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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measured and modeled humidification factors of fresh smoke particles from biomass burning: role of inorganic constituents" *by* J. L. Hand et al.

Anonymous Referee #3

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Summary

The study by Hand et al. presents a useful combination of measurements and modeling of hygroscopic properties of smoke aerosol originating from laboratory burns of fuels native mainly to the United States. The measurements suggest that particulate organic matter (POM) and light absorbing carbon (LAC) are not significantly hygroscopic. Modeled hygroscopic growth curves agree with the measured curves to within measurement uncertainty, which the authors use as a basis to suggest that POM and LAC are, for the most part, hydrophobic. Overall, the writing is concise and the methods are clear. I have some straight-forward suggestions below, which I consider to be

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in the category of minor revisions. As FLAME-related experiments continue, I certainly hope that the same methods presented in this study will be used to expand the number of data points in Figure 9.

General Comments

I am concerned that the authors did not address the possibility that some fraction of the POM is hydrophilic. When I read p. 4227, lines 12-15 or p. 4251, lines 20-24, for example, the logic sounds circular. There is evidence of water soluble organic aerosol from burning of woody savanna in southern Africa described by Gao et al (JGR, 108(D13), 8491, doi:10.1029/2002JD002324, 2003), and a lab analysis of the hygroscopicity of organic species by Chan et al (Environ. Sci. Technol., 39, 1555-1562, 2005) which offers evidence supporting analysis of fRH measurements from SAFARI-2000 described by Magi and Hobbs (J. Geophys. Res., 108(D13), 8495, doi:10.1029/2002JD002144, 2003). Based on Figure 9, the effect of POM on fRH in your study is probably small, but it would be interesting to know just how much POM could be modeled as hydrophilic and have the modeled growth curves still agree with measured growth curves. The reason I think this would be useful is that many chemical transport models and general circulation models simulate POM as partly hydrophilic. Since you are modeling hygroscopic behavior, I think you need to quantitatively comment on the possibility of hygroscopic organic aerosol, rather than assuming POM is hydrophobic, especially given statements like p. 4238, lines 13-15.

More comparisons with previous work should also be included. Day et al. 2006 (already cited) Table 2 lists mean fRH (RH = 71-94%) for sage brush and ponderosa pine of 1.39-1.76 and 1.25-1.95, respectively. Values listed in your Table 2 for sage brush and ponderosa pine are markedly different. Quinn et al. (GRL, 32, L22809, doi:10.1029/2005GL024322, 2005) showed how POM impacts hygroscopicity during 3 major field campaigns. The SCAR-B study of fRH by Kotchenruther and Hobbs (J. Geophys. Res., 103(D24), 32081-32089, 1998) suggested much less hygroscopic growth for biomass burning in South America than the SAFARI-2000 values discussed by Magi

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and Hobbs 2003 (above). Perhaps this was a result of higher percentage contribution of POM and LAC to the overall aerosol composition in South America as compared to Southern Africa (eg. compare Table 6 by Reid et al., J. Geophys. Res., 103(D24), 32059-32080, 1998, to Figure 2 by Magi, Atmos. Chem. Phys., 9, 7643-7655, 2009). Semeniuk et al 2007 (cited in your study) limited the analysis of hygroscopic behavior of SAFARI-2000 organics to organics mixed with inorganics.

Specific comments

1. p. 4227, lines 3-4: Somewhere in the Abstract, you should include the caveat that the fuels were limited to species from W and SE USA only (per line 24).

2. p. 4230, line 23: Delete ‘a’.

3. p. 4233, lines 13-24, Figure 1: I suggest that you delete Figure 1 and relevant text in this paragraph. There is nothing unusual in the comparison.

4. p. 4235, Section 2.3: Can you comment on how you overcame the limitation of DMPS size distribution being for diameters between 0.04-0.65 um and scattering measured for particles less than 2.5 um diameter? More to the point, when you integrate the DMPS size distribution with a refractive index to derive scattering using Equation 6, how closely does this match nephelometer scattering at low RH?

5. p. 4235, line 18: Do you mean ‘underestimation’?

6. p. 4238, line 14: Can you get your deliquescence and/or metastable curves to still agree with the measurements if some fraction of POM is hydrophilic?

7. Table 1: Thank you for stating your assumptions in this table. Please add ‘at a wavelength of 530 nm’ after ‘refractive index’ in the caption. A study worth noting is the recent review of LAC properties by Bond and Bergstrom (Aerosol Science and Technology, 40, 27-67, 2006), who suggest LAC refractive index is 1.95-0.79i and density is 1.7-1.9 g/cc. The Stelson (1990) study cited in your Table is more relevant to urban aerosol.

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Interactive Comment

8. Table 2: Entries 2, 3, 13, 14 all have inorganic/organic ratios of exactly 0.02. Is this correct?

9. Figures 2-6: I suggest that you change the scales on the y-axes – it is hard to glean anything from Figure 4, for example. I would also suggest changing the scales on the x-axes so that you are presenting the measured range of RH (roughly 20-90% seems adequate) Is it meaningful to show modeled fRH and GF beyond the measurement range?

10. Figure 9: I like this figure, but I would change the scale to 0-0.6 on x-axis, and 0.8-2.3 on y-axis to more effectively highlight the data points. How much would the best fit line change if you plotted fRH vs IMPROVE inorganics/POM instead of inorganics/(POM+BC)?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4225, 2010.

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