) apply to your study? This needs to be explained more clearly.

Pg 22389 line 22-25: "Hence, the lack of correlation in this study between OA and LWC based only on inorganic water suggests that LWC is not a limiting factor in OA production in this region." How did the authors draw this conclusion from the previous statement? Please explain.

Pg 22390 line 21: r2 = 0.2-0.5 is a low correlation, not moderate. Also, the authors need to specify in the manuscript what they mean by moderate and high correlation.

Pg 22391 line 12: The paper by Henry and Donahue (2012) is a chamber study where the normalization of organics to sulfate is used to account for wall loss. The authors need to justify more clearly why the same methodology can be used in this field study.

Pg 22392 line 1-3: "It is noted here that we acknowledge the potential role of diurnal PBL dynamics or loss processes (e.g. deposition) in contributing to diurnal patterns observed here for the PMF factors." This needs to be stated earlier in the discussion, specifically before the discussion of the PMF factors.

Pg 22397 line 22: I do not agree with the authors' suggestion that m/z 75 can be used as a marker ion for IEPOX-OA. This mass peak does not appear to be significant in the IEPOX-OA mass spectra shown by the authors (in Fig. 2) and Budisulistiorini et al. (2013).

Pg 22398 line 24: I do not agree with the authors' assessment that 91Fac, which is characterized by a prominent mass peak at m/z 91, is attributed to isoprene chemistry. First, given that the authors have an ACSM with unit mass resolution, the m/z 91 fragment ion in their mass spectra may not necessarily be C3H7O+, as strongly implied by the authors. Second, the chamber study by Surrattt et al. (2006), which was cited by the authors to justify their discussion that the m/z 91 fragment ion correlates with isoprene chemistry, was not performed using a high resolution ToF-AMS. Consequently,

ACPD

15, C9006–C9010, 2015

the m/z 91 fragment ion was only suggested to be C3H7O+ in that paper. Third, high resolution ToF-AMS data collected by Xu et al. (2015) at different rural and urban sites in SE US, which includes data collected at JST, showed that the m/z 91 fragment ion is primarily a CxHy fragment in all the mass spectra. Fourth, the m/z 91 fragment ion may be C7H7+, a marker ion for SOA formed from monoterpene chemistry (Boyd et al. 2015). While the authors acknowledge this possibility at the end of this section, this needs to be brought up earlier in this discussion section and the authors need to be more circumspect when correlating the 91Fac to isoprene chemistry.

Fig. 2: Please separate the JST and LRK datasets into individual figures. It is currently hard to understand. This is especially the case for 91Fac from the LRK site since its color is similar to 91Fac from the JST site.

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ACPD

15, C9006–C9010, 2015

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