

Interactive comment on “Are EARLINET and AERONET climatologies consistent? The case of Thessaloniki, Greece” by Nikolaos Siomos et al.

Anonymous Referee #1

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The subject of the manuscript is relevant to the journal, as different end-users need vertically-resolved aerosol profiles obtained from climatological observations, instead of using models (i.e. ARMA model). Nevertheless the paper shows conceptual errors that introduce serious issues that makes it not suitable for publication. Please refer to comment section for details.

Major Flaws:

1) The two climatologies cannot be compared and no conclusion can be drawn. AERONET is a daytime measurement, while lidar observations are taken and averaged independently, both during daytime and nighttime. For sure, being different at night and day both the atmospheric conditions and aerosol emission sources (e.g. traffic and or household heating), a non-negligible bias is introduced in the analysis and

consequently it is not possible to establish whether the correlation is good or not. 2) In the text it is clearly specified that a mean value for the lidar optical thickness is obtained by averaging measurements based on the elastic scattering technique with those obtained with the Raman scattering technique. The use of different techniques introduces a further bias. 3) The most important contribution to the aerosol backscattering and extinction coefficients is coming from the first hundred meters that are heavily affected by overlap function. Only marginally In section 3.2, line 14 pag. 6 overlap problems are described . As nighttime and daytime profiles are averaged together, an additional source of bias is introduced: what about the profiles for which the aerosol load is confined below 500m? It looks like those profiles cannot be compared at all with AERONET retrievals as in fact only a portion of those aerosol layer is detected. 4) Even for daytime profiles, in the manuscript it is not even specified if an overlap correction is performed (i.e. shooting the lidar horizontally) and what is the extent of the lidar blind region and what the authors did to overcome this problem. 5) It seems that the comparison has been performed based on data from the EARLINET database. In spite of points 1) and 3) above, the comparison has been done considering on average 52 days per year (corresponding to Monday morning schedule). On 52 these days, how many of them are cloud free? Are then the averages statistically meaningful? The paper is missing such analysis.

Specific Comments:

Line 2 Pag. 1 Measurements are not deployed, instruments are.

Line 3 Pag. 1 Please read: "These two instruments are members of two different networks..."

Line 4 Pag. 1 Please read:" The instruments are operated under a different time schedule."

Line 12 Extinction is not defined. It is clear in the lidar community but for general audience it should be given a broader definition, as the vertical-resolved extinction

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coefficient

Line 16 Pag. 1 Please read: "a priori climatological profiles" Line 16 Pag. 1 Please read: " they can be used by modelers community"

The English in the abstract was improved. This should be extended on the whole manuscript. Often sentences are too long and convoluted.

Line 1 Pag. 2 atmospheric particles don't show variability, but concentrations or load yes.

Line 4 Pag.2 atmospheric conditions is more appropriate than wind circulation, other phenomena as convection are important.

Line 14 Pag.2: "The in situ technique. . ." please rephrase as the sentence is not clear.

Line 17 Pag. 2. References are not at all exhaustive. This comment is valid through all the manuscript.

Line 31 Pag. 2 Raman indicates a person last name, then should be caps lock everywhere in the text.

Line 33 Pag. 2 As written before, it is missing an analysis on how much lidar data were used in the analysis (yearly and month-by-month)

Line 1 Pag. 3 Few minutes is not acceptable scientifically. AERONET specifications are available at NASA GSFC website.

Line 25 Pag.3 AERONET aerosol optical depth at 440nm should be greater than ~ 0.05 since the calculation of Angstrom exponent at very low optical depths could introduce error due to the uncertainty of the AOD measurements (0.01) for wavelengths greater than 400nm. For high AOD and fine mode particles, the UV wavelengths may not fit on the logarithmic linear scale so some error can be introduced. How the authors dealt with those aspects?

Line 25 Pag 3. This is another potential serious issue underestimated and neglected in the paper. Why level 1.5 AERONET data are used? Level 1.5 data have pre-field calibration applied, however the calibration can change during the deployment (usually a linear rate due to slow deposition on the sensor head lenses), hence, the need for a post-field calibration. This means that Level 1.5 may show a large bias.

Line 1 Pag. 4 Pre-processing, not preprocessing

Line 25 Pag. 4 Why is not reported the used Lidar Ratio in the retrieval?

Line 30 Pag. 4 see Major Flaws section

Line 15 Pag. 5 why less structured? Is it due to the smoothing? If yes, how the profiles were smoothed?

Line 1 Pag. 6 The statement is not correct. The maximum height is reached not at noon (too generic) but at 12 Local Solar Time.

Line 15 Pag.8 The two distributions would not be similar if the lidar instrument reached full overlap closer to the ground.

Line 1 Page 9. "This is. . ." please make the sentence clearer.

Section 4.2.2 Integrated backscatter. It seems that this section doesn't make any sense. There is not added value in this intercomparison. It is exactly the same of integrating the aerosol extinction coefficient to retrieve AOD. Moreover, dividing arbitrary AERONET measurements by 50sr lidar ratio introduces very high errors.

While the reviewer recognized the potential importance and relevance of the comparison, the results reported in the paper are affected by severe methodological problems, which completely compromise their quality. The analysis of the present dataset should be reformulated removing all major methodological problems illustrated above.

The reviewer is available and willing to review again a completely revised version of the paper with consistent results obtained after addressing all the methodological problems

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indicated above.

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