

Interactive comment on “Flight-based chemical characterization of biomass burning aerosols within two prescribed burn smoke plumes” by K. A. Pratt et al.

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Review to “Flight-based chemical characterization of biomass burning aerosols within two prescribed burn smoke plumes” by K. A. Pratt, S. M. Murphy, R. Subramanian, P. J. DeMott, G. L. Kok, T. Campos, D. C. Rogers, A. J. Prenni, A. J. Heymsfield, J. H. Seinfeld, and K. A. Prather

This paper presents very interesting in-situ data on the evolution of biomass burning particle in a very initial state after emission. It is well-written and the results are clearly explained (with some exceptions as detailed below). I therefore recommend publication after the following comments have been considered:

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Major Comments:

I have one major concern regarding the O/C ratios inferred from the C-ToF-AMS. The relationship between f44 (m/z 44 / total organics) and O/C from Aitken et al., 2008, (Equ. 1 in the present paper) was derived from HR-ToF-AMS data. My own experience from running a C-ToF parallel to a HR-ToF shows that the f44 values measured by a C-ToF are higher than those measured by the HR-ToF, and thereby the calculated O/C ratios may be too high. This may be due to higher fragmentation or due to lower ion transmission of larger m/z. The magnitude of this effect appears to be dependent on the actual tuning of the ion extractor and/or the mass spectrometer voltages. So, unless there is no experimental verification for the relationship between f44 and O:C from C-ToF data, I suggest to remove the statements on O:C ratios and report only the f44 values. Once the relation between f44 and O:C for the C-ToF has been established by laboratory measurements, the O:C values can easily be calculated later on.

Additionally, the effect of elevated gas-phase CO₂ (as mentioned at the end of section 2.3) may disturb the inferred O/C ratios. Can the delta CO₂ (not measured) be estimated from delta CO?

Minor Comments:

Section 3.2:

Page 17520, lines 4ff and Figure 4:

In our recent observations (Brands et al., AS&T, 2011), we detected Na⁺ along with K⁺ in biomass burning mass spectra, using the same ablation laser (266 nm). It amazes me that the data presented here don't show any Na⁺ signal.

Page 17521, Line 25 ff:

EC/BC particles smaller than 100 nm may not be detected by ATOFMS. Maybe there is a fraction of non-coated, small soot particles that are missed.

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Section 3.3.1:

Page 17532, line 9:

While I fully agree that aging and mixing increases the O:C ratio, I am skeptical about the absolute values, as already stated above. The reason that your O/C after 80 min is higher than Grieshop O/C after 5 h may be the above mentioned C-ToF-AMS effect. Grieshop used a Quad-AMS, I am not sure if f44 from Quad data are comparable to C-ToF data.

17524, line 10:

“=” misses

17525, line 15ff

Is m/z -71 only due to levoglucosan in the A-ATOFMS spectra? Or may there be other contributions from organic acids?

17526, line 7:

the sentence after “similarly...” refers to A-ATOFMS data, correct?

Sections 3.3.1 and 3.3.2 both end with: “. . .the behavior of . . . during the initial aging of biomass burning particles/aerosol is unclear . . . and requires further study.”

Section 3.3.3 and Conclusions:

Acidic particles? Is this result based on A-ATOFMS or on AMS data? From section 3.3.3 this was not clear to me. Maybe a plot of NH₄ vs. SO₄ + NO₃ (from AMS) would help to see whether sufficient NH₄ was present to neutralize the acids.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17507, 2011.

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