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

Anonymous Referee #2

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General remarks

The paper presents a combined photometer/lidar method to retrieve profiles of microphysical properties (here in terms of particle effective radius) and fine mode contribution to the total aerosol optical depth. The paper may be appropriate for publication after major revisions. Presently, the paper cannot be accepted because of the missing uncertainty analysis. The lidar uncertainty analysis is of fundamental importance. Most of the argumentation is just speculative at the moment. The results are solely based on the spectral slope of the particle backscatter coefficient for the wavelength range from 355 to 1064 nm. It is well known in the lidar community how difficult the retrieval of the 1064 nm backscatter coefficient is, and therefore how large the uncertainties in 1064 nm backscatter coefficients can be (100% or even higher). So, all the product based on the wavelength dependence of backscatter or the respective extinction coefficient, in turn based on 1064nm backscatter values are very, very uncertain. Without a rigorous error analysis (uncertainty discussion) the results are not trustworthy at all, and the paper must be rejected.

Details

Page 4, lines 1-10: What about the other combined lidar/photometer methods (GARRLIC, Lopatin et al., AMT 2013, POLIPHON, Ansmann et al., ACP 2012)?

Page 4, line 27: There are a variety of O'Neill papers (beginning in Appl. Opt. 2003 or even earlier) dealing just with the slope of the AOT to get the fine-mode and coarse-mode contributions to total AOT. One of these methods is now a standard retrieval in AERONET. Any comment to that?

Page 7, line 12: Please improve: aerosol extinction-to-backscatter ratio, not aerosol-to-backscatter ratio.

Page 7, line 18-27: Please specify (quantify) the uncertainty in the backscatter retrieval at 1064 nm. Estimate by varying the assumption on the AOT contribution of the lowermost atmospheric layer (below the overlap height), i.e., for the layer of 1 km which is not covered by lidar, and please quantify then also the error in the lidar ratio estimates. Furthermore, at 1064 nm the backscatter coefficient solution is very insensitive to the lidar ratio input, but very sensitive to the reference value. This means that the uncertainty in the 1064 nm lidar ratio can be very large, when the AOT of the AERONET Cimel is matched within a given AOT range (+/- 0.1). I speculate the error is already in the range of 100% for the column lidar ratio caused by uncertainty in the 0-1km AOT contribution. And when one varies the reference value (calibration value in the free troposphere), too, the overall uncertainty will be higher by another factor of 2, I could imagine. What reference value did you use for the different wavelengths? Please state!. At 1064nm this reference value should then be varied by plus/minus a factor of 10 in the uncertainty analysis. In this way, the uncertainty in the backscatter coefficient profile and in the column lidar ratio can be easily quantified, as well as the consequences (uncertainties) for the Angstrom exponents.

Page 11, line 6: I trust the lidar ratio at 355 nm (80sr), and also at 532nm (70sr), but I do not trust the 1064nm lidar ratio. What is the uncertainty here (see discussion above)?

Page 11, line 20 to page 12, line 16: All statements are speculative without uncertainty numbers, for the Angstroem exponent and the Delta Angstrom value. Please provide uncertainty numbers and then a save, tentative argumentation, avoid speculation. Volume depolarization ratios are at all 1% or lower. Such low volume depolarization ratio (even at 355nm) do not indicate the presence of coarse dust particles. And the trajectories do not support any significant transport of Saharan dust to Lecce. So all this is speculative, the scientific value of the entire discussion here is close to zero.

Page 13, from line 20 downward: again , just speculations without uncertainty values.

Figure 2: The extinction coefficients are large and pronounced in the layer above 3 km, and this layer has sharp lower edges and pronounced variations with height. Can such structures be preserved after almost 10000 km of air mass transport? The Angstroem exponents show a systematic trend (monotonic increase) with height, seems to be a bias. Please provide the respective uncertainty analysis. To be clear, I do not believe in any of the shown results in this figure. The reader needs uncertainty ranges for all parameters so that he/she can draw own conclusions from the presented plots.

Figure 3: The graphical framework is convincing, but the lidar results are not. Uncertainty bars have to be provided in the revised version.

Page 14, line 16: The use of the altitude independent lidar ratio is just ANOTHER weak point of the analysis, not the only one! The rather uncertain 1064nm backscatter retrieval is the main error source. And as already said, I do not believe that desert dust was dominating the optical properties in the lower troposphere, so lidar ratios around

55sr are not justified (may be just one option of several) , and the use of lidar ratios of 88sr, 75sr, and 55sr for heights above 2.5km is just playing around with lidar ratio values, nothing else.

Page 15, from line 23 to the rest: I stop reviewing the rest of the paper. Again, the scientific value is rather low, most remaining parts contain speculations only. We need a robust uncertainty analysis! This is the main message of this review. Without that, the paper has to be rejected.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18535, 2013.