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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud condensation nuclei activity of fresh primary and aged biomass burning aerosol" *by* G. J. Engelhart et al.

Anonymous Referee #2

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Engelhardt et al. present CCN experiments on fresh and diluted photochemically aged biomass burning aerosol. The main finding of the study is that after aging variability of chemical composition is decreased, thereby simplifying the modeling their impact on clouds and climate.

The experiments are novel, data appear to be of high quality, the manuscript is well-written, and the results are relevant to the community. I therefore recommend publication in ACP.

Comments:

I agree with the comments made by referee #1 and encourage the authors to address those. In addition:

I am missing a more quantitative comparison with prior results. If data are available for overlapping fuels, these should be added to Table 1. The main reason for this request is to begin understanding similarities and differences between studies (e.g. what is the variability of pine burns not only within, but between different methodologies). Adding such a comparison to the table will make it easier for future investigators to select fuels.

I am also missing discussion about heterogeneity within the CCN data. Previous studies showed that it is quite difficult to assign a single kappa value to an entire burn. It varies with particle size and the time/dilution history of the plume. Indeed this point seems to be one of the main motivations for this study. Yet Figure 2 assigns a single "primary" and "oxidized" kappa to the aerosol, with no regard to variability. The authors should not only add variability bars (also suggested by referee #2) but expand the manuscript to discuss metrics of variability. For example, Figure 5 could be a good starting point in computing a mean kappa, delta kappa, and show an increase and/or decrease in the delta kappa with time. In fact, Fig. 5 seems to show that variability within the plume increases 4 hours after lights on, even though variability between fuels decreases.

Another measure of heterogeneity to analyze would be the spread of the activation curves, of which none are shown. It would be useful if the authors added at least examples for each burn to a supplement.

In the manuscript the authors state that "analysis of the sigmoidal fit was carried out according to the method of Rose et al. (2008)". What does that mean? Does that include the fit to the data? Is a multiple charge inversion used? If so, was it done for both for the CN and CCN? If the method of Rose et al. was used, is it valid for heterogeneous samples and SMPS scans? If no inversion was used, how much does this increase the uncertainty? Please add instrumental parameters: e. g. sheath-to-aerosol flow ratios in the DMA and CCN and potential adjustments made to the data (e.g. if 100% activation was not reached due to the presence of non-activating particles), was the resulting activation spectrum scaled to one? If not, what does that mean for the 50% activation diameter? In general the methods/data analysis section should be expanded to help the reader understand what has been done specifically to this data set.

Much of the analysis is descriptive (regress quantity x vs. y) rather than predictive. Perhaps the data scant do not allow more quantitative analysis? E.g. based on prior correlations of O:C ratio vs. kappa and OA/inorganic fractions, can you predict the evolution of kappa values?

Eq. 2 should include “Dd” not “d”, “S” should be “Sc”, and T and sigmaw should be fixed to be consistent with the definition in Petters and Kreidenweis (2007).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7521, 2012.