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Atmospheric Measurement Techniques Discussions



# *Interactive comment on* "Evaluation of the Lidar/Radiometer Inversion Code (LIRIC) to determine microphysical properties of volcanic and desert dust" *by* J. Wagner et al.

**Anonymous Referee #3**

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## General Comments

This paper describes a microphysical retrieval method for a combination of ground based elastic backscatter lidar plus an AERONET sunphotometer. There is certainly interest in the community in microphysical retrievals from remote sensing, and specifically for vertically resolved microphysical quantities that can only be obtained using active remote sensing. It is important to explore how well this kind of retrieval can be done with these commonly available measurement capabilities, including specifically cases with coarse non-spherical particles that this paper focuses on. For the most part, the paper is well written and includes an appropriate amount of explanation and



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detail.

However, I think the manuscript should be improved in a few key areas. I believe the conclusion should be more specific about both the positive and negative results found in the study. The authors state that one of the goals of the paper is to "evaluate the potential" of the LIRIC method, so they should specify details in the conclusion about when the method is most applicable and when it is less applicable (i.e. the two distinct case studies had different results: why?), what are the strengths and weaknesses, and (if possible) what possibilities are there for improving the method.

Also, I think the data analysis and interpretation of the case studies is somewhat weak, specifically in terms of the lists of potential explanations for discrepancies between the LIRIC and Raman/POLIPHON results. Several possible explanations are listed, but without further investigation that would help to distinguish whether or how much these different explanations contribute, even in cases where it seems like follow-up would be easy.

On a less critical note, I think the flow of the introduction and methodology sections could be improved to make the retrieval much clearer to the reader. While almost all of the information I would want is presented somewhere in the manuscript, the introduction could provide a better foundation for understanding the methodology and results. Also in the introduction, it would be good to add a discussion about how this technique is similar to or different from other microphysical retrieval techniques, such as Veselovskii et al. (2012), Leon et al. (2003), Veselovskii et al (2002), and Mueller et al. (1999).

**Specific Comments**

Introduction (also see general comment above): I think the description of the LIRIC technique could be improved, to better prepare the reader to follow the explanations and results in subsequent sections. Specifically, I think it's important to clarify (1) what the inputs are and specifically that only backscatter (not Raman extinction) lidar sig-



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nals are used for the lidar part, (2) what outputs are provided, and (3) the relationship between the AERONET retrieval and the LIRIC retrieval (that is, that the AERONET retrieval is input to the LIRIC retrieval).

Page 915, Line 3-10: It's potentially confusing at first that you are not using the Raman extinction measurement in the retrieval. Please consider adding another sentence that would explain why only the elastic backscatter signals are used and the motivation for having a Raman lidar for this study (i.e. for the validation).

Page 916: Does the radiometer inversion assume a single complex refractive index for all particles in both the fine and coarse mode in the whole column? Do you have any comment about whether this is a reasonable assumption and what effect it might be expected to have? Please discuss whether there are there any other important assumptions that might affect the accuracy besides the spheroidal model, the use of column-equivalent values of some parameters and the assumption of a single refractive index?

Page 916, Line 7-10: Which of these two AERONET inversions were used in the case studies examined in this paper?

Page 915-916, Section 2.2: Is there is a minimum AOT required (0.4?) to obtain the AERONET inversion for use with LIRIC?

Page 918, line 9-12 and 920, lines 13-20: This couple of paragraphs is a slightly awkward compromise between too little and too much technical information. The phrases "Multi-term LSM formulation" and "LSM-based statistically optimized retrieval procedure" by themselves are not very informative. At a minimum, Least Squares Method (?) needs to be spelled out. I think a more complete description of the retrieval method would be appropriate here (or in an appendix) since the prior Chaikovsky and Wagner references are not in peer reviewed literature. If so, then I think it would be good to have the equations that define the system (the ones described on 920 as "a quadratic functional that consists of several terms"). Less important would be a description of



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the inversion method including the smoothness constraint. If you prefer to have just a sentence or two without much technical detail, then I think it is important to make these sentences more descriptive and understandable by someone who is not already familiar with the Dubovik retrieval. In either case, please include the full list of references right at the start of this discussion (page 918).

Page 918, line 9: Is it covariances between different heights (or range bins) that are required or covariances between different wavelengths? Without further explanation about the covariance matrices, I'm not sure this detail adds much value.

Page 918, line 14-15: Consider adding "also" after profiles and adding another sentence something like this: "Comparing these profiles to the lidar measurements allows us to solve for the particle volume concentrations." With no mention of the lidar, which is better suited to measure aerosol profiles, it's confusing to read that the aerosol profiles are estimated from the photometer.

Page 918-919: Is the LIRIC system of equations overdetermined (after the AERONET inversion step)? It seems that there are more measurements (4 lidar measurements per range bin plus column constraints) than unknowns (3 concentrations per range bin), although of course they are not orthogonal. If so, can you make any statement about whether 3 is the minimum number of lidar wavelengths needed for this retrieval? If the authors have any basis for answering this, I think there would be interest in it.

Page 920, line4: If you say "minimized with respect to the particle volume concentrations, C" rather than just "minimized", it would be clearer.

Page 920, line 26-Page921, line 2: the idea of forming the error bars from the results of multiple runs using different regularization parameters seems a bit suspect. These presumably depend on how much the regularization parameters are tweaked and therefore are not a good representation of the propagation of the input measurement errors. While there might be an argument that the errors from different runs are random within the measurement errors (I'm not sure I actually believe that), 5-10 runs



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does not seem like enough variation for a Monte Carlo type error bar calculation.

