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Atmospheric Measurement Techniques
Discussions

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 #2

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The paper reports on retrievals of parameters of aerosol size distributions on the basis of the linear estimation technique. The authors outline why this method is useful for analysis of multiwavelength lidar data and emphasize that this method may serve as, or even could become the complementary tool for the much more complicated methods of data inversion that are currently used by several groups for analyzing their multiwavelength lidar data. The authors themselves have ample experience in the in-

version methodology and thus use this additional method as an alternative approach. The authors show measurement examples and compare their results to results from observations with AERONET sunphotometer.

The paper is well written, easy to understand and it clearly points out to the advantages of the linear estimation method compared to the more complicated "traditional" inversion methods that are used by the lidar community for inferring some of the microphysical parameters of interest.

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

However, the paper by De Graaf and Donovan, Applied Optics, already is an example of missing out on the opportunity of providing more comprehensive information on the applicability of the linear estimation method. Particularly in view of the fact that the satellite community is eager to use more simple methods like the one presented here over the more complicated inversion methodology.

Given the fact that the original, basic idea of the linear estimation method dates back to the mid 1990s I find it necessary that more detailed work and results are written down in any new paper on this methodology, 15 years after the first paper by Donovan et al. in this research field. Authors who have such great experience as Dr. Veselovskii certainly has can perform the additional simulations/data analysis for these two case studies. He does not need to jump into the tantalizing work of comprehensive simulation studies with synthetic data and/or large sets of experimental data, which would go beyond the purpose of this paper. The publication of this paper would not be delayed by more than a few months.

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. Please show your results not only for a few limited imaginary parts (page 3066: 0.01, 0.02, 0.05). This approach oversimplifies this study in a way that simply forces "a good outcome" of the study. De Graaf and Donovan already made a serious shortcut in their study by using oversimplifying aerosol models. Their study thus only showed that the method works with, to my personal opinion artificial aerosol models, rather than with "real world" aerosol models. This paper here can correct for this missing link. Why do the authors show these specific measurements? Are there other data available

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

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

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.

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.

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

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?

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

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.

Fig 10: error bars are missing.

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.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 3059, 2013.