Atmos. Chem. Phys. Discuss., 15, C11622–C11625, 2016
www.atmos-chem-phys-discuss.net/15/C11622/2016/

Atmospheric Chemistry and Physics Discussions
Open Access

# *Interactive comment on* "Single particle characterization of biomass burning organic aerosol (BBOA): evidence for non-uniform mixing of high molecular weight organics and potassium" *by* A. K. Y. Lee et al.

**Anonymous Referee #1**

Received and published: 15 January 2016

General: This article covers measurements of a biomass burning plume sampled in Toronto using a state-of-the-art LS-SP-AMS, ATOFMS and a variety of other instrumentation. There are a few things that can use highlighting, discussion and clarification and I outline those items below. The experimental section is well-written and referenced. References to an unpublished paper (Willis et al 2015) covering details of an AMS factor analysis make it difficult for me to interpret the factor analysis results presented here. I therefore suggest that the authors present an overview of the PMF results in the supplementary section. The authors can do a better job of stating a clear

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

hypothesis – it is stated that the primary goal of this paper is to examine the mixing state of K, rBC, and BBOA – but why exactly? Also, the quantitative capabilities of the AMS measurement can use some more discussion. With a stated mass resolution of 2000 is it really possible to reliably quantify K mixing state? I know that O:C ratios are potentially quantified with AMS, is that possible here? The strengths of this paper include tying the properties of the different particle types to optical properties and volatility. Overall the paper is interesting and is worthy of publication in ACP and I commend the authors for a solid piece of work.

Specific: Abstract: it would be nice to have more background about the potential sources of the bboa given that these appear to be field measurements. There is no mention of the geographic region in the abstract. What sort of biomass burning plume is this? Is it representative of biomass burning plumes worldwide?

P. 32160, around line 10: There should be a better stated hypothesis here. Why is it so important to determine the mixing state of K and BBOA? What does it mean to optical and hygroscopic properties if they are internally or externally mixed?

P. 32160, lines 14-16: Awkward sentence

P. 32160, lines 16-19: Awkward sentence

P. 32163, line 16-21: It is stated that the PMF results will not be discussed here because they are discussed in Willis et al 2015. However, Willis et al 2015 is in review. This leaves me to conclude that the relevance of these results are dependent on how another paper is peer reviewed. I suggest that the authors work with Willis et al to provide a summary of the PMF analysis in the supplementary section of this paper. I have questions about other factors obtained via PMF (see below) and the reference of Willis et al is useless to me at this point.

P. 32165, line 16: Were the K+ and Org_rBC distributions obtained using the same instrument?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P. 32166, lines 1-4: Was this calculated only for rBC containing particles? This is confusing - I suggest the authors give more detail on this calculation and its interpretation in the supplementary section. It is confusing because the authors are invoking spherical morphology while using effective densities for fractal soot.

P. 32166, line 9-14: One should not need to solely rely on cluster analysis to tease out trends of HOA type aerosol. For example, the time series for hydrocarbon like OC marker peaks can be plotted. Does such a time series give the typical HOA temporal signature? Also, only the BBOA factor from the PMF analysis is shown. Was the PMF analysis able to extract an HOA factor? This would be a salient point of discussion.

P. 32166, Line 15: what do the percentages correspond to? Number % or mass % of particle type? Mass % of sulfate?

P. 32166, line 27: How was the potassium deconvoluted from the organics here? Was there enough resolution to distinguish between K and organic fragments? I think that some detail on this would be important for the interpretation of K in some of the other BBOA clusters where K may not be as abundant (but still present). If the authors could provide an example of a deconvolution of K peaks from organic peaks this would provide strong evidence of the quantitative capabilities for the technique.

P. 32167, Line 6: Is it possible that the particles having low K are from sources having low K. Another possibility is that the BBOA having low K is SOA. Please discuss.

P. 32167, Lines 16-18: One way to compare the two techniques is to compare the NUMBERS of biomass particles classified by ATOFMS and LS-SP-AMS. Was this attempted? If not, I suggest the authors look into this. Inevitably, the ATOFMS is much more sensitive to K and it is not clear how well K can be deconvoluted from organics for the AMS technique and thus the potassium signal may be below the AMS detection limit. Have any studies looked into the quantitative capabilities of the AMS for potassium for biomass burning aerosols? Discuss.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P. 32167, Line 25: is it possible to quantify O:C?

P. 32169, Lines 13-17: Is there any difference in volatility for the BBOA-K and the rest? Is it possible that the mixing state of the particles can affect the volatility of the particles?

P. 32170, Line 27: It is stated that the properties of the BBOA particles depends on burning conditions, but would also guess that this is also a strong function of source or fuel type as well. I suggest the authors consider that possibility.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32157, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper