Atmos. Meas. Tech. Discuss., 4, C2618–C2624, 2012
www.atmos-meas-tech-discuss.net/4/C2618/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Linear estimation of particle bulk parameters from multi-wavelength lidar measurements" *by* I. Veselovskii et al.

Anonymous Referee #1

Received and published: 12 January 2012

General comments:

This paper demonstrates a retrieval of bulk particle properties such as effective radius and particle volume density from multi-wavelength lidar measurements. This retrieval has an advantage in computational speed and stability over the inversion-with-regularization approach to microphysical retrievals from lidar measurements. In addition, it seems more straightforward to understand (at least to this reviewer), which is another nice boon. The improved speed and stability are advantages bought at the cost of not retrieving the particle size distribution, by casting the retrieval equations such that the matrix inversion (the most computationally expensive step) is done on the matrix $\mathbf{K}\mathbf{K}^T$ rather than $\mathbf{K}^T\mathbf{K}$, that is, on a square matrix with rank equal to the number of measurement channels instead of a square matrix with rank equal to the number of bins in the modeled size distribution.

Not producing a particle size distribution may prevent the use of this retrieval in some applications, but the gains should outweigh this consideration in terms of overall usefulness of the proposed retrieval. The high computational cost and labor intensiveness of the inversion-with-regularization method seem to be the primary hurdles limiting wider use of that method. The demonstration of the greater speed of the linear estimation method is important and shows that this method should enable the retrieval of large data sets of bulk particle properties which could open up opportunities for further validation (for example by comparison with in situ measurements) and possibly for use by modelers or others in the scientific community. In particular, the computation speed of this algorithm seems to be of the right order of magnitude to be useful for future satellite or airborne multi-wavelength lidar instruments. The demonstration of the retrieval for a reduced set of inputs $(3\beta + 1\alpha)$ is also quite useful, although it would be even more useful and interesting if additional combinations of channels were demonstrated.

A disadvantage of this method is its dependence on Mie modeling which is only appropriate for spherical particles. If the authors were not able to remedy this, it would be a serious disadvantage, but it appears that this concern is already being addressed by the addition of a treatment of non-spherical particles. Hopefully, the results of that effort will be published in the near future.

The direct comparisons with the inversion with regularization approach are relevant, and the authors do a good job of presenting the strengths of the LE approach in comparison with inversion with regularization, but this algorithm is not actually new. The key pieces of the algorithm seem very similar to those of Donovan and Carswell [1997] and de Graaf et al. [2009] whom the authors cite. Ideally, another sentence or two would be added to more completely cite earlier stages of the development of the algorithm, for example Thomason and Osborn [1992]. But most importantly, the authors should discuss more clearly how their approach differs from the cited references and

AMTD

4, C2618–C2624, 2012

perhaps refrain from describing the LE technique as “the new method” or “the new technique” unless they make a case that their modifications qualify their version of the methodology as original.

A strength of the manuscript is its organization and clarity. In particular, the algorithm description is direct and transparent, and very clear considering the complexity of the content, probably partly due to the streamlined organization that is not bogged down with details of the particular implementation. However, there are several instances where more discussion of particulars would be helpful. These are mentioned in the specific comments below.

Specific comments:

Page 7503, Equation 5: My understanding of the earlier references is that the principle components analysis is used to invert the $\mathbf{K}\mathbf{K}^T$ matrix, and some of the lesser components are removed to decrease the propagation of error. Is this step relevant to the current work also? Was it tried?

Page 7504, line 13: “The elements of matrix **F** can be computed and stored. . ..” This is true assuming you know how the aerosol properties depend on the volume distribution and how the measurements depend on the volume distribution. The coefficients in **P** for simple choices of bulk properties are given later in the paragraph but the calculation of the kernels **K** requires modeling. Much later in the manuscript there is a brief mention that Mie calculations are used. It would good to have a more concrete discussion of the kernels, or at least a sentence mentioning the Mie calculations, earlier in the paper. This could perhaps be here in this paragraph or when the kernels are first mentioned on page 7502. I realize that this information is covered in earlier papers, but for completeness and understandability it should be described at least briefly here.

Page 7505, line 9: “for example, in our case we use maximum 5 different observations.” This seems to imply that the retrieval is done completely independently for each vertical level or bin. Is this correct? It would be good to address this more explicitly somewhere

AMTD

4, C2618–C2624, 2012

Interactive Comment

in the paper.

Page 7505, line 24: “The use of smoothness or other a priori constraints may require rather sophisticated developments.” So is the smoothing on the input data discussed on page 7516 the only smoothing that’s done in the current methodology? Is there smoothing on the calculated kernels, as done by Donovan and Carswell [1997] and De Graaf et al. [2009]? If there is any smoothing, please describe it in the paper.

Page 7506, line 5: “**F** can be calculated using the detailed size distribution with very large N_v” How many size distribution increments were used in the retrievals that produced Figures 5 and 7? How much accuracy is gained compared to using 5-7 bins as quoted for the inversion-with-regularization approach? Can the gain in accuracy be quantified?

