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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol characteristics at the Southern Great Plains site during the HI-SCALE campaign" *by* Jiumeng Liu et al.

Anonymous Referee #2

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The paper by Liu et al. summarizes aerosol composition measurements during the HI-SCALE project in Oklahoma. Measurements of aerosol composition and trace gases were carried out using a high-resolution AMS and PTR-MS. HYSPLIT backtrajectories were used for source region characterization and positive matrix factorization was used to categorize OA into distinct factors. Since the campaign included a spring and summer segment, authors could contrast the seasonal composition differences. They conclude that overall biogenic VOCs seem to control OA formation. Case studies were explored in more detail to investigate OA formation from biomass burning and isoprene oxidation. Overall the paper is well written and fits the scope of ACP. Discussed seasonality in OA composition and formation pathways are interesting. There are some issues that need further clarification, especially in the assignment of PMF factors and

interpretation of those factors. Details are highlighted below.

1. There are many average values presented throughout the paper; it will be useful to also report the standard deviation of the averages to get a better understanding of the variability in the data. 2. L113: How was the AMS bounce fraction corrected for? Was SMPS data used solely for this instead of composition-based CE? If so, how were differences in upper size cut of the two instruments and density considered? 3. L180-181: what could the source of sea salt be at a region so far from the oceans? The majority of these particles will not make it through long-range transport? 4. L198: Another reason for low concentration of aromatics is their higher reactivity during the summer. This should be considered as a possibility. 5. Last paragraph in section 3.2: I find the discussion on oxidation state as measured by O/C, OSc and f44 circular. All of these metrics are different ways to show the same thing and primarily follow the f44 patterns, so the fact that O/C and OSc in spring were higher than in summer and then f44 in spring was also higher is not an additional evidence for increased oxidation state. 6. L245-250: BBOA factor: There's some enhancement in acetonitrile (as shown in SI) during the BB event in Spring. What is the correlation for the time series of BBOA and acetonitrile shown in Fig. S11. Furthermore, peaks in CO seem to correlate with peaks in BBOA factor; I wonder if correlation can be looked at for a subset of times (say when BBOA factor is higher than a certain amount or the time shown in S11) to have an external verification of the BBOA factor assignment. What was f73 in the BBOA factor? Was the fraction highest in the BBOA factor? If so, then it's circular to identify a factor based on f60 and f73 and show the great correlation of the factor with individual tracers at m/z 60 and 73. 7. Lines 251-261: the poor correlation between HOA and CO could also mean that CO has other sources. Given the influence of biogenic emissions, it's likely that secondary CO production is also contributing to the observed CO concentrations, which would certainly not be correlated with HOA. How is CO correlation with some of the BVOC tracers? 8. L259-261: I'm curious if the HR data of the HOA factor includes only the CxHy+ type of ions. Was the PMF run with the HR spectra or UMR? Given the use of HR-AMS, I assume the former although

there's no evidence of HR-type of PMF results. If HR spectra were not used in PMF, please explain why not. 9. L284-286: I thought OOA-1 is always the more oxidized type. The separation of OOA factors don't fully make sense to me. OOA-1 in Spring has a higher contribution of m/z 55, 57, etc, and lower f44, yet its OSc is higher than OOA-2. In summer, OOA-1 has a higher contribution of m/z 29, 43, 55, 57, similar f44 and lower OSc. It's confusing that in one season OOA-1 is more oxidized and in the other season it's OOA-2. Could it be that the two names are swapped (or one is a typo here)? Could it be that the OOA factors are not 'cleanly' separate from one another? 10. L429-430: If the lifetime of levoglucosan is 2 days, I don't think a significant amount of it would have decayed during a 6 hr transport time (remaining concentration=exp(-6/24) C0=0.88 of initial concentration). Please clarify. Perhaps you mean the transport time from Canada is longer, in which case that time should be noted here and not 6 hrs.

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