

Interactive comment on “Measurement of size dependent single scattering albedo of fresh biomass burning aerosols using the extinction-minus-scattering technique with a combination of cavity ring-down spectroscopy and nephelometry” by Sujeeta Singh et al.

Anonymous Referee #1

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The authors present very valuable data, regarding the optical properties of BB aerosols. This data is of high importance to both modelers and experimentalists. The authors did a careful job in providing an accurate measurement of the SSA, based on their previous work Singh et al., 2014 . However, the optical properties of these particles should be measured in a meaningful way. The authors should have a clear characteristic for the measured particles, in order to compare their results to the literature, or make any statement about the SSA. While, the SSA measurements are

C1

carefully conducted, the conditions of the measurements are very poorly characterized. My recommendation, is to carefully address the comments listed below:

General comments:

1) The author should go over and recheck every single reference that they site, and make sure it is sited correctly. The introduction include at least 5 references which were sited either wrong or in a misleading way. P3 line 105-109: This sentence implies that, Riziq et al., 2007;Riziq et al., 2008 and Dinar et al., 2008 were using extinction minus scattering technique. Or alternatively measuring both extinction and scattering. These 3 papers where measuring only extinction using the CRD, and retrieved the scattering/abortion via Mie theory calculation. Same comment applies to the flowing 2 references Butler et al., 2007;Miller and Orr-Ewing, 2007. The authors claim that these papers measure the SSA of isolated aerosol particles. However, both the Butler et al., 2007 and the Miller and Orr-Ewing, 2007 were measuring/determining only the extinction of light by single aerosol particles.

Additionally, this paragraph is almost an exact copy of a paragraph in the group's previous paper (Singh at al., 2014 aerosol science and Technology, page 1345 last paragraph). Same comment for the paragraph stating at P4 line 132, is almost identical to a paragraph in p 1346 in Singh at al., 2014). The authors should address these issues.

P12 line 416-418: The Beyersdorf, 2013 reference is not a peer reviewed paper, this data was presented after the flight and represents very preliminary data from a meeting. The authors should remove this reference.

2) P6 lines 202-204: The authors state that, the samples were diluted and sonicated prior the introduction to CRDS and nephelometer, they also report that the size distribution changed after nebulization. Was this size distribution change a result of just atomizing problems, or is it a change in morphology or/and chemistry? The authors need to address this question as part of this paper's framework. The authors claim that their measurement would represent fresh soot that has undergone cloud processes,

C2

however sonication and atomization may or may not, change the soot's morphology. If the authors wish make this statement, it should be supported by measurement (e.g. electron microscopy). Chemical analysis is also required to make any statement about these measurement. Does these measurement represent coated particle or un-coated aerosols? The authors clearly state in p15 lines 537-538, that composition and morphology, have the most significant effect on fresh BB aerosols. While, these properties are indeed dependent on the burning stage particle size fuel type and condition; one needs to show that these properties do not change in water solution and atomization/solicator. The authors mention that a future work will be addressing changes in the mixing state and morphology, these issues should be addressed as part of the current manuscript.

3) When comparing to their results to literature values, the authors refer to the MCE as an explanation for agreement/disagreement with the literature values. For example: 1) p11 line 394, 2) P13 line 436. However, the MCE was not measured in this work. This makes the compression to literate be very speculative. Also how could the authors show difference between fuel types without making sure that similar MCE is shown for all cases? I agree with P.Pokhrel comment claiming that:” They concluded burn condition does not control the SSA and AAE and mention as a one of the major findings in abstract but have not tested SSA and AAE correlation with either MCE or BC/OA ratio’ The authors responded that they are currently working to address these important aspects of BB aerosols, including chemical analysis and optical properties as a function of aging using an indoor smog chamber in forthcoming work, this should be done (at least partially) as part of this manuscript framework

Same comment applies to the any of the possible explanations given in the last paragraph of p 13 (lensing, volatilize low molecular organics.) This explanations should be supported by some chemical analysis

Minor comments: 1) The author could address the multiple charged particles issue, by performing a Multiple charge corrections (see for example Flores et al.,2012 ACP)

C3

2) For all of the figures: please make sure all of the figure are consistent and clear. For example: The font size is different in every single figure. Figure 1: The drawing is cut on the left, figure 11: there is an axis on the right size (but other figure are open). Comparing figure 9 to 10: There have the same x –axis but one start with the actual number 500 and doesn't have minor ticks. Figure 11: the number 0.6 is cut, the legend is 'smooshed' etc. 3) Please provide an explanation to changes of the SSA with the particle size, for example: why is the Cedar smoldering 500nm has a lower SSA than the cedar smoldering 400nm?

Specific Comments: 1) Figure 4: This figure is very confusing and difficult to follow, Please make it more clear. 2) P8 line 255: Please change the period to comma. Avoid using 'and' after the period 3) P9 line 308: please add reference 4) P9 line 313: “particles that have the same electrical mobility, but different mobility diameters were separated” This sentence is not clear did the authors mean: same electrical mobility but different mass selection? 5) P9 lines 321-323: The authors state the SSA has a slope of zero over the range of 500-680nm, however the x-axis in the figure ends at 660nm. Please add the missing data to the figure. 6) P12 line 405: please change Lewis to Lewis et al., 7) P12 line 406: please add reference

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C4