Anonymous Referee #3

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This paper presents a study of the aerosol vertical profiles in Corsica Island mainly from lidar measurements. Sun-photometer and satellite AOD measurements are used to constraint the inversion of the lidar signal using the Klett's method. Then, the vertical profiles of the aerosol extinction coefficient are obtained and their monthly evolution and their relationship with the airmass origin are studied in the paper. Moreover, the discussion of specific episodes for mineral dust and pollution aerosol are carried out. However I am concerned of publishing this paper on ACP due to the following reasons: - The paper lacks a clear focus and there are no new scientific findings. - The used methodology is well known and the database is too short (around 1-year measurements) to stablish a climatological study of the aerosol vertical profile. As the authors claim in Pag. 3 and Line 21, there are already a lot of papers devoted to the characterization of the aerosol vertical profiles in the Mediterranean basin. What is different in this paper? - A more detailed analysis of the quality of the lidar data is needed (see specific comments) - The paper is not well organized. The instruments, the observations, the data processing methods for each kind of measurements (lidar, sunphotometer, satellite, etc.) and the results are addressed in the same section. I suggest the separation of observations, methodology and results in different sections. - The data analysis is poor and confusing, a more in deep discussion of the obtained results is desired.

ANSWER. The introduction has been improved. As noticed **by reviewer 2: "the large spatial variability of the aerosols in the area and the complex situation concerning aerosol sources can justify additional information over a less studied area like Corsica**". This has been now clearly stated in the introduction.

CORRECTION. The introduction has been reorganized into paragraphs reflecting a traditional and logical succession of ideas : (i) importance of aerosol for climate (ii) the Mediterranean atmosphere (iii) why setting a station in Corsica (iv) the objective of the paper.

Specific comments: P 2. L 10: What the ±0.09 means? The same throughout the entire paper, Are these quantities the standard deviations, the uncertainties? Please explain it. P9. L5.

ANSWER. Yes it is.

CORRECTION. We have replaced ± by standard deviation in the text.

Figure 4 compares the AOD obtained from sun-photometer, lidar and satellite. If the sun-photometer (or satellite) AOD are used to constraint the inversion of Lidar signal. It is clear that the AOD obtained from the lidar is close to the sun-photometer and satellite observations. What do the authors attempt to demonstrate with this picture?

ANSWER. Lidar are working day and night why satellite and sun photometer only provide daytime measurements. Moreover not all the lidar retrievals are constrained by AOD measurements. So there could be a difference.

P9. L10. In Figure 5, the authors show the extinction profiles on a monthly basis for 2012 and

2013 separately. This is the most problematic figure. Several issues indicate that the quality of the lidar data or the way in which the Klett inversion was applied are suspicious. - Usually such sharp peaks in the extinction coefficient, which are observed in most of the figures, are associated to the presence of clouds. However, it is rare to observe them in the monthly mean profiles since the aver-ge of several profiles should smooth that peaks. Therefore, this feature may indicate that the cloud screening is not working well during the automatic data processing. Has the cloud screening been tested by visual inspection? On the other hand, how many measurements were used to evaluate the monthly mean extinction profiles? If the average is done only with few profiles, the remaining cloudy peaks may strongly impact the resulting average. I suggest to impose a minimum number of profiles to carry out the average and to avoid this. - The figures show a systematic increase in the extinction coefficient with the altitude over the 2 km. It is difficult to associate this increase with the real aerosol behavior, especially in the monthly mean profiles. In almost all cases the extinction at 7 km altitude is higher than that observed near the ground (e.g. Fig. 5 May, October). This behavior might be related with the poor quality of the lidar signal or with the reference altitude chosen for the molecular signal. In the unlikely case in which this high extinction coefficient (around 7 km) was due to the aerosols, why not extend the y-axis in order to show the upper boundary of the aerosol layer? The authors should explain this issue with detail. - The authors should include the standard deviation in these figures and give the number of measurement profiles used in the average.

A detailed check of these points is mandatory to trust in the results presented in this paper.

ANSWER. We have identified that the degradation in the quality of the extinction retrieval comes from the choice of the reference signal in the upper part of the profile. The cloud screening is not involved in this issue. Selecting a constant altitude whatever is the signal-to-noise-ratio (SNR) has introduced artifacts in the retrieval of the extinction profiles, including spikes and remaining relatively high extinction coefficient in the upper range of the profile. We now use the SNR to delimit the useable part of the profile. The SNR is estimated for each altitude by computing the mean of standard deviation of the range corrected lidar signal at 3 successive altitudes. A threshold value of SNR=10 is still acceptable for most of the profiles and removes spikes and drift in the mean extinction coefficient. However it removes also most of the profiles for which we have identified high AOD and high altitude transport. So the results presented in the last section are affected by a large uncertainty in the extinction coefficient profiles. We suspect that this problem is caused by the dust deposition on the telescope that reduces emission and reception. The case study on the dust event in June-July 2012 is not affected by this problem because during this period an operator was on site. Further investigation on the noise reduction is required to provide accurate estimate of the extinction profiles at high altitude for those cases.

CORRECTION. Although we have solved the issue regarding most of the data presented in this paper, the discussion can't be based on analysis of high AOD events since those cases remains problematic and required further analysis. Such analysis is not possible within the limited time frame requested for revision. As a consequence, we believe it is not worth submitting the present revised version of the manuscript. However, we have followed your recommendations regarding the structure of the paper and we expect to have an improved version for a further submission.

Sections 3.3 and 3.4. The comparison of the extinction coefficient obtained with lidar with the in situ measurements at the surface and the determination of the layer altitude is meaningless if the quality of the lidar profiles is not firstly checked.

Pag. 12. The discussion section is confusing. The authors analyze several specific events (dust and pollution) during the measurements period in terms of the AOD and air mass origin. They study a day-by-day characteristics of each single episode. I suggest analyzing separately the average behavior and characteristics for all the dust episodes, and all the pollution episodes founded during the measurements period.

ANSWER. Correct

CORRECTION. We have followed your recommendation. We have reorganized this section in two parts: dust and pollution aerosols.

Unfortunately, I am very suspicious of the data analysis procedures and the results obtained in this paper.