Also related to the size distribution, it seems that there actually is a retrieved size distribution, which could be fairly easily calculated using Eq. (6) with no additional matrix inversions, at least for example cases. It could be informative to see how this distribution compares to the “true” size distributions in the simulations of Section 3, or how it compares to the size distribution retrieved by the inversion-with-regularization approach in the comparison in Section 4.

Page 7509, line 5-8: “. . . we can attempt to estimate $m(\lambda)$ from available observations. Specifically, we can choose one optical data $\mathbf{g}_j^*$ and estimate it from the rest of N-1 data using Eq. (10).” This description isn’t as clear as most of the rest of the paper. From looking at the cited references, I gather that the idea is that the backscatter and extinction are themselves particle properties, so the $\mathbf{P}$ kernels can be chosen to give extinction and backscatter back. That is, if all N optical data were to be used, then $\mathbf{P} = \mathbf{K}$. Is this correct? It would be good if another sentence or two of explanation were added to the paper. Also, it’s probably better to say “using Eq. (9)” rather than Eq. (10), since Eq. (9) represents $\mathbf{p}$, the particle properties, in terms of $\mathbf{g}$, the measurements.

Section 3: Theoretical calculations of the errors are derived in section 2 (i.e. Eq. 21

for the random error), and then error estimates are derived numerically via a sensitivity study in section 3. So, are the errors estimated in section 3 comparable to the theoretical derivation of the errors in section 2?

Page 7514, lines 18 and 22: Uncertainties are given for the retrieved values in Figure 5 (i.e. 0.22 ± 0.055 for effective radius and 1.37 ± 0.05 for the real part of the refractive index). What do these uncertainties represent? Random error or random plus systematic error? Are they from the theoretical calculations in section 2, the variability of the selected 1% subset of the solutions, or something else?

Page 7514, line 19: "The vertical profile of the effective radius obtained with LE oscillates less than the profile obtained with regularization, suggesting a more stable inversion." It seems that the averaging procedure described at the top of page 7510 must be a big factor in the smoothness of the profiles. Page 7510, line 2 suggests that the averaging procedure is the same for both retrievals, so it apparently does not explain why the LE retrieval looks more smooth; this may indeed imply that the matrix inversion itself is more stable. So I'm curious whether single solutions from the LE method are individually smoother than single solutions from the regularization method.

Page 7518, line 9: "removing extinction at 355 nm enhances uncertainties of retrieval". It would be useful to know more about this. This result should probably be mentioned in the body, probably on page 7512, rather than bring it up for the first time in the conclusion.

Technical Comments:

Page 7501, lines 5 and 10 and throughout: Muller should be spelled with an umlaut.

Page 7501, line 19: This sentence seems rough from a grammatical or idiomatic standpoint. I suggest adding "the" before possibility, changing "retrieval" to "retrievals" and changing "to provide" to "of providing".

Page 7502, line 2: Add "and" in the list: "volume, surface density, effective radius, and

AMTD

4, C2618–C2624, 2012

complex refractive index”

Page 7502, line 13: Graaf should be De Graaf.

Equation (9): typesetting error. I believe $\mathbf{F}_\mathbf{g}$ should be $\mathbf{Fg}$. That is, $\mathbf{g}$ should not be a subscript but a separate variable in the product $\mathbf{F}$ times $\mathbf{g}$.

Throughout: minor inconsistencies in the typesetting of the equations, specifically italics and bold. For example, in Eq. (7), the transpose symbol T is shown both in bold and not in bold in the same equation. The variable g is not italicized in Eq. (18) whereas it is italicized elsewhere. Some of the equations (e.g. 19, 20, 21 and 23) have commas after them. Is this necessary? In the case of Eq. (19) in particular, I think it would be better to drop it, since here it looks like a new symbol, v’, has been introduced. Also, the use of p as a subscript in Eqs. (1) and (26) could potentially be a little confusing, given its unrelated use as a variable denoting aerosol characteristics starting at Eq. (8).

Page 7504, line 11: I think it would be better to say “Using Eqs. (6) and (7)”

Page 7508, line 2: I think you mean to compare Eqs. (15) and (21), not (20).

Page 7510, line 9: “minimization of ρ in Eq. (20)...” I think you mean Eq. (26).

Page 7509, line 12: The in-line equation for $\Delta\mathbf{g}_j$ has a typographical error. The minus sign should not be superscripted.

Page 7511, line 18: “the spread in values ... is below 20, 35, 50$\%$ for input errors 10, 20, 30$\%$, respectively”. Even better, to me it looks like you could say below 15$\%$ (not 20$\%$) output error for 10$\%$ input error.

Figure 5: The profile of index of refraction for the $3\beta + 2\alpha$ retrieval is missing. The error bar is also missing. Also, it would be good to mention the error bars in the caption.

Page 7518, line 9: Consider rewording to say “increases retrieval uncertainties” rather than “enhances uncertainties of retrieval”.

AMTD

4, C2618–C2624, 2012

Interactive Comment

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 7499, 2011.

AMTD

4, C2618–C2624, 2012

Interactive Comment