General comments

This paper does a good job of clearly describing a retrieval algorithm that is of unquestioned importance, and therefore I feel it should certainly be published in some form.

However, this is a somewhat unusual situation, given that the algorithm has been in use for a decade already.

The paper seems to be something in the nature of a user's guide or ATBD, a clear and coherent description of the algorithm, to be sure, but very little else. While the algorithm description and **the few brief** examples shown in the