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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of aerosol backscatter, extinction, and lidar ratio from Raman lidar with optimal estimation" *by* A. C. Povey et al.

Anonymous Referee #2

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GENERAL COMMENTS

The authors present an optimal estimation technique applied to calculate aerosol extinction and backscatter coefficient profiles from Raman lidar measurements. They use simulated signals as well as real-world observations to demonstrate the applicability of the method and discuss the related errors. The manuscript presents a new approach and is therefore suited for publication in AMT. The paper lacks some critical assessment of the methodology in view of the range of realistic measurement conditions and the applicability to different instruments. There seems to be not much experimental experience with respect to aerosol observations behind the discussion. I agree with most of the points raised by V. Shcherbakov and anonymous referee #1, and I will not repeat everything here. Below, there are additional points that should be considered in

a revised manuscript.

SPECIFIC COMMENTS

Page 9298-9300, Introduction: I find the introduction rather misleading. First of all, the view on the state of the art of aerosol lidar measurements is rather narrow. Saying that "lidar is not as widely applied as other techniques…" does not adequately reflect how much lidar has contributed to our current knowledge on the spatio-temporal distribution and the properties of aerosols in the atmosphere, in particular on their vertical distribution. Which "other techniques" have provided more information? The given references seem to be quite arbitrarily selected, e.g., citing Vaughan et al. (2004) published two years before the launch of CALIPSO when there are dozens of papers available on the successful CALIPSO mission after seven years in space, including very comprehensive overview papers, is just not appropriate. In addition, there should be a better introduction to the kind of retrievals available for the derivation of 1) optical aerosol properties and 2) microphysical particle properties. In the current version of the manuscript, these two things are just mixed up. It does not become clear, e.g., that the conventional Raman lidar approach for the calculation of extinction and backscatter coefficients (called Ansmann method here) is an analytical solution of a relatively simple problem represented by two equations with two unknowns. In contrast, the retrieval of microphysical parameters always requires the numerical solution of a non-linear ill-posed problem. As already stated by V. Shcherbakov, the argumentation why we need a non-linear regression technique and why this is a "modern retrieval theory", if an analytical solution to the problem, including analytical error propagation, is available (which is obviously considered as old-fashioned by the authors), is not very convincing. Throughout the paper I miss a critical discussion on how much the retrieval depends on the a priori and what happens in more complex atmospheric situations than the rather simple PBL + free troposphere scenes considered here (e.g., Saharan dust outbreaks, lofted layers, smoke plumes, smog situations etc.).

Page 9302, line 14: "…C(R), known as the overlap function…" This should be C(R)

= K O(R), with K being the absolute calibration factor and O(R) the overlap function. Later on, you speak only about the calibration factor. E_L is usually included in K, but obviously you consider E_L as varying, in contrast to other system parameters (e.g., transmitter and receiver efficiencies). You should be more specific in the explanation here.

Page 9303, second paragraph: Again, the retrieval of optical and microphysical properties is mixed up in the discussion.

Page 9304, lines 5-21 and later occasions discussing photon counting and correction of dead-time effects: "Lidar use photomultiplier tubes (PMTs) as their detector..." This is not solely true, lidar also use other detectors (photodiodes of different kind). The description of the principle of photon counting and analog detection is not very clear and sounds a bit unaware, e.g., what is a "current spike" (usually we measure a voltage pulse), why does discrimination depend on number of photons per bin? First, phi is defined as number of counts, and then it is said that phi is proportional to E for analog operation... In the retrieval, only photon counting is considered, although it is mentioned that the CUV lidar measures both photon-counting and analog signals. These should be usually "glued" and non-linearity in photon counting should be avoided, at least in the respective height ranges where photon counting is appropriate. Lot of discussion is spent on the correction of dead-time effects, which obviously produce one of the largest errors in the retrieval. It is even said that this is not adequately considered in other publications. How can the authors judge that? To my feeling, the whole discussion results from inappropriate parameter setting and operation of the data acquisition system of the used lidar. For a well-adapted data acquisition system this kind of errors should play only a minor role.

Page 9305, Eq. (10): What is B?

