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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical profiles of urban aerosol complex refractive index in the frame of ESQUIF airborne measurements" *by* J.-C. Raut and P. Chazette

J.-C. Raut and P. Chazette

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We thank referee2 for his interest in the article. We have tried to clarify certain points.

1/ **It is not clear to me what the authors called Part 1 and Part 2. Do they refer to height ranges or coordinates in latitude and longitude or flight legs? Can it be described more clearly?**

Both flights for 19 and 31 July have long flight legs at 950 m, where the plane was circumventing the urban area. Aerosols measured in the lowest altitudes ($<$950 m) are therefore different from aerosols studied over 950 m. Part 1 and Part 2 thus

correspond to height ranges where aerosol properties are studied for each specific day. Part 1 and Part 2 refer to aerosols located inside and outside the Parisian plume respectively, as it was precised at the end of Sect. 2.2. Their accurate description for each case (inside/outside and upwind/downwind the plume) was also mentioned in Table 2 and Fig. 3.

2/ **How the correction factor is used? Do the authors simply multiply the resulting total scattering between 7 and 170 degree by this correction factor? It seems quite a small value given that the phase function in the first 10 degree can be quite different (and higher) than in the rest of the 180 degree.**

The correction of the non-observed angles of the nephelometer has been calculated using the ratio of the integral of the phase function between 0 and 180 degrees on the integral of this latter between 7 and 170 degrees. The correction factor given in the article (1.035) corresponds to the term between parentheses. Such a small value can be explained by the predominant very small particles in the size distribution (Table 1). A higher coefficient would be expected for larger particles since these latter clearly modify the phase function in the small scattering angles, e.g. by Fraunhofer diffraction.

3/ **The authors give in Section 2.2 values of the AOT +/- 0.03 and +/- 0.05 (probably from the AERONET stations of Creil and Palaiseau, it is not said in the text), whereas they stated in Section 2.1 that AERONET errorbars for the AOT was +/- 0.02, independently from the aerosol loading (which I doubt). What is the correct errorbar?**

AERONET website suggests an uncertainty of 0.01 8211; 0.02 in AOT, that is wavelength dependent, due to calibration uncertainty for the field instruments

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(http://aeronet.gsfc.nasa.gov/new$_web/system_descriptions_calibration.html$).

So as to be in agreement with this result, we have only given in text the maximal uncertainty (i.e. 0.02), which has never been contradicted by any author. Should someone find some contrary information, he should mention it for the whole scientific community. In Section 2.2, aerosol optical thicknesses (AOT) are taken from measurements in AERONET stations of Creil, Paris and Palaiseau as thought by referee2. The corresponding values are given with an errorbar higher than 0.02 because the calculation of the uncertainty has been realized at 532 nm, which takes into account the errors on aerosol optical thicknesses (0.02) and on Angström exponent. We made it clearer in the new manuscript.

4/ **The ACRI retrieval method is not well described, not in a clear manner. A diagram would help a lot (like in Raut and Chazette, 2007).**

A diagram has been added in the new manuscript, helping the understanding of all the steps of our method.

5/ **Lots of the hypothesis (a priori dispersion of nr and range of ni) made in the application of the method presented are based on results obtained in Raut and Chazette (2007) with data from May 2005. In the submitted paper, the data are from July 2000 and no discussion is given about this difference.**

The authors disagree with the referee. No hypothesis has been taken from the results obtained in Raut and Chazette (2007). The only one hypothesis required in our method

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is the likely range of imaginary parts. We chose ni <= 0.05 because anthropogenic aerosols from automobile traffic do not present higher imaginary parts. AERONET retrievals discussed in Sect 5.2 and chemical analyses reported in Sect. 5.3 have finally shown that ni was always lower than 0.025 in July 2000 over Paris area. The assumption we made ni <= 0.05 is therefore not forcing at all. The dispersion of the real part nr <= 0.02 is not an assumption but is linked to the likely range of the imaginary part as presented in Raut and Chazette (2007). The dispersion of the real part is weak and permits its assessment. We have added this point in the article.

6/ **An underlying argument (not explicitly given as is) to the previous comment is that Paris and its surroundings are loaded with anthropogenic aerosols from automobile traffic. But is there really no other aerosols present on the specific dates of the study? Reference it!**

Automobile traffic is the main source of photooxidant pollution and aerosols in large cities such as Los Angeles (Lurmann et al., 1997) or Paris (Menut et al., 2000). There are naturally other sources of anthropogenic aerosol particles like emissions from plants burning household refuse or airports (quoted in Sect. 4.3), but they can hardly contradict the very large hypothesis ni <= 0.05. We have added this point.

Lurmann, F. W., Wexler, A. S., Pandis, S. N., Musarra, S., Kumar, N., and Seinfeld, J. H.: Modeling urban and regional aerosols: II. Application to California8217;s South coast air basin, Atmos. Environ., 31, 26958211;2715, 1997.

Menut, L., Vautard, R., Flamant, C., et al.: Measurements and modelling of atmospheric pollution over the Paris area: An overview of the ESQUIF project, Ann. Geophys., 18(11), 14678211;1481, 2000.

