

## ***Interactive comment on “Radiative budget in the presence of multi-layered aerosol structures in the framework of AMMA SOP-0” by J.-C. Raut and P. Chazette***

**J.-C. Raut and P. Chazette**

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The authors thank referee3 for his positive response to the paper and useful comments. Please find a point-by-point discussion and answer of the issues raised by reviewer 3. To facilitate the work for the reviewers and readers, the reviewer's comments and suggestions are preceding each reply in bold face.

**(figs. 1 to 5 panels b, and comment in the text 12474, 23-26) in the graphs of the Angstrom coefficient, the addition of this parameter as computed from the PCASP size distributions might be beneficial, either to compare with the other two measurements, and to spot out regions where the particles left undetected**

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by PCASP resided.

The graphs of the Angström exponent illustrate the coherence of the datasets from an experimental point of view. As suggested by reviewer3, this parameter can also be calculated from the particle size distribution determined by the PCASP. However, this approach also requires the knowledge of the ACRI, which constitutes one of the objectives of the study. Hence, at this stage of the paper, Angström exponent cannot be retrieved from the PCASP size distributions. However the authors agree to add this modelled parameter with the measured ones. Although it cannot serve to discuss the validity of the measured extinction and scattering coefficients (because the ACRI determination also needs those optical properties), it can be useful to show that the ACRI retrieval method does not trigger off significant bias in the Angström exponent.

**(Fig. 5b) There is a generally good coincidence between the lidar-nephelometer derived Angstrom coefficient and the sunphotometer one, being the latter close to the value the former attains in the lower layers, where the optical length is greater. This should also apply to the fig. 5b case but in fact it does not. Could the authors comment on that?**

This is a good question. Generally the graphs of Angström exponent in Figs 1 to 5 indicate that the sunphotometer-retrieved Angström exponent corresponds both to the column-integrated Angström coefficient as measured by the FAAM and to the value the ULA-derived Angström exponent attains in the mineral dust layer with a larger optical thickness. Nevertheless, as pointed out by reviewer3, this is no longer valid for case 5, where both ULA-derived and FAAM derived Angström exponents are in agreement but differ from the sunphotometer s one. We have to notice that the discrepancies can be due to the locations of the flights and to the wind direction. The FAAM aircraft typically covered a horizontal distance of 100km during deep profiles,

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whereas the ULA covered much shorter distances and the flight tracks were directly above Banizoumbou (case 3) or Niamey (cases 1, 2, 4 and 5). Wind measurements obtained on ARM site at the altitude of 6.5 m above the ground show that the prevailing winds blow from the northwest direction ( $330^\circ$ ) on 26 and 28 January (cases 1 to 4) and from northeast direction ( $50^\circ$ ) on 1 February, which is typical of the Harmattan wind blowing over the Sahel in the dry season. Although the winds generally blew from the North, thus carrying mineral dust particles, a north-westerly wind can also bring a local contribution of anthropogenic aerosols because Niamey airport is located South-East from the Niger capital. Pollution with such a local origin superimposes to the dust cloud and can explain Angström exponent values slightly greater than 0 that have been observed on cases 1 to 4. This turns out to be a local phenomenon in Niamey, which is not representative of situations encountered in Banizoumbou where AERONET station is located. This point will be mentioned in the revised manuscript.

**(12478, 8) the sentence "at not-forward angles Mie theory overestimates the scattering" is not true in general, but only at scattering angles close to the forward and backward directions. This is implicitly acknowledged in the following lines 16- 19. Nevertheless, the abovementioned sentence is misleading and should be clarified.**

Yes, this is a mistake. In our case, for all prolate or oblate particles with an aspect ratio comprised between 0.5 and 2, Mie theory underestimates the scattering between  $80^\circ$  and  $140^\circ$ . The sentence will be corrected: at not-forward angles Mie theory mis-estimates the scattering.

**(12478, 20-22) The fact that the extinction for equivalent area spheres is lower than the extinction for aspherical particles does not apply to all possible size distributions, since it is - very weakly - dependent on the average size parameter.**

**Is it as stated for small size parameters. I understand that for this particular case it does apply. However this might be stated to avoid misinterpretations, also in view of the caveats one should bear in mind when using the measured size distributions, which might mis-estimate the larger radii tail of the distribution.**

In our study, the large and well-marked minimum at  $120^\circ$  in the scattering phase function of spherical particles is not present in simulations when spheroids particles are used. Hence, the case of spherical particles corresponds to a minimum for extinction and scattering cross-sections. The authors agree with referee3 in the fact that it is dependent on the average size parameter and will note in the revised paper that it does not necessary apply to all possible size distributions. We have also to notice that, in our study, the scattering is overestimated close to  $180^\circ$  when assumption of spherical particles is done.

**(12481, 10) There and in the following, it is not clear whether the uncertainties attributed to ACRI are due to the variability of the dataset, to the propagation of measurement errors in the respective methods, or a combination of both. In this latter case, how does the two compare? For instance, the isopleths of the A1 method, as in fig. 6, are more inclined with respect to each other than those of the A2. This would mean that the former more effectively constrain the two determined parameter, with respect to the latter. Has it been taken into account in the computation of the uncertainties?**

The standard deviations given in Sect. 5.1.1 are due to the variability of the dataset. Notwithstanding sensitivity analyses based on a Monte Carlo approach have been performed to assess the uncertainty in the calculation of ACRI. In the computation of ACRI, there is not any propagation of errors since both the real part and the imaginary parts are retrieved in a unique step. This is the result of a function minimization of

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two variables (Sect. 4.1.1). In the case of A1 approach, the uncertainty on ACRI is due to the uncertainties on the extinction coefficient, BER and size distribution. In the case of A2 approach, the uncertainty on ACRI is due to the uncertainties on the scattering coefficient, the single-scattering albedo and the size distribution. Hence, the fact that the different approaches do not constrain the optical parameters in the same manner has been implicitly taken into account. It is as if the isopleths of Fig. 6 were surrounded by errors bands. The total uncertainty is given by the surface delimited by the intersections of those bands. This could not been represented on Fig. 6 as the uncertainties on the size distribution correspond to a translation of the isopleths. Monte Carlo approaches have given a total uncertainty of 0.03 (0.04) on the real part and 0.006 (0.012) on the imaginary part of ACRI in case of A1 (A2) approach. In this particular case, A1 approach more effectively constrains ACRI values but we have to notice that this does not apply to all cases. The errors on ACRI due to the uncertainties of the measurements are lower than ACRI dataset variability, which is consequently significant. This discussion will be added in the final paper.

**(12489, 13- 12490,15) It is difficult to compare these radiative forcings with other from other cases, with maybe the same aerosol kinds, but different burdens distributed vertically; the authors should quote their respective optical depths along with the radiative forcings, at least.**

The authors agree with referee3. This point has been answered in the reply to the comments of reviewer1. The authors will quote the respective optical depths of the mentioned studies along with their radiative forcings.

**The technical corrections have been done.**

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