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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol measurements at a high elevation site: composition, size, and cloud condensation nuclei activity" *by* B. Friedman et al.

Anonymous Referee #2

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The authors measure CCN concentrations, single particle composition and particle size distributions of aerosol sampled at the Storm Peak Laboratory field site. The data set is grouped by different sources; biomass burning, organics, soot, and sea-salt. The authors find that sulfate which dominates the composition plays a large role in determining the overall hygroscopicity . New Particle Events, modify the particle number, and thus change CCN concentration.The article is appropriate for ACP. The title aptly describes the article content. The language and writing are straight forward. Concerns about the article are listed below.

Concerns.

P18285 L 24. How is POA defined? What instrument is used to define the amount of unsaturated hydrocarbons? Is Unit mass resolution data used to define this property? And at which peak? Or is it multiple likely unsaturated peaks? The analysis and identification of this fraction is not clear.

P18284. L25 The authors write, "CCN concentrations follow the overall aerosol particle concentration, with high variability at higher supersaturation, as smaller particles are able to activate. The two concepts in this sentence are somewhat confusing and opposing. As written, the sentence would suggest that there is higher variability in the correlation of CCN concentrations to total particle number at high supersaturations. But at higher supersaturations the ratio of CCN to CN should become closer to one and CCN will follow CN closely. Hence, why variability at high supersaturation? Perhaps the authors could clarify.

If I understand correctly, the authors define their CCN activity by the ratio of CCN measured with the DMT CCN counter divided by the number of particles greater than 80nm (P18286). This definition is problematic and somewhat misleading. By P18289 the authors acknowledge the issues and state "we hypothesize that particles smaller than 80 nm are able to act as CCN". I would suggest it is not a hypothesis but a reality skewing the interpretation of CCN activity results. One can do a quick back of the envelope calculation, let's assume the particle is entirely sulfate then at 0.12, 0.22, and 0.42% supersaturation the d_crit is somewhere near 90, 75, and 50 nm. If sulfate is a CCN activity driver at this site then particles below 80nm will be counted as CCN. Why not use total counts from the SMPS? Even though the SPLAT cannot see below 80nm, the activity of the entire distribution of particles is just as important (especially since the CCN counter is activating particles in this range).

Furthermore, as a reader the importance of this developed fraction is not clear. If the authors wish to continue publishing this fraction, I would suggest including a paragraph emphasizing that it is difficult to use Splat II mixing state above 80nm to infer CCN activation properties below 80nm. I would also suggest explaining the utility of this

fraction versus conventional total CCN counts.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18277, 2013.