

## ***Interactive comment on “Optical closure experiments for biomass smoke aerosols” by L. E. Mack et al.***

### **Anonymous Referee #1**

Received and published: 13 May 2010

Overall, this paper represents a great deal of work and will become a useful reference to our scientific community. Yet, the following major comments needs to be addressed for clarification of the results.

Minor comments:

Eliminate all acronyms in the abstract – FLAME and USFS

Line 17 should state (e.g. Ramanathan et al., 2007), page 7470

Between lines 13 and 15 equivalent sign should be replace by approximate sign, page 7472 ; also page 7474 line 14

The following sentence on page 7475 needs to be clarified:

“It is well known that particulate emissions vary considerably between the flaming and smoldering phases of combustion, and Reid et al. (2005) suggested relationships between the combustion conditions and the w of the smoke aerosol.”

The phrase “well-known” needs to be supported by references.

Further needs to be replaced with Furthermore on Page 7476, line 7

The statement on page 7477 regarding the properties of ammonium sulfate (nearly spherical) need to be supported with a reference

Page 7477 – statement by a factor of 0.884 needs to be clarified. 88% different? Or a 12% discrepancy?

Page 7478 – line 21, nonsphericity statement with regards to size, needs a reference.

Page 7479 – Need to cite the size distribution instrument used by Hand et al., 2010, was it also the OPC?

Page 7481 – need reference or further explanation after – high absorption lead to inaccurate OPC sizing. . .

Page 7481 – need reference on line 15, after ~630 nm are present.

Page 7484 – need reference on line 14 after smoke aerosol.

Major comments:

The SSA mean standard deviation is surprising low at 0.007. Are you sure of this calculation? I would definitely suggest the authors look again at this method for calculating the relative uncertainty in SSA measured. This is difficult to review, as there are no plots of SSA over time for the different species.

I agree with the previous comment that all Latin species names should be used for clarification. The term needlerush does not have meaning to me.

The 0.884 correction factor needs a further explanation. Why do you expect different

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losses of particles in the sampling system for the 2 instruments? Did the instruments have different lengths of sampling lines? You should provide a few calculations pertaining to particle losses with respect to this system. Could there be other reasons for the discrepancy?

In section 3.3, the uncertainty described for figure 2 should be carefully considered. All calculations were done for the upper and lower limits of the range of refractive index/density pairs as previously reported in the literature. Yet, these values in the literature are NOT absolute and are associated with a certain degree of uncertainty. This uncertainty was not included within this figure. Furthermore, there are no uncertainty bars on any of the points, including the measurements (maybe they are too small, but see comments above). This statement also propagates into figure 3, where the same methodology is used.

Finally, the comments from Paul Zieger should be considered carefully, as it represents a very good review.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7469, 2010.

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