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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long term measurements of aerosol optical properties at a pristine forest site in Amazonia" *by* L. V. Rizzo et al.

Anonymous Referee #3

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General Comments:

This manuscript is a careful study of in situ aerosol properties over Amazonia. The Amazon is an important and understudied region for understanding radiative forcing by aerosols on a regional basis. I recommend publication of this manuscript after attention to the points discussed below.

Specific Comments:

Abstract:

Pg. 23335, Line 2 and 18: Pristine? May reconsider the use of the word 'pristine'.

On Pg. 23337, Lines 25-26, the authors state that this region 'is influenced by external aerosol sources like regional biomass burning, urban plumes and African dust advection.' And on Pg. 23338, Lines 9-10, they also state '. . . the site was affected by regional transport of pollutants, either from biomass burning or urban plumes.' This is not pristine; it is an occasionally perturbed site.

Line 5: 'major' classes of particles

Line 12: what is fine mode? Define here rather than on Pg. 23336, Line 1.

Line 16: replace 'particle' with 'aerosol'.

Line 27: replace 'advection' with 'advected'.

Pg. 23337, Line 8: replace 'focus' with 'focuses'.

Pg. 23338, Lines 12-15: Has any study of the size-dependent particle passing efficiency of the inlet been performed? An inlet of this length would be expected to show losses for small particles by diffusion. If small particles are preferentially lost, how does this affect the results for SSA and for the SMPS measurements? Also, no mention is made of the sampling lines to the instruments. These lines may not have laminar flow and other loss processes (e.g., gravitational settling or inertial losses) may come into play. Can the authors please comment on this? How severe are the losses at the small and large ends of the size distribution?

Pg. 23338, Line 24: replace 'Data was. . .' with 'Data were. . .'.

Pg. 23339, Lines 1-2: What are the average truncation corrections for wet and dry season aerosols? These particles would be expected to be of very different sizes and thus the magnitude of the truncation corrections should be different.

Pg. 23340, Lines 2-4: Was a comparison of the T and P sensors used in this study performed so that the conversion to STP conditions during the period the nephelometer was broken was consistent with the conversion during the other study periods? The

temperature inside the nephelometer is noticeably warmer than the temperature at the nephelometer inlet due to heating of the scattering volume by the lamp. Is this T appropriate for adjusting the absorption measurement to STP?

Pg. 23341, Lines 25-28: Diesel generator emissions. . .

Pg. 23344, Lines 1-2: This reference is for remotely sensed aerosol parameters (i.e., AERONET-derived) which are of relatively high uncertainty and only valid for larger optical depths. Should make reference to typical values as determined by in situ studies.

Pg. 23344, Line 5: One of the earliest references for aerosol forcing efficiency is: Sheridan, P.J. and Ogren, J.A., J. Geophys. Res., 104, D14, 16793-16805, 1999.

Pg. 23346, Line 7: What is the imaginary part of the refractive index? Is it 0i?

Pg. 23346, Lines 10-28: This seems like a fairly complicated correction scheme. Do you have any references (i.e., previously published works) for this? Readers may wish to know the details of how this is done.

Pg. 23347, Line 15: '. . . 30 to 3 h.' I assume the authors mean 30 min to 3 h.

Pg. 23347, Lines 16-18: How were the data scrutinized? Was there a threshold for particle number, above which the data were considered contaminated by local pollution? If so, what was that threshold? How can you ensure that you are not eliminating valid data from other aerosol sources (e.g., biomass burning events) that originate from the same wind direction?

Pg. 23348, Lines 25-27: Is the elemental composition of African soil different than that of Amazonian soil? How can you be sure that the increased '. . .increased concentrations of crustal elements Al, Si, Ti and Fe on ïňĄne mode aerosol ïňĄlter samples.' is due to African mineral dust advection.

Pg. 23349, Lines 14-22: Why did the median scattering ratio (2009/2010) decrease more than the median absorption ratio (2009/2010), if there were less fires in 2010

than in 2009? Wouldn't you expect a greater decrease from 2009 to 2010 in light absorption if there were fewer fires in 2010, assuming black carbon is a combustion aerosol from these fires?

Pg 23350, Lines 9-11: 'Increased mass fractions of these elements between January and March 2010 suggest the advection of mineral dust and biomass burning aerosols from Africa.' This is speculation. These elements could be from local soil sources or from certain urban emissions (e.g., power plants). Unless you know that the elemental signature of African soils is significantly different from that of the local soils, you can't be sure of the origin of the soil dust.

Pg. 23351, Lines 20-22: How were the AERONET data adjusted to 637 nm wavelength?

Pg. 23352, Lines 5-13: The suggestion that photochemical formation of secondary organic aerosols is the primary particle formation mechanism during the wet season requires one to believe that this occurs to a great extent during periods of extensive cloud cover (i.e., low light conditions). Do the kinetics of photochemical particle formation support this suggestion?

Pg. 23353, Line 6: replace 'estimative' with 'estimation'.

Pg. 23353-23355, Section 4.3: I'm not sure it makes sense to compare the size distribution data, which are limited to particles below 500 nm mobility diam, with the optical properties (angstrom exponent and backscattering fraction), which include contributions from much larger (PM7) particles. How would the relationships you have determined be affected by the mismatched size ranges used for comparison?

Figures: All of the figures should be remade at higher resolution. These were difficult to see clearly and are not of publication quality.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23333, 2012.