Page 9305: Be careful with the usage of N; it is defined as the molecular number, isn't it? But then it is used as a synonym for the particle density.

Page 9306, line 4: Negative values are "natural" when the signals are noisy and the truth is close to zero. Setting all negative values to zero will cause a positive bias, e.g., when calculating layer-mean or columnar values such as aerosol optical depth.

Page 9307, line 7: What does a five-bin average mean? There is no physical measure behind. A bin can be of any length, depending on the system and the parameter settings of the acquisition.

Page 9307-9309, Sec. 2.4: This section starts with the sentence: "Arguable the most important component of an optimal estimation scheme is its a priori." Having that mind, I am rather skeptical about the approach described in the following which obviously (over)simplifies the state of the atmosphere. A simple PBL + free troposphere layering is assumed. The scale height is derived from backscatter sondes launched at background stations known for very clean conditions (Wyoming, New Zealand, Greenland; Fig. 3), and a model-derived, rather narrow distribution of extinction, backscatter, and lidar-ratio values for continental-type aerosol is pre-defined (Fig. 2). This a priori may be suitable for the few measurement cases investigated here, but it has nothing in common with the most interesting aerosol scenes that can be found in the global atmosphere, e.g., thick dust plumes (which can reach heights of 6-10 km), heavy pollution situations (which may exhibit extinction coefficients of 1-5 km-1), forest-fire events (which may produce highly absorbing aerosol with lidar ratios >100 sr) etc. Other authors came to the conclusion that the highly complex and varying vertical aerosol conditions in the atmosphere do not allow the formulation of an a priori based on absolute values and have used derivatives instead (see, e.g., Lopatin et al. 2013). A more critical discussion on the general applicability of the a priori is required. It should be discussed if the a priori must be re-formulated depending on the observed situation. Also, more realistic data sources (e.g., global datasets from CALIPSO, lidar network observations) should be considered for the determination of the a priori.

Page 9309, line 25: What is RACHEL? Should the reader know about?

Page 9309-9315, Section 3: The simulations presented here are very close to the a priori. Thus conclusions can only be drawn for these simple situations. It would be more interesting to see what happens in cases of more complex atmospheric layering, with lofted layers in the free troposphere etc.

Page 9311, line 25: "These gives. . ." ?

Page 9312, line 2: ". . .the error caused by the improper dead time correction. . ." What does it mean? How was it simulated?

Page 9312, lines 3-6: Is 300 m effective resolution true for the entire height range? Fig. 7 indicates that the resolution decreases with height. Why isn't a progressive smoothing used for the conventional solution as well? And shouldn't there be different smoothing lengths for the different products (according to Fig. 7)? That would make the results better comparable.

Page 9312, lines 21-22: "The Ångstrom coefficient. . .is commonly accepted to lie in the range 0.5-1.4. . ." First of all, this is not a coefficient (in the sense of backscatter and extinction coefficients), but an exponent or just a parameter. Second, the given range is much too narrow. Large dust particles exhibit Ångström exponents of about 0, whereas small absorbing particles show Ångström exponents of the order of 2-2.5.

Page 9313, lines 10-16: ". . .the system simulated with tau_d = 50 ns. . ." That is very unrealistic indeed, and it is unclear why such a value is discussed at all. Typical photon-counting systems have dead times of < 5 ns. As mentioned previously, this dead-time discussion is not very convincing.

Page 9313, lines 17-20: It did not become clear from the previous discussion that the algorithm works with an absolute system calibration. This is quite unusual, in particular for a system working at 355 nm for which a Rayleigh calibration of the signals should work without problems. As already stated above, the use of C(R), O(R), and E_L should be more explicitly explained. It should also be discussed why Rayleigh calibration is

not used.

Page 9314, lines 19-24: Again, if this plays a role, it just indicates that the system has not been well characterized and set up. Characterization of the dead-time performance belongs to the quality control of the measurement. Operating detectors under (strongly) non-linear conditions is just not acceptable. Slight corrections are necessary, but it should not be a major task of the algorithm to account for detector non-linearity effects.

Page 9315, lines 2-3: It was clear from the beginning that the a priori distributions do not cover the natural variability of the parameters (see comments above).

