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Interactive comment on "Optical and physical properties of aerosols in the boundary layer and free troposphere over the Amazon Basin during the biomass burning season" *by* D. Chand et al.

Anonymous Referee #2

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Review of acpd-2005-0084-1-0-0

Overall review: In this manuscript the authors present regressions of light scattering, absorption, CO and TEOM estimated mass at a surface site and on an aircraft for the LBA campaign. While I empathize with the authors on what they are trying to do and express, the paper is in a state where the findings are not easy to apply. Key information on the vertical profile of aerosol particles and CO is totally absent and we are left with a series of non-interpretable regressions. The two regressions with real meaning, the construct of the mass scattering and absorption efficiencies, are not well executed on a number of levels and most likely are biased. Most significantly, the TEOM is well known to underestimate organics species, particularly smoke. I suspect that their derived values of the mass scattering/absorption efficiencies are biased high by 10-30%. Gravimetry using samples off of the exact pipe from the nephelometer and PSAP is the only reasonable way for the mass efficiency measurements to be constructed.

While I have many criticisms, I see real promise in this data if presented properly. Although I do not knwo if the author's can make all of the changes in a timely manner. Specific issues are listed below.

Abstract and throughout: The usage of "Boundary layer (BL)" and other planetary boundary layer terms throughout the manuscript needs to be more specific. Authors should be specific as to what they mean, Planetary Boundary Layer (PBL), Surface Layer, Convective Boundary Layer (CBL), beneath the trade inversion, etc. It is not at all clear from the manuscript what the boundary layer dynamics they are referring too are. A plot of an ideal boundary layer structure would be very helpful.

Page 4377 line 8, Section 2.1, Line 5. "see level" should be "sea level"

Section 2.2 Use of "Dry air (RH$<$40%)" is not really dry. You need to get down below 30% to be considered dry. If you look at the collective works of Tang, and consider that the background RH for the region is fairly high, then particles will no doubt be on the upper hysteresis curve. Hence, there can be tightly bound water on the order of 10 to 20% of particle mass.

Section 2.2. It is fairly unclear as to which instruments have which cut-points. Similarly, instrumentation is presented in a hodge-podge manner and is difficult to follow. A table would be very helpful. Authors mention the SMPS but do not give any data. Why? it may help their case on a number of discussed topics.

Section 2.2 Use of the TEOM. While TEOMs are considered "usable by the EPA" in reality they tend to have serious biases, specifically with regards to organic aerosol particles such as smoke. By heating to 50oC, often semi-volatile organics are driven off. If drying for light scattering is done by perma tube, and for the TEOM by significant heating, then no doubt any measurement of the mass scattering efficiency will be biased high, at least 15-30%, and maybe more. Please see “Long-Term Field Characterization of Tapered Element Oscillating Microbalance and Modified Tapered Element Oscillating Microbalance Samplers in Urban and Rural New York State Locations, by Schwab et al., AWMA 2004” This problem has been known for some time, but this is the most recent discussion. I suspect that the very high mass scattering efficiencies that are found here are a result of this bias.

Section 2.3.1 Why did you make corrections to the radiance research nephelometer and not the TSI? Besides, as shown by Anderson lon-lambertian light source errors are much larger than truncation to begin with. The use of a RR neph at the surface and the TSI on the airborne platform constitutes a study design issue for the paper’s primary premise- comparing surface and airborne particle properties. How did the instruments compare in fly-bys?

Section 2.3.2 The authors argument with the Bond corrections is reasonable, but not well executed. First, at the very least the authors should state the difference in photo-acoustic and Bond corrections, especially of they are going to present data to three significant figures. Referencing a paper in preparation in this context does not help us reviewers. Also, Pat Arnott’s instrument is not a primary standard either.

Section 3.1 Discussion of Figure 3. Clearly there are two populations in this figure, with a slop changing at ˜120 ug m-3. These should be analyzed separately.

Section 3.1. Comparison to SCAR-B. The comparison is not really complete. First the SCAR-B measurements were made at ˜PM4 as opposed to the modified PM1.5 done here. This would in itself make a 20% change. Also in the SCAR-B campaign comparisons are made between aged and source optical properties. Aged values equivalent to PM1.5 would be ˜4.1 m2 g-1, compared to the 2.7 to 3.6 m2 g-1 given here. Besides, if you have a value for mass scattering efficiency that is higher than every other measurement in existence for all other places in the world, you may want to try and justify it. If the TEOM is off by 20%, well within its uncertainty for smoke particles, and an almost certain bias here, the measurements are in agreement. As for absorption, in their review paper Reid et al admits that the original integrating plate values for SCAR-B are biased low. But, even here the values do not quite reach the values presented here, even with the extinction cell. One thing that the authors may want to consider is that the particle concentrations for this study are higher than those in SCAR-B. This no doubt effects the evolution process that converges to larger particle size. What does the SMPS data suggest?

Figure 4. This is almost unreadable and impossible to follow. Such regressions offer very little context. How about a few vertical profiles? How are the authors defining the PBL? Thermodynamic soundings. "data for different flights is shown in different colors"? A key on the figure would be most helpful. Even so, in this context all that can be said is that the regressions are different at different levels and flights- no surprise there. This data needs to be analyzed in the larger context of large-scale meteorology. Reid et al., [1998] found the development of the surface mixed layer and the top of the CBL/trade inversions to be very complicated. Given the significant number of flights the authors should be in a position to add to this.

Section 3.2. I think a scatter plot of aircraft values of light scattering, CN, and CO, versus surface measurements at overpass is an absolute necessity. This is especially true considering the relationship between ground and airborne measurements is the point of the study.

Section 3.3. The development of the OSH is totally nonsensical, especially considering you have aircraft data! What are the vertical profiles of bscat? What do those tell you?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4373, 2005.