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Atmospheric Chemistry and Physics Discussions

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 #2

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The manuscript deals with an interesting and tricky topic, providing very useful data and information on optical properties of BB-derived particles. I would like to congratulate the Authors for the huge quantity of work done and for the attention dedicated to solve all the technical aspects related to this kind of measurements. From the linguistic point of view, the paper is quite clear and well written. I see in this manuscript two main weaknesses. The first one regards two important lacks: the calculation of the Modified Combustion Efficiency (MCE) and the determination of EC and OC. In this field

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of study, this information is very useful since both influence the final optical properties of the particles. Although these lacks don't affect the goodness of the results, they make impossible a direct comparison between the data they show and the literature they cite, forcing the authors to a sort of speculation (as pointed out by the other Referees). In the Authors response to AC1 they state that "there are schemes that relate SSA and AAE to either MCE. . .the unknown, MCE or EC/(EC+OC), can be solved precisely" knowing SSA and AAE. But just few lines later they state "But what gives rise to differences in MCE? The authors state, in the manuscript, that it is influenced by fuel type, fuel state, and burning conditions". So, if the authors would calculate the MCE or EC/(EC+OC) values considering the schemes available in literature, they are assuming that fuel type, fuel state and burning conditions are the same in both the experiments. How it could be possible? The second one is related on the "distance" between the BB aerosols produced in the Authors "soot generation setup" and the particles they are measuring. They clearly state that particles changed in size distribution and morphology after the various processes of collection, sonication, nebulization. Also chemical composition changed both during preparation (partial removal of semi-volatile species) and during storage (moreover Authors do not determine chemical composition in any way). Although I agree with the authors that the particles they are measuring are likely more close to fresh than to aged BB aerosols (no photochemical transformation, no SOA formation), these particles are very different from the original ones. I wonder how much the optical properties shown in this paper are representative of real fresh BB particles. I think that the previous Referees have pointed out the crucial problems and I have no questions to add, except one: in Figg. 5-10 there is a clear point of discontinuity (especially in Figg. 6, 8 and 10) in correspondence of 580 nm: the values measured with the dye laser (< 580nm) are more similar for the different fuels while much more widespread in the case of the OPO laser (>580 nm). I have not find any comment in the text about this evident difference. The authors are aware of the limitations present in their work. I think that these limitations are well explained in the text and clear to the reader. The Authors should anyway include some integrations as suggested by the

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Referees. Overall, I consider this paper scientifically remarkable and complete.

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