Page 9315, lines 6-7: The measurements are done with the CUV? But the simulations were performed for RACHEL? These are different systems? How is this considered in the simulations/retrievals? For which of them does the dead-time discussion hold?

Page 9315, line 16 and page 9316, line 5: "Six profiles were selected from March 2010. . ." and "The radiosonde that morning. . ." The first formulation implies that the profiles are distributed over the month, but then it seems it is a series of measurements from the same day. Actual observation times must be given here.

Page 9315, line 26: ". . .averaging kernels, which widen from 30 to 100 m." For which parameter, extinction and/or backscatter?

Page 9315, lines 5-6: ". . . a step decrease in pressure. . ."? Really? Probably, you mean temperature?

Pages 9315-9316, Figure 12: Why the extinction profiles are cut? They seem to go up to high values in some cases. At the same time, the maximum values of backscatter remain nearly constant. That will give a highly varying (unrealistic) lidar ratio. There seems to be a large influence of the overlap correction, i.e., the systems appears to behave very unstable.

Pages 9316-9317, Figure 14: Same as in Figure 12; overlap correction seems to be inappropriate, leading to a strongly increasing lidar ratio from top to bottom, which is not expected considering the well-mixed conditions indicated by the backscatter.

Page 9317, Figure 15: Same as before; the high extinction in the low heights leads to too high AODs (please explain, how the AOD is calculated). Even if not the complete column is covered, the lidar gives higher values than the sun photometer. The opposite would be expected. The agreement is not impressive at all. There is a difference of a factor of 2 over large periods. Also, the difference between the conventional and the new method is rather large. If the oscillations were due to the laser energy, they shouldn't occur in the conventional solution. Again, it becomes clear that the need for absolute calibration is a deficit of the method.

Pages 9318-9319, Eyjafjallajökull observation: The discussion is not convincing and the results cannot be judged as long as it remains unclear what kind of depolarization is shown. Conclusions can only be drawn if the calibrated particle linear depolarization ratio is provided. I can hardly identify a layer, at least not after 10 UTC. The higher depolarization seems to be connected to the gap between the mixing layer and the residual layer (isolines of depolarization in the lower panels of Fig. 16 may help to avoid this impression).

Page 9318, lines 23-25: The lidar ratio is strongly shape dependent. Furthermore, the processing of particles would be rather due to sulfate coating than humidity growth (beside the ash, there is a lot of sulfate produced from volcanic SO2). The particles are large in any case. Thus I would speculate it's the shape change that leads to the lidar ratio decrease. But anyhow it's speculation.

Page 9319, line 6: "…decrease with height…" Do you mean decrease with decreasing height?

Page 9319, lines 8-9: "…it does not seem that sufficient time has passed…" Time since when? What would you expect, what time would be needed?

Pages 9320-9321: Conclusions should be adapted according to the comments above.

TECHNICAL CORRECTIONS

Page 9301, line 7: "Jacobean" should be Jacobian

Table 1: Units are missing.

Fig. 2: Explain the black line.

Fig. 8: X-axis titles should indicate the error of the parameters instead of the parameters.

Fig. 8, caption, third line: fvs. -> vs

Fig. 8, caption: Clarify that correlation and intercorrelation is for the errors, not for the parameters.

Fig. 9 and 10: Provide information on vertical resolution.

Fig. 11: Lines are too thin, nearly invisible.

Fig. 12: Very hard to distinguish the lines. Indicate vertical resolution. Why are the extinction profiles not completely shown?

Fig. 13: as for Fig. 8, clarify that errors are shown.

Fig. 14: Check superimposed (a), (b), (c) on y-axis titles.

Fig. 16/17: Check reference to Fig. 17 in the text. Y-axis title is missing.

REFERENCES

A. Lopatin, O. Dubovik, A. Chaikovsky, P. Goloub, T. Lapyonok, D. Tanré, and P. Litvinov: Enhancement of aerosol characterization using synergy of lidar and sun-photometer coincident observations: the GARRLiC algorithm, Atmos. Meas. Tech., 6, 2065-2088, 2013

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 9297, 2013.