Atmospheric
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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.

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We would like to thank the reviewer for an extended and detailed comments which, we hope, helped us to improve the manuscript.

Below we put our responses:

1. "The retrieval instrument and the validation instrument are co-located and presum-

ably 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."

The instrument is not automated and requires two operators in attendance due to aircraft safety issues. Therefore we take measurements only episodically. We tried to choose a couple of long-term night measurements with high aerosol loading and high PBL top.

2. "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."

Yes, we agree that such comparison is not a straightforward and in revised manuscript it is called evaluation instead validation. Still such comparison is useful because it demonstrates that lidar derived particle parameters are very reasonable at least.

3. "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."

Yes, comparison with in situ is important, but these were not available

4. "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."

In the revised manuscript we added new material concerning comparison LE with regularization. Comparison of inversion of full and reduced (extinction at 532 nm is re-

moved) input data sets is also added. The spheroids are included in the new version of LE code but we didn't touch this question here, because particle depolarization is below 5%. Application of LE for dust measurements is in progress and will be presented as a separate manuscript.

5. "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."

In the revised manuscript we added new material including:

i. Modeling. The following scenario was considered to support analysis of our measurements cases: fine mode dominates PSD and its modal radius varies with range, while coarse mode is fixed, the particle number density is constant. As a result, the effective radius of PSD varies from 0.17 mcm to 0.5 mcm. For this scenario the synthetic input data sets were generated. The cases of error free data and data with added 10% noise are considered. The particle properties are retrieved using regularization and linear estimation techniques from 3+2 and 3+1 data sets. The results presented demonstrate that LE inversion is more stable and that estimation of the particle properties from 3+1 data set is possible.

ii The time sequences of particle parameters on 21 Aug are retrieved with regularization and linear estimation techniques from 3+2 and 3+1 data sets. The results presented demonstrate that time sequences derived with all four methods are consistent.

So we believe that these revisions improved the scientific quality of the manuscript and revised version will meet the journal criteria.

Specific comments

6. "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.""

Accepted and corrected

7. "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 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)."

In this paragraph we say that comparisons of lidar measurements with independent collocated instruments are necessary. It is general statement but we don't mean that all this will be done in current paper. In the revised manuscript we added numerical simulations which give some estimates of uncertainties.

8. "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."

Index "i" just shows different bulk parameters. In Eq.1 indices "i" are removed.

9. "Pg 3065, 18: Explain what the tilde indicates. Doesn't it indicate an estimate? If so, then the wording "N estimates of ẼIJg 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. "

Tilde means that the estimates of optical data are obtained for different initial guesses about inversion interval and complex refractive index. So for every initial guess we get set of estimated input optical data (N values), which are used for discrepancy calculation. Corresponding paragraph in manuscript is rephrased.

10 "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."

Discrepancy is a function of refractive index and inversion interval. This is why we can estimate these values from discrepancy minima. Corresponding paragraph in manuscript is rephrased.

11. "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."

Reviewer is right, it may happen for some special cases. But normally the range of considered values of refractive index and inversion intervals allows to get discrepancy of several percents. From our experience, high discrepancy is related mainly to the instrument (or measurements) problem.

12. "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 ratio 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,

C2405

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?"

We don't use Klett method for data processing. The only reason we show it is to illustrate aerosol layer structure, because in Klett method the height resolution is 7.5 m and extinction profile can be shown to higher altitude. But we see, that it confused all three reviewers, so in revised manuscript we introduced a special comment explaining it. Yes, it is possible to show just range corrected lidar signal but we think that Klett extinction is more illustrative.

13. "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.?"

We believe that this is the propagation of noise. Corresponding comment is added.

14. "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."

The LE technique can provide the biases in the retrieved parameters due to existence of "null space" or due to choice of "search space" for radii and refractive index. These biases (especially for volume density) shouldn't change much for small variations of particle radii. This comment is added to the text.

15. "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 airmass, etc.? Also, what difference in the measurements is driving this difference in the retrieval? Does

C2406

the Raman retrieval exhibit any change in lidar ratio in this region?"

The lidar ratio at 355 nm in this region is enhanced from ∼60 sr to ∼70 sr and decrease of mR is probably due to water uptake by the particles. Unfortunately the sensitivity of our technique didn't allow us reliable detection of corresponding increasing of the particle radius. This fact is supported by simulations which we added to the revised manuscript. Corresponding discussions are also added.

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

The reviewer is right, our comparison with AERONET can't be considered as validation, so in revised manuscript we change it for "evaluation".

17. "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."

We put corresponding comment in the section describing the algorithm.

18. "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?"

The longest wavelength in the measurements is 1064 nm, so corresponding backscattering kernels are insufficient for full characterization of the coarse mode particles, especially when the fine mode dominates. Adding of extinction at 532 nm doesn't improve the situation. The particle depolarization ratio in our measurements was below 5%, so the contribution of irregular particles was low and the use of spheroids model in the situation like this has no advantage.

19. "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?"

For the second day the PBL was below 2 km, so interpolation the profiles to the ground introduces too high uncertainty, so these results are not presented. In the final version of manuscript we removed corresponding figure and added comments.

Technical comments

Corresponding revisions are introduced in the manuscript.

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