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Comment

# ***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.

Our responses:

1. "Regardless of this nice work the paper cannot be accepted in its current version.

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It offers incremental additional information over the literature that has already been published in this field. The main point I have to criticize is that the authors miss out on a great chance of presenting a comparison of results if they apply their method not only to this so-called 3+1 data set but also to the 3+2 set which seem to be available for the two measurement examples. I could not find a comment in the manuscript in which they state that they did not measure the second extinction coefficient. I could not find a justification why they do not use the second extinction value for their study, which would then make a useful sensitivity study. The authors say (imply) that processing data of 3 backscatter and 1 extinction coefficient may (often) be equivalent to results from the retrievals that use 3 backscatter coefficients and 2 extinction coefficients. If that is the case, why don't they show it in their paper? The paper in this sense reads more like an advertisement of "this product is better than the other product" rather than showing results that either convince or at least quantify the differences in using 3 backscatter and 2 extinction or 3 backscatter and 1 extinction coefficient. This is even more important in view of the fact that a very limited set of data was processed: 2 cases. It may sound a bit unfair if I say: "one case is no case and two cases are still not sufficient" to convince the reader of the usefulness of the methodology. From following the literature in this research field I understand the difficulties the inversion community is confronted with since its beginnings: they often rely on case studies as comprehensive sensitivity studies are either time consuming and/or the studies cannot take account of all the problems that come with experimental data."

We accept the critics of reviewer that comparison of regularization and linear estimation technique is necessary, and that results obtained from 3+2 and 3+1 data should be compared also. 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  $\mu\text{m}$  to 0.5  $\mu\text{m}$ . For this scenario the synthetic input

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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.

2. "Aside from this mandatory upgrade of the content of the paper: 3+2 versus 3+1 retrievals including a reasonable discussion of the differences and similarities of the results, I am missing some other information that could further illustrate the merit of the method: please show correlation plots, show backward trajectories of the measurement examples that justify and/or corroborate your assumptions and results. "

To the revised manuscript we added the comment concerning the particle origin basing on backward trajectories.

3. "Please show your results not only for a few limited imaginary parts (page 3066: 0.01, 0.02, 0.05). "

We think that the range of the imaginary part 0-0.05 is quite representative for real aerosols. Sure there are specific cases where it can be higher, but Goddard AERONET for gives  $m_i < 0.01$ , so at a moment we prefer to limit our consideration by this range.

4. "Why do the authors show these specific measurements? Are there other data available for comparison? "

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

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choose a couple of long-term night measurements with high aerosol loading and high PBL top.

5 ".A comparison to AERONET is useful but insufficient (see also my criticism regarding the AERONET data products)."

We consider it as a first step and it is definitely useful. But we agree that comparison with AERONET can't be considered as validation. In revised manuscript we call it evaluation.

6. "The discussion on page 3068 lines 3 – 16 is pointless unless results of 3+2 retrievals are shown"

3+2 retrievals are added to revised manuscript.

7. "Line 23, page 3068: "sulphate particles" is speculative. Show trajectories and give more proof of this assumption. Otherwise please keep your assumption more open regarding what "type of aerosols" you observed."

"Sulfate particles" are removed.

8. "Page 3073, line 9: you speak of validation f your results with AERONET data. This is incorrect. There is nothing like validation among methods that use the same methodology: in this case remote sensing and "inversion". Furthermore I would like to remind that AERONET has never been validated regarding its own results. We simply assume that AERONET retrievals are correct. I definitely want to give credit to the great job the AERONET team made since AERONET came into existence 20 years ago. But "validating" your results to another methodology that was never validated itself, this is a "ring closure". You may speak of comparison and consistency checks, if you like to do so."

We changed validation for "evaluation" and "comparison" in revised manuscript.

9. "Figures: Fig 3: I do not understand why decades after Raman lidar came into

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operation extinction coefficients using the Klett method are shown. The plot looks nice, but does not add anything to the original purpose of the paper. Show a range corrected backscatter signals. That will do the job, too, particularly as you show the MEASURED extinction coefficient on Figure 4."

The idea was just to illustrate the structure of aerosol layers with high height resolution. We don't use Klett in retrieval. Corresponding comment is added to the text. Still we think that Klett extinction is more illustrative than range corrected signal, so we prefer to keep it.

10. "Please explain why the extinction shows a lot of structure (quite a bit of color changes in Figure 4) but the effective radius does not change significantly? It is hard to believe that effective radius is that "stable". I also do not believe that there is compensation process between refractive index and particle volume, which could justify that particle effective radius is so constant during measurement time. Keep in mind that your search space of the refractive index is limited and very coarse. Please comment on this possibility that ht search space has significant influence on the results presented here. Let me ask directly: how sensitive is your method with regard to effective radius? Could you provide error bars?"

In the revised manuscript we added new material, showing that 3+1 set loses some sensitivity to the variation of small particles (with effective radius below  $0.2 \mu\text{m}$ ). This may be one of the reasons why retrieved time series of effective radius look smooth. Still we observe increase of the radius during the night, which correlates with decrease of the real part of the refractive index. We estimate accuracy of the effective radius retrieval as 30%, it includes the possible biases due to the choice of "search space" for refractive index and inversion interval. Corresponding comments are added to the manuscript.

11. "Figures 6 and 7: I make the same comments as for Figure 3 and 4. Replace Figure 3 with a realistic range corrected backscatter signal, and please explain why the

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effective radius in Figure 7 does not change at all though extinction changes considerably?"

We think that increase of extinction is due to the rise of the particle number density mainly, though small increase of effective radius is also observed. As we have mentioned 3+1 set loses some sensitivity to small effective radii.

12. "Figure 4 and 7: show your results also in terms of correlation plots, please. That might give some indication where the problem of insensitivity of effective radius lies."

It is not very clear what correlations the reviewer is referring to. The increase in extinction can be explained by the increase in number density as mentioned above and we do not believe that this issue requires another figure to clarify.

13. "Fig 10: error bars are missing."

Added

14. "Fig 11: this comparison bears little significance unless you explain how you manage the overlap issue and how you deal with different measurement times and observation geometries of the two instruments."

We agree. In the revised manuscript this figure is removed and the results of column volume comparison are given in the text.

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