

Interactive comment on “Retrieval of height-temporal distributions of particle parameters from multiwavelength lidar measurements using linear estimation technique and comparison results with AERONET” by I. Veselovskii et al.

Anonymous Referee #1

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The paper describes the results of a microphysical retrieval from Raman lidar profile measurements for two night’s worth of ground-based lidar measurements using the Linear Estimation retrieval method. The paper also shows a comparison with near-coincident column retrievals from AERONET. A previous paper introduced this retrieval method and showed its value and potential for important aerosol characterizations, so the current focus on validation and detailed results is appropriate. This paper shows

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only a single case study. The retrieval instrument and the validation instrument are co-located and presumably both operate more or less continuously, so there should be many cases to choose from. It’s not clear what makes this case special and why there couldn’t be more. Further, the validation comparison is made with AERONET, a valuable and important dataset because of its availability and global reach, but which also depends on strong assumptions and is somewhat incompatible with Raman measurements due to differences between day and night and column-total vs. vertically resolved measurements. While I don’t disagree that the comparison is useful, these limitations mean it is not really a validation or even quantitative. Comparisons with in situ measurements (or both) would give better insight into the validity of the LE retrieval, if there is any possibility of obtaining coincident in situ measurements. The comparisons shown here do not reveal any new insights about the LE retrieval that were not already mentioned in the 2012 paper by this group, and the current paper does not suggest that there has been progress to improve the weaknesses discussed there, such as the lack of a non-spherical dust model and lack of spectral dependence of the refractive index. While I believe that the material that is included in this paper is high quality, I feel that the content makes only a relatively small incremental contribution to the literature on lidar microphysical retrievals. The paper would be more effective if enhanced with either more direct, quantitative validation or a larger sampling of results with more variation in aerosol properties.

Specific comments

Pg 3062, line 19-20 “numerous issues . . . should be resolved”: In fact, the current paper does not resolve, or even address, these issues. However, prior work by the same group suggests that these issues, e.g. non-spherical particles, are being addressed, so the statement seems unnecessarily pessimistic. It might be better to say something like “the strengths and limitations of microphysics retrievals should be further explored.”

Pg 3062, line 24-25 “to determine realistic uncertainties”. Similarly, this paper does not give any estimate on uncertainties based on comparisons with measurements. A

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quantitative comparison between the lidar retrieval and another instrument would be an estimate, but no quantitative comparison results are given. In fact, I believe this comparison doesn't support a quantitative estimate, due to the lack of better coincidence (AERONET is only for daytime and the retrieval is given only for nighttime).

Pg 3064, 15: Explain i in " $(i=1)$ ", " $(i=2)$ ", etc. Is this the moment of the distribution? Also, replace i here or in Equation 1 with a different symbol, since they refer to different things.

Pg 3065, 18: Explain what the tilde indicates. Doesn't it indicate an estimate? If so, then the wording "N estimates of \tilde{g} that we compare" should probably be changed to "N estimates of g that we compare". But also, it's not really N estimates of a single value, but single estimates of N different values, so it should be rephrased.

Pg 3065, 18-25: discussion of discrepancy. In Eqn 4, it appears that discrepancy is a function of the refractive index m , but the discussion seems to imply that it's a single selected minimum discrepancy. Please rephrase to clarify.

Pg 3065, 23-25. "Normally a high discrepancy points to problems in the measurements". I'm interpreting this to mean an instrument problem or excessive noise in the measurements, but it seems like it could also point to a problem with the assumptions in the retrieval not being compatible with reality in a given case, particularly the refractive index.

Pg 3069, 1-12, Klett method. I am very confused by this discussion. In the first paragraph you say you use the Klett method instead of the Raman method to produce the extinction data shown in the figure. In the second paragraph you say the extinction data in the next figure is from the Raman signal. However, the data in the two figures look the same with the same amount of detail. So should I infer that the Klett method and not the Raman method is used to calculate all the extinction data that are used in the microphysics analysis? This is really unexpected to me. Even using the Raman method to get a reasonable value of the lidar ratio, using a constant lidar ra-

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tio for the whole region means that your backscatter and extinction data are not at all independent. I guess this would have serious implications for the multiwavelength retrieval, which has limited information content even in the best case of truly independent measurements. Do the results from previous papers by your group, for instance those discussing the use of 1 vs. 2 extinction channels quoted on page 3068, line 6, apply in this case? Can you discuss what sort of errors the assumption of constant lidar ratio might introduce? On the other hand, if you are indeed using the Klett method, then why not go further and make an attempt to do the retrieval during the day at the time of the AERONET measurement using the same lidar ratio, to improve the coincidence for the comparison?

Pg 3069, 20, "oscillatory in the region characterized by low particle extinction". Please discuss briefly what mechanism explains the oscillatory behavior. Is it a propagation of noise in the measurements; is it triggered by gradients, etc.?

Pg 3070, 6, "the uncertainty of the relative change in the parameters should be lower". Please give some more discussion or a reference to support this statement.

Pg 3070, 27-28, refractive index is lower. Can you say what this implies about the aerosol, for example, more absorbing or less absorbing? Is it likely to be due to a difference in relative humidity or different species in the intruding air mass, etc.? Also, what difference in the measurements is driving this difference in the retrieval? Does the Raman retrieval exhibit any change in lidar ratio in this region?

Pg 3071, 12, "To validate the retrievals". I'm not convinced this comparison is a validation, for several reasons. I agree that the retrievals do look consistent; however, that's as much as you can really say. First, the AERONET data is also from a retrieval, not a direct measurement, and the assumptions required by AERONET (such as a single refractive index for all aerosols) could potentially cause significant errors with respect to "truth". Second, the differences in measurement strategy between the two systems, column vs. profile and daytime vs. nighttime, are important. The latter in particular has

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prevented you from making any quantitative estimate of error in the retrieval system. A true validation should probably produce a quantitative result.

Pg 3072, 1-2, spectral dependence of the refractive index. The microphysical retrieval does not allow for spectral dependence at all. That should probably be mentioned here.

Pg 3072, 15, "lidar retrievals are less sensitive to the coarse mode". Is this because of the measurement wavelengths, or because of the retrieval assumptions? Would using two wavelengths of extinction instead of one improve the sensitivity? What about using a non-spherical particle model? The second is only relevant if the coarse mode is expected to be dust. Is it, in this case?

Pg 3072, 9-20, volume density comparison. Why is this comparison only for the first day whereas the refractive index and effective radius comparisons were made for both days?

Technical comments

Pg 3065, line7. "The limitations on the range of parameters variation" = "The ranges of allowed values for the parameters".

Pg 3065, 17, "Graaf" = De Graaf.

Pg 3066, 27-28, "range of m_i variation" = "range of variation of m_i ".

Figure 1. The caption says the third value of the imaginary refractive index is 0.03, but the annotation in the figure says 0.05.

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