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7/ **I have a hard time to believe that a constant BER profile of 0.014 sr-1 can be used for the Paris ABL at all season of the year. Can the authors add some errorbars to this value? Is there any Raman measurement available in the Paris area?**

We agree with the reviewer that BER is usually not constant with height. We have never stated that this value could be used for any season in the year. The value $0.014sr{-}1$ has been calculated as indicated in Chazette et al. (2005) for the three days considered in the study. An altitude-dependent BER would have been indeed particularly appreciated in this study but the lack of a Raman lidar leads us to suppose a constant BER in the atmospheric column. The only way to introduce errorbars on the BER lies on a comparison of the BER from lidar and that calculated using a Mie model and the airborne measured microphysical parameters taking into account RH profile with altitude. This study had already been performed in Sect 5.5 and exhibited a maximum absolute difference of $0.0008sr{-}1$ on the BER that remains constant with height. That is not evidence that the real BER is constant with altitude but shows that the method does not introduce a bias in BER vertical profile.

8/ **Furthermore an important work of cross comparison is needed between the retrieved and measured optical and microphysical parameters. In other words, the loop is not closed. The authors have taken as reference the size distribution from the PCASP and CPC measurements, the scattering coefficients from the nephelometer and the AOT from AERONET. A check if the recalculation of all optical measurements (AOT and sky brightness) still leads to values within the error bars of the measurements would validate the method.**

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We have proved that the scattering coefficient given by the nephelometer had given (with the size distribution) ACRI through a Mie model, and that these ACRI enabled the simulation of an extinction profile in accordance with lidar extinction profile within error bars on measurements (see Fig. 7). Note that the Mie code also permits the derivation of scattering coefficient profiles that are equal to the measured ones. Given that lidar extinction profile was in agreement with the aerosol optical depth in the BER determination, this result thus indicates the coherence between the scattering coefficient from nephelometer and the optical thickness from AERONET station, and therefore with sky radiance.

Moreover, as the authors have specified several times into the article, all error bars on the retrieved optical parameters (ACRI, extinction coefficient, AOT) have been calculated thanks to the use of Monte-Carlo methods. These stochastic approaches assume independent variables having a Gaussian probability distribution function in this study. That implies that the retrieved parameters have an error taking into account all the uncertainties of the input parameters, such as the measured optical and microphysical parameters.

9/ **A comparison between the measured size distributions and AERONET is also necessary (as mentioned also by Rev. 1).**

The comparison of the measured size distributions and AERONET has been performed and presented in the reply to the minor comments of referee1. Both size distributions are in accordance for the accumulation, which is the more optically efficient. The fine mode measured by the PCASP and coarse mode measured in AERONET stations have a negligible influence on an optical ratio such as BER (0.0135+/-0.0009).

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10/ **What about the comparison of the BER from lidar and that calculated using a Mie model and the measured microphysical parameters?**

This is a good idea from reviewer2. That is exactly the reason why the required comparison of the BER from lidar and that calculated using a Mie model and the measured microphysical parameters had already been done and discussed in Sect. 5.5. We have shown that the BER deduced from the synergy lidar 8211; sunphotometer was in accordance with the BER simulated through a Mie code using size distributions taken either from PCASP, or from AERONET products.

11/ **I have a hard time to believe the backtrajectories arriving at 500 m with so many deposition on their way. Therefore I agree with the first minor comments from Rev. 1 about performing a sensitivity study of the backtrajectories.**

This sensitivity study has been realized and discussed in the reply to referee1.

12/ **Some parts of the sections Comparison to AERONET measurements and Comparison with lidar profiles are not at the right place. Both AERONET and the lidar measurements take part completely to the retrieval of the ACRI and should come earlier in the paper.**

In the article, we made a difference between two kinds of AERONET products. Optical depth is calculated from spectral extinction of direct beam radiation at each wavelength based on Beer-Bouguer law. It is not an inversion product from sky radiances. Only optical depth is required in the retrieval of ACRI. It is the reason why the other AERONET measurements, such as refractive index, single-scattering albedo and volume size distribution, which are inversion products from Almucantar measurements,

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have been presented later in Sect. 5.2 and Sect. 5.5 and only for comparison and discussion of the uncertainties purposes. We think that making distinction between method and comparison to other measurements (such as AERONET products) is more relevant.

The same argument is valid for the part concerning lidar comparison. We cannot say that lidar measurements took part completely to our approach. Indeed, only a constant BER value has been taken from lidar for the retrieval of the imaginary part of ACRI. We also showed in Sect. 5.5 that the BER retrieved from AERONET inversion products and a Mie model was in accordance with the BER from lidar suggesting that lidar extinction coefficient could be considered as an independent parameter enabling a comparison as in Sect. 5.4. BER has been deduced from a convergent method between aerosol optical thicknesses from lidar and sunphotometer. Thus, only a global value of aerosol loading over the atmospheric column has been necessary in ACRI retrieval, and never the shape of extinction profiles from lidar.

13/ **I think a table is necessary in the Section Comparison to previous studies. Furthermore an identification of cases similar to the study presented here should be emphasized. My sensation is that, as it is presented here, that section could perfectly fit in the introduction, qualitatively speaking, as it browses the different techniques used to retrieved range-resolved ACRI.**

Such a table can lead to a clearer presentation of the results corresponding to the different approaches and will be added in the new version. Although all of those methods are not easily comparable (hypothesis, type of the instruments, convergent methodology, time, place), the common point of the approaches by Ferrare et al. (1998), Müller al. (2002) and Redemann (2002) is the synergy between lidar and in situ measurements. We will follow the advice of reviewer2 in separating the description

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of the techniques in the introduction part and the comparison of ACRI values in Sect. 5.1.

14/ **Specific comments**

The size of the font has been increased in Fig. 1 and English corrections have been done.

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