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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Effects of systematic and random errors on the retrieval of particle microphysical properties from multiwavelength lidar measurements using inversion with regularization" *by* D. Pérez-Ramírez et al.

Anonymous Referee #1

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The paper reports on error estimation related to data inversion. The data are acquired with socalled multiwavelength lidar that delivers backscattering at 3 wavelengths and extinction at 2 wavelengths. The authors study two cases, described by a monomodal lognormal distribution and a bimodal lognormal size distribution. The authors consider the case of statistical and systematic errors in the input data. The authors treat the systematic errors as statistical errors and in this way develop a parameterization which allows them to estimate the error in their inversion products from the measurement errors. Main finding, according to the authors is that errors at different wavelengths are

additive and that the measurement error transfer in a linear fashion to the output data products.

The authors provide insight on the error propagation which to my knowledge is the first time that such work is available. I consider the results important in view of the fact that the underlying mathematical equations are nonlinear and that mathematical tools for error estimation in data inversion are needed.

However, the manuscript does not show in a convincing way that a simple error propagation exists. The authors to my opinion wash away important stumbling blocks in their line of argumentation and the way they present their results.

The authors start out with two simple case studies. They use one monomodal and one bimodal distribution. The mode widths they use represent comparably narrow size distributions. I am wondering if this narrowness creates the linear behavior of error propagation and particularly the additive character of the errors of the different data products from lidar. The authors use several refractive indices to compute the optical data that are used in the data inversion. The number of refractive indices is very limited, too, and thus may obscure a non-linear behavior of error propagation. The authors do not show in a convincing manner that the results they obtain can be generalized to the general case in which the size distribution may have any kind of mode radius and mode width. The weighting of the two modes (in the bimodal example) may also be simple a lucky shot. Yet, the authors present in figure 4 their results of the sensitivity study which is admittedly a highly attractive and elegant way. The Gauss-like distributions indicate a rather simple relationship between input and output errors. However, these results are based on two size distribution and a few refractive indices only. Either the authors show in tables in figure that these relationships indeed hold for a broader range of input parameters (of the size distributions), or the authors make it very clear in their paper that these error propagation rules hold for a very limited input data set, cannot be generalized yet and need further investigation. The authors also do not explain in detail how they treated the input errors. A few more details on the gauss error of the input data would be valuable in this context. I am surprised that statistical errors can be treated like statistical error. To my opinion this concept is in contradiction to the theory of statistical mathematics, or it is at minimum an oversimplifying concept with respect to the definition of systematic errors in the presence of statistical error. I may be wrong on my opinion and would be happy to see more explanations and also illustrations that explain in more detail the validity of this elegant assumption. Finally, the errors of the output data products with regard to statistical errors are basically known from previously published literature. Even though the authors point out to this fact by referring to their own work, they should consider a more precise description of the novelty of their work, which is the treatment of systematic errors, which brings me back to my doubt that systematic errors can be treated like statistic error. Or to put it into a simple procedure: why not subtract the systematic error from the mean optical data and do the inversion according to the statistical error only?

Some general comments:

Page numbers and line numbers would be very helpful in the revised version. It would make it easier to refer to points that need to be revised.

Check your reference list: I do not find Tesche et al., 2013 and Wanger et al., 2013;

Sentence: "From an instrumental point of view . . ." I am missing references to Veselovksii et al., 2002 and Muller et al., 2001.

Sentence ". . . data are affected by small random errors": please quantify what you mean by "small"?

Sentence "We will show that the results obtained can also be used to assess the sensitivity of the retrievals to random errors in a new way": I find this part a major weakness of the paper, as the proof of concept is insufficiently described (two size distributions and a few refractive indices only). Particularly: why don't you simply correct for the systematic error and treat the error source of statistical error only?

You use the real part from 1.33 - 1.65 and the imaginary part from 0 . . . 0.01. In how far does this constraint force the error propagation to be linear, and in how far does the refractive index constraint naturally lead to the nice parameterizations of error propagation? Some words on this would be helpful to evaluate the merit of this study.

You use the fine mode radius of 140 nm and the coarse mode radius of 1500 nm for the volume distribution. What are these values for the number concentration? Are these numbers realistic values for size distributions? 1.5 for the fine mode and 1.8 for the coarse mode are at the lower range of numbers for natural size distributions. I suspect that your linear error propagation is in part the result of this serious constraint. If you use broader size distributions you may completely lose the linear error propagation. Please comment on my assumption.

You did not test the imaginary part of 0 in your sensitivity analysis? How would you go around this problem in practical application in which imaginary parts are considerably less than 0.005?

Sentence: "A more depth discussion about limitations of the averaging procedure used here to retrieve accurate values of particle effective radius is in Veselovskii et al. 2013": this paper is not published. I tried to find more information and I see that the manuscript is in the discussion status. Please provide a short summary here.

Sentence "The lowest sensitivities are to biases . . . and 532 nm". I am raising once more the point that the choice of size distribution may lead to this conclusion.

Moreover, many works . . .. found an inverse relationship between the . . .. for low values of the Angstrom exponent". This sentence does not really need references from 2003 and 2009. These findings are considerably older.

Sentence: "but the generality of the results needs to be examined": it is certainly this sentence that bears proof in this paper.

Sentence "The values used as the baseline on . . ..with no induced systematic errors" this means you did not use any errors are all? So again; how representative are your results?

Last paragraph before section 3.2.2: I am a bit confused about this very generalizing comment. I do not find proof in the paper that this is the case.

Sentence: "although different combinations of over/under estimations are allowed" it remains unclear which combinations you really used. It also remains unclear in how far the spectral slopes became distorted to a degree that renders the input data set useless, as they do not represent the real situation anymore.

Sentence "simultaneous biases in the optical data of 1, 2, 5, and 10%": there remains the question in how far the use of 1% and 2 % is already having that much influence on your results that the linear error propagations results from using such small errors naturally occurs. You seem to force the linear behavior of error propagation by using unrealistically small errors. If I understood your final plots you use all results, i.e. 1, 2, 5, and 10% in one plot? If that is the case, then any non-linearity that appears in error propagation, let's say at 10% error might be masked by the results for the other error cases at 1% and 2%? Please comment on this and show the parameterizations for the different error levels separately.

Sentence: "Therefore, we conclude that the results of Table 1 can be reliably used to . . . biased input data". It may be the case that you can transfer the numbers from table one into parameterizations. Nevertheless, you use many assumptions in your retrievals. You use two size distributions, a limited set of error bars which favor low error cases. This to my opinion does not justify that you show the parametrizations without pointing out that this scheme cannot be generalized, if more size distributions are used.

Sentence "We take this result to be an indication that the solutions . . ." part for last paragraph before section 3.3: please show a plot that exemplifies this "local minimum in the multidimensional solution space.

Results of figure 4: you used 50000 biased optical data sets: what did this mean in reality? 1, 2, 5, 10% errors and then you parameterized? As you show your parameterization for up to 100%: What was the maximum ADDED error of the backscatter and extinction values (5 error values) that, for example led to, e.g. 50% and 100% error in the microphysics (as shown in the plots in figure 4)? Did you also test a large error for one input set and no error for the other four sets? Does this parameterization describe this case, too?

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4607, 2013.