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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.

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REFEREE COMMENTS: 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 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).

AUTHORS RESPONSE:

The authors admit that this additional information on MCE would be useful, and is being implemented in our future work, which is currently in progress. In this work the burning conditions will be highly controlled for temperature and oxygen content, allowing us to vary MCE. While no direct MCE- or EC/(EC+OC)-based comparisons are possible, the discussion uses qualitative comparisons of burn conditions. At this time, the only possibility of performing these measurements would be to return to the original fuel samples and measure their fire-integrated CO and CO2 values. However, even if those fuels were still there, they would have been sitting in the open for over a year. The fidelity of the samples would be very questionable.

REFEREE COMMENTS: 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?

AUTHORS RESPONSE:

The MCE- and EC/(EC+OC)-based schemes used data gathered during FLAME-4 experiments. In that work, a variety of fuels were burned under several conditions (mainly open burns and several types of cookstoves). Both of these factors are already varying, and the papers of Pokhrel et al. and Liu et al. attempt to find a robust fit for SSA and AAE as a function of MCE and EC/(EC+OC), respectively. This is a worthy goal, especially for use in modeling efforts. However, for some samples we have investigated, these trends have some deficiencies.

REFEREE COMMENTS: 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.

AUTHORS RESPONSE:

While it is possible that these samples have more in common with soot that has undergone processing in pyrogenic clouds, the authors are not aware of any such field measurements. Thus, putting our measurements in that context is not currently possible. The closest comparison would be fresh soot.

REFEREE COMMENTS: 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 found any comment in the text about this evident difference.

AUTHORS RESPONSE:

Two sets of mirrors used in this work and 580 nm marks the boundary between the ranges at which they are highly reflective. It does not denote the wavelength range of the two light sources. Due to differences in mirror reflectivity, differences in the error and level of noise are apparent in the different ranges. All the work was done

using OPO. The dye laser was not used for this work. The experimental section on the paper will include a sentence to show that only the OPO was used. Regarding the discontinuity at 580 nm, we already provided an explanation in the text Line 246-259. In response to comments by Rudra Pokhrel on the same issue, we provided the following explanation: “Our main reasoning for this was that data in the 580-660 nm had poorer S/N than data in the 500-580 nm range. This is due to the smaller reflectivity of the mirrors in that range. The values for extinction, scattering, and absorption cross sections were high in the 580-660 nm range for 300 nm particles and low for 400 nm particles but maintained the same slope. For the same-day run for both wavelength ranges, we found nearly the same values for the 400 nm particles. In all cases the SSA did not change significantly due to adjusting the extinction and scattering values. Measurements were done several times at different days and the results are consistent” It is also worth noting that the level of noise is not the same for different particle sizes, which is largely due to number density differences.

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

AUTHORS RESPONSE:

The authors would like to thank the referee for their kind remarks. We are unsure about what ‘integrations’ the reviewer is referring to. If the referee is suggesting that we integrate the comments offered by the other referees, we have already indicated how we intend to integrate their suggestions into the final text for publication.

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