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

Scientific Significance:2

Scientific Quality: 3

Presentation Quality:1

This paper presents the results of a three years measurements (can we really speak of "long-term"?) of aerosol light scattering and absorption coefficients, as well as of the other derived optical parameters such as the single scattering albedo, the scattering Ångström exponent and the backscatter fraction, in the Amazon basin. Several other measurements such as particle size distribution, particle number concentration, particle mass for fine and coarse modes, chemical composition, gravimetric analysis, AOD observations and meteorological parameters are also used to interpret the results and make various correlations. The results are globally well organized and described. There is however too much figures and tables. The authors should optimized the presentation to obtain more concise figures summarizing the main results of the paper. Some of the results have also to be better discussed since there is incoherencies in some deductions .

At several places, sentences like "the measured values are in accordance with the value found in the region/ previous publications (reference)": in all these cases, please give the values found in the literature or previously published so that the reader can directly compare without reading the referred paper. For example: p. 23351 lines 7-10, p. 23352 lines 21-23, p. 23353 lines 4-6, …

Abstract:

Several points of the abstract have probably to be modified in dependence of the corrections made to the paper (aerosol indirect to direct effect, aerosol forcing efficiency, advection from Africa,…).

- P. 23336, lines 1: the sentence leads to misunderstanding: the authors want probably to say that advection from Africa lead to an enhancement of crustal element in fine mode particles. The present sentence let thing that there is an enhancement of fine mode particles, what is not the case (Fig. 15).

Introduction:

- p.23336 line 14: "the region": please specify once again "the Amazon basin" since several other regions are cited in the previous sentence.

Experimental:

- p. 23338 line 4: please give the size of the reservation either in the text of in Fig. 1.

- P. 23339 and Table 1:I am quite surprised to see an error at 700 nm two times smaller than the one at 550 and 450 nm for a scattering coefficient of 1 Mm^{-1}.
- P. 23339 line 16: backscattering instead of back scattering
- P. 23340 line 5-9: I find the MAAP error of 4% very small. I remember a previous estimate made by A. Petzold leading to 12% error. I wonder if the authors have taken into account all possible errors, since the present reported error is much less than all the errors for scatter and backscatter coefficients.
- p. 23347 lines9-11: in the text or in the figure caption of Fig. 3, please specify the location (in degrees) of Manaus and the diesel generator or the wind direction bringing influenced air masses. For the diesel generator it is given in the following §! Please put the information before.
- Figure 4: wind direction is probably given in °.$
- P. 23347 line17-18: please reformulate: it seems that it is the analysis that comprise 2% of the measurement period. Idem p. 23348 line1: “comprising” should be changed, for example by “corresponding to”.

Results and discussion

- p. 23349 line15-19: If the difference between 2009 and the other years is due to fires, it seems surprising that the scattering coefficient is much more (5 times) enhanced (110%) than the absorption coefficient (23%), since fires probably produced a lot of black carbon. Can you please explain this difference between scattering and absorption coefficient ?
- p. 23349 lines 20-22: The higher scattering coefficient during dry season isn’t also (and principally) a consequence of a larger aerosol concentration due to a lower removal by precipitation?
- P.23350 lines 6-11 and Fig. 7 and 8: The absorption coefficient is enhanced in January and February, as well as in a lower extend in March 2010. The PM2 crustal elements are enhanced to a much lower extend in January and February 2010 , but to a larger extend in March 2010. The 2009 February to May period present however a high concentration of crustal elements that is not mentioned and explained in the text and that has no correlation with the absorption coefficient (Fig7). Potassium has a higher proportion in January and February 2010, but the potassium in the fine mode is associated with to biomass burning and biogenic sources in the Amazon in the text (lines 8-10). How can then all these results convince the reader that the increase in absorption in January-February 2010 relates to advection of mineral dust and biomass burning from Africa ??? This isn’t coherent.
- P. 23350 lines17-19: the increase of the column AOD due to African advections that is not detected by the in-situ measured extinction is not clearly visible on Fig. 9. Some cases can be identified in Jan-march 2011 (large AOD aeronet), but similar cases can be found during the dry season without Africa advection (for ex. Dry period 2008). Perhaps another representation could allow to see the described effect.
- P. 23351 lines 7: what is the “particle SSA”?
- P. 23351 lines 10-12 and fig. 13: the aeronet retrieved SSA is given for the 1993-2011 period is compared to the in-situ 2008-2011 SSA. It would be much better to have the same averaging periods to compare both SSA. Various trends and particular events could have happened in the 15 years that are not taken into account in the in-situ measurements.
- P. 23351 lines 18-23: is this information in the scope of this paper ?

