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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertically resolved aerosol properties by multi wavelengths lidar measurements" *by* M. R. Perrone et al.

Anonymous Referee #3

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The authors present a graphical method that is used to derive particle size and the mixing-ratio of the fine-mode and coarse-mode particles. The input data are vertically-resolved (profiles) of extinction coefficients derived from simple backscatter lidar measurements. The authors present results of several case studies of data taken with lidar in Lecce, southeast Italy. The authors' mains conclusion is that it is important to understand the fact that particle properties may change with altitude and that Lecce experiences various pollution conditions like dust from Africa, pollution from the Mediterranean region and long-range transport from North America. The authors conclude that their method is applicable for future research.

The paper is not acceptable. It lacks in novelty, has strong weaknesses in the methodology, lacks any kind of convincing comparison, and the results largely repeat findings

from the multitude of lidar observations taken in EARLINET (south European stations) that show similar findings. I acknowledge that each EARLINET station in itself adds a valuable piece of information to the overall knowledge that is collected on aerosols, their spatial and temporal distribution, and particular properties at each station along the Mediterranean rim. So each station certainly bears its own merit in this network and should not be neglected only because similar findings were made by another station. It is the statistics that counts, and by adding similar observations from different stations this statistics can be collected as added value. This is a good thing to do. However, I do not see the novelty of work in this paper, as it is incomplete, and the applied methodology itself introduces errors that lead to wrong or biased conclusions. In summary: the robustness of this study is not presented in any convincing manner. It is known that the vertical dependence of aerosols exists. The graphical method the authors use is not that new at all. In that regard I am missing reference to the papers by o'Neill (Applied Optics, 40, 2368-2375, 2001; JGR, 106, 9787-9806, 2001; maybe even JGR 107, EID = 4125, 2002 may be relevant in this context). In view of these reference, even though they may not 100% reflect the current approach, I am wondering about any other publications that may also be missing in the reference list, which in turn does not allow me to understand the novelty of this present paper.

Coming back to o'Neill: he showed long time ago that the curvature of the extinction spectrum can be used to extract information on the fine-mode and coarse mode fraction of aerosol. So that would leave me with the novelty (in this present paper) that vertical profiles of extinction have never been analyzed in that way, and as far as I recall there may have already been a previous paper on this present method being published some time ago. The authors may want to comment on this, as I may be wrong in my assumption. The main flaw is that the authors wash away the fact that they do not use Raman lidar or HSRL to measure extinction which provides the necessary precision of the extinction data so that the graphical curvature method can be used in a robust way. I do not mean to say that every lidar in the world must be Raman lidar or HSRL, but if a lidar has no Raman channels any lidar team these days must put specific emphasis on error analysis and carefully consider conclusions in view of lack of robustness of extinction data. The authors use backscatter profiles, make assumption on the lidar ratio (in part corroborated by backward trajectories), they extract extinction profiles, and then use the curvature method to obtain their results on mixing ratios of fine-coarse mode, and particle size (or mean size). The authors in a very unclear manner make a sensitivity analysis in which they claim that the errors of the extinction values are not that much influenced by their assumptions, though they have to make quite a bit of guess work on the lidar ratios. Such conclusions as drawn in this paper simply cannot be made on the basis of simple backscatter lidar and the fact that lidar ratios vary with height, even more if we consider that lidar ratios cannot be measured with 0% measurement error. Even 10% measurement uncertainty are hard to achieve with Raman lidar; Angstrom exponents also do not have errors less than 20% in that case. Experience with Raman lidar (which measures extinction) rather shows errors of 30-40% and I do not see these error bars in figure 2b, 2d, 5b, 5d.

I do not find a clear description (in terms of hard numbers that can be clearly followed) of error-propagation of this complex analysis. Error bars are missing in spots where it is crucial to show them. Figures 8 and 9 are prime examples of avoiding error bars, and this gives a false impression on the accuracy (and precision) of the method.

The corroboration by backward trajectories has flaws. For instance, the trajectories in figure 4a show advection from North America. When I counted the symbols in this 2-d plot and compared to the lower panel (Figure 4b) it seems that more symbols (time steps) are shown. So some part of the trajectories in Fig. 4a is missing. This in turn makes me wonder what I am seeing in this trajectory plot, and if the conclusion of long-range transport is justified at all. We need to keep in mind that the trajectories need to be somewhat close to the ground which in turn means that the respective air parcels could have picked up aerosols which then might have been transported over long distances from the US to Europe. Was this the case here, or couldn't it be the case that the air parcels picked up aerosols in the Mediterranean area, just 1 or 2 days

prior to arrival over Lecce? So is the conclusion on aerosol types, which decides on the correct choice of lidar ratio, which in turn decides on the correct extinction values and their uncertainties correct at all? In the end I have to state that this whole approach comes very close to a circular argument.

The AERONET data in that regard do not prove a lot, as they are describing the columnar results, but the novelty of the paper can only hold for applying the method to the aspect of analyzing vertical profiles.

I also see flaws in data analysis of the lidar profiles. The Angstrom profiles in fig. 2b and 2d show a continuous increase of the Angstrom exponents with height. This increase is so "stable", I have never seen this before in my career. I do not want to argue that only because I have not seen such a behavior it may not be possible that it exists. But I do know that such systematic behavior can easily be produced by wrong alignment of the lidar instrument or a flaw in data analysis: I simply have seen it too many times before, and after considering all possible error sources such kind of profiles returned to what I would like to call "normal" behavior of the vertical distribution of aerosols and Angstrom exponents.

The argument that DREAM results support the results of the lidar analysis has flaws in itself. It is known that the dust emission models still are not accurate enough to provide an unambiguous proof of experimental data. Particularly in this present study the model data must be of high quality, because the lidar data simply do not have the necessary robustness. What also strikes me is the fact that figures 2c and 12b show model results (DREAM model) with a comparably high dust concentration. Why is the volume depolarization ratio less than 5%? Even if I consider a reasonable conversion to particle depolarization ratio I do not see a way of ending up with values of more than 10% for the particle depolarization. In view of the overall set of data presented in this study, 10% particle dust depolarization (even under consideration of mixing conditions) cannot be true. Either this number for the depolarization ratio (5% or less) is a typo (I also see low depolarization ratios in Fig. 5a) I am not able to understand the results at all. I find additional flaws in data analysis, but I think I made my point clear. The methodology certainly bears its advantages, however neither the way of data analysis nor the quality of data is good enough to draw the kind of conclusions made in this paper. Last but not least: the authors strip themselves off the merit of the paper because they show three cases studies, nothing more. In view of long-term studies at this site a statistical analysis could extract problems with data analysis and also add valuable information to our overall knowledge of the vertical distribution of aerosols in South Europe.

I recommend that the authors withdraw this paper and start all over again. They should reanalyze their data, include a comprehensive and traceable error analysis, give credit to publications related to their methodology, search for similar studies so that they can corroborate the novelty of their work, and push their work to a more comprehensive description of the aerosol situation in South Italy.

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