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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Water Uptake by Fresh Indonesian Peat Burning Particles is Limited by Water Soluble Organic Matter" *by* Jing Chen et al.

Anonymous Referee #1

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Chen et al. use the HTDMA technique to study the water uptake properties of biomass burning particles from dried smoldering eagle fern and black wattle plants. Hygroscopic growth was correlated with chemical composition from an ACSM as well as OC/EC ratios, and WSOC content. Smoke collected on filters was extracted using water, and a liquid-liquid ocantol-water extraction. Extracts are atomized and analyzed for chemical composition and hygroscopicity. One of the main findings is a correlation between f44 and the mean kappa observed with the HTDMA.

The manuscript contains new data, collected with a valid set of techniques. The octanol-water extraction method is new. However, the direct utility of the study remains unclear. The authors do not explain the scientific value of analyzing water and octanol-water extracts. How can these be used to help understand biomass burning

aerosol? Yes, products will be different in the extracts, and it may help build correlations of kappa vs f44 over a wider dynamic range, but beyond that, I do not see how the extracts help understanding hygroscopic growth or help improving aerosol forcing estimates as claimed in the conclusion. The authors need to explicitly make this case in the discussion of the results. Furthermore, the correlation of kappa with f44 is now firmly established. Also, the water uptake properties for aerosol from predominately smoldering combustion in small burn settings is also well known to range between 0 and 0.1 from previous studies. Repeating this type of study with more fuels adds only incrementally to the known body of literature. Perhaps the NMR functional group data could be used better for an explanatory model? Overall, the manuscript needs to be revised to demonstrate how the presented data advance the scientific understanding of biomass burning aerosol (what new insight was learned and/or how it could be applied) and then re-reviewed.

Other comments

pg. 8: "In all cases, narrow monodisperse distributions were observed". This has to be evaluated against the width of a truly single component aerosol. The authors should compare the width of the distribution against some standard compound produced by atomization to support their point.

pg. 10: It is unclear why Eq. (2) is provided. The data in the Table 2 appear to be calculated from the data. For a mix of compounds, the Eq. (2) should be formulated for multiple components. Furthermore, Eq. (2) is only valid for infinitely soluble compounds. If the equation is used later, the relationship to solubility should be made clearer, especially in the context to the water and octanol extracts.

pg. 11: "the value of κ for acacia burning particles is similar to that was measured for WSOM extracted from a prescribed forest fire experiment in Georgia (USA) (κ = 0.10), which was estimated from a molar volume of 1.6 $\times$5 10-4 m3 mol -1 (Asa-Awuku et al., 2008)." And "The κ value of biomass burning WSOM separated by XAD-8 is esti-

mated as 0.29, using molar volume (6.2 $\times$ 10 -5 m 3 mol -1) estimated from a CCN measurement by Asa-Awuku et al. (2008)." It is unclear what is meant here. Did Asa-Awuku measure water uptake or CCN and compute kappa? Did they measure (average) molecular weight and kappa is calculated from that? If so, what is the relevance? Please explain.

pg. 12: (Lee et al., in preparation). I believe papers in preparation should not be cited in ACP articles.

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