- P. 23352 line 5-9: Fig. 14 does not allow to see an increase of 50% of the scattering coefficient (6-5 to 10 Mm-1). It has to be mentioned that the assessment of the second sentence is not shown.
- P. 23352 line 10:The daytime shift of sub micrometer particle diameters towards larger sizes…
- P. 23352 line 13-15: you observe a greater enhancement of the absorption than of the scattering coefficient. However your explanation given in lines 13-15 would induce the opposite, that is a larger diurnal cycle of the scattering than the one of the absorption coefficient! Please explain.
- P. 23352 line 17 and figure caption 14: I didn't find the word "diel" in my dictionary. Do you mean "diurnal"?
- P. 23353 lines 12-14: and how many % for the wet season?
- P. 23354 lines 14-15: this affirmation is quite difficult to verify with Fig. 17. Fig. 17 principally shows that:
    - The correlation is rather poor between the compared variables
    - The dry season leads to better results than the wet season
    - The correlation is always better with VMD and SMD than for CMD, what can be easily explained by the fact that the Ångström exponent and the backscatter ratio are sensitive to the size (that is more correlated to the surface and the volume) and not to the number of particles
    - The fitted lines correspond to nothing and cannot be used as eye guide
    - It seems to have a somewhat better correlations with the Ångström exponent than with the backscatter ratio.

  All this should be better described under 4.3! Moreover Fig. 17 is restricted to the 10-500 nm particle diameter whereas the Ångström exponent is explained in the text to be sensitive to aerosol between 0.5 to 0.8 micrometer. In the text (p. 23355 line 5) coarse aerosol are also mentioned.
- P. 23355 line 24: "below" means that the forcing efficiency is smaller (more negative) than -3.5, but it is between -3.5 and 0!
- P. 23356 line 26-27: the indirect effect is the modification of cloud albedo and cloud life time by the aerosol. It is not the presence of clouds. The author cannot therefore make the conclusion that the indirect aerosol effect is larger than the direct one.
- P. 23355 line 27-p. 23356 line 1 : where can it be seen that the direct effect can be as important than the indirect one in the dry season ? Just because the cloud cover is of about 50% ' + see previous comment.
- P. 23356 line 8-13: As it is clearly explained, the only reasonable comparison between the in-situ calculated forcing efficiency and the one measured by REM is for days without clouds. The authors have to do this comparison and not the one described in the paper.
- P. 23356 lines 13-15: compare these values with REM ones.
- P. 23357 line 3: please mention also here briefly how this refractive index was found.
- P. 23359 line 10: 1-15 points over how many?
- P. 23359 last §: redundancy, please remove as well as Fig. 21

Conclusion:

This section should not only list the previous discussed results, but also synthetize them to obtain a global picture of all results.

Tables: there is globally a lot of tables!

- Tables 5: redundancy with Figures ? please use always quartiles or percentiles, but no mix of them.

Figures: there is also too much figures!

- Fig. 2: % of data necessary?
- Fig 5-6: one figure?
- Fig. 7-8: one figure?
- Fig. 8: labels not clear (once PM2 crustal and once k/PM2), please put a) and b) in the figure and figure caption
- Fig. 9, not very readable
- Fig 10- 11: redundancy with Fig 5-6 and tables ? it is not clear that you make a mean 2008-2011 seasonal cycle and why do you use 10 days means . (idem for Fig 12)
- Fig. 10-16 the dry season should also be colored in rosa as in previous Fig.
- Fig. 16: one fig with 2 axis
- Fig. 17: better 2x3 fig than 3x2 fig! + see comments in text
- Fig 20-21: not necessary