Page 923, line 15: Reference the earlier work by Sugimoto et al (2003). Tesche et al. (2009) are responsible for applying the equations from Sugimoto et al. to extinction from a Raman lidar. Since you are partitioning only the backscatter, I think the prior reference is appropriate.

Page 923, line 23: "The parameters $a_f$ and $a_c$ are almost insensitive ...". What is the basis for this statement? Please add an explanation and reference.

Page 924, line 9: "obtained from the Raman lidar measurements or from combined photometer-lidar observations". Which option was used for the cases presented here? If the Raman lidar ratio measurements are used, is it still a column-equivalent value, or in that case are $S_{aer1}$ and $S_{aer2}$ height dependent?

Figure 4 and discussion, Page 926, line 27. I'm confused why the LIRIC derived coarse mode backscatter coefficient is lower than POLIPHON for the peak near 2 km, but the particle depolarization ratio at that height is greater (in Figure 6). I would expect both errors in the same direction.

Page 926, line 29 – Page 927, line 3 and also Page 928, lines 24-28. The statements attributed to Mueller et al (2012) in the current work seem much more specific than how I interpret what Mueller et al actually said. I believe that they see discrepancies between AERONET and in situ measurements which they partially attribute to the spheroidal model, but I do not see where they quantify an upper bound on the difference this can make, or a specific statement that this is due to the phase function at 180 degrees (although they do say less specifically, "the AERONET models were not designed to work at 180 degrees"). The statement in this manuscript implies that 20% is a theoretical upper bound, but Mueller et al. (2012) is an empirical study and doesn't make any theoretical explanation that would allow for a determination of an upper bound.



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Page 926-927. The two paragraphs starting at the end of 926 and ending near the end of 927 list several possible explanations for a systematic bias between the LIRIC and POLIPHON results. You could learn more about the likelihood of some of these possibilities with relatively easy follow-up analysis. First, you note (here and elsewhere) the possibility of a discrepancy between the LIRIC coarse vs. fine partitions and the Raman spherical vs. non-spherical partitions. It seems to me that you have the means to compare like quantities. Given the four concentrations introduced in Eqns 5 and 6, it should be easy to compute the spherical and non-spherical fractions from LIRIC to make a more direct comparison with POLIPHON. Then you could know for sure if this explanation is applicable in these cases. You also propose that the pure dust depolarization ratio might be too low. A sensitivity test could determine how big this value would have to be to produce good agreement. If the answer is about 34% or less, that would support this possible explanation, but if it would have to be much larger, it seems unlikely. Finally, you point out that the column value of backscatter and extinction used in the LIRIC analysis may not be an adequate representation of the height-dependent values. The Raman lidar provides fully resolved lidar measurements of height-dependent backscatter and extinction measurements. Is there any way to use these to check this possible explanation?

Page 928, line 22-23. Discrepancy in the lidar ratio is referred back to the discussion that the 180 degree phase function in the spheroid model would lead to errors in the backscatter but not extinction. To address this, first of all, you should show a comparison of extinction. But I'm not sure I believe this explanation, because in this case the lidar ratio agreement is poor and the backscatter agreement is good (implying perhaps that the extinction agreement is poor).

Page 929, line 2. Lidar ratios of 78-80 sr at 355 nm seem very high. Mueller et al (2012) also saw a similar problematic result for the AERONET results in that study.

Page 929, line 10, "good agreement". The agreement is not terrible, but the following discussion suggests that the depolarization should be almost a reproduction of the



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input profile, and it is not that good. Is there a reason for the high bias in the lower part of the dust layer and low bias in the upper part?

Page 933, lines 1-5: Note that the poor agreement in the previous graphs was below the lowest altitude Raman values in these graphs. There's nothing to indicate that these results aren't also poor at low altitudes, so the "surprisingly" good agreement is not necessarily a contradiction.

Page 933, line 9: Please consider adding "using a combination of backscatter lidar and photometer measurements" or some similar phrase to the end of the sentence.

Page 933, line 14-15: This may be too strongly worded. In the discussion, the mass concentration comparison for the volcanic aerosol was described as acceptable considering the large error bars, which sounds less confident than "good and trustworthy". The mass concentration comparison for the dust case was better. Being more specific about the differences between the cases (as suggested in the General Comments) would be good. Also since no comparison was made with the volume concentration, it is misplaced in this sentence.

Figures: What are the horizontal and vertical resolution of the profiles shown in the figures?

Figure 9: I don't understand why the fine and coarse mode portions from the LIRIC results don't add up to the total backscatter coefficient below about 0.5 km. Here the coarse mode is 0, the fine mode is approximately 1 per Mm-sr and the total is nearly 3 per Mm-sr.

Figure 11: The small angstrom exponent below 1 km seems inconsistent with the result that it is entirely fine mode with no coarse mode at that altitude. Is there an explanation for this?

**Technical Comments**

Page 913, line 1-4: This is not a sentence. Was "analysis" meant to be "analyzes"?



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This would make it a sentence.

Page 917, line 1: “RFOV”: Please spell out.

Page 918, line 8: missing word, “down to the”

Page 925, line 7: suggest adding “the” to make “dominated the particle backscattering”.

Page 929, line 6: suggest “is near zero” rather than “fluctuates around zero”.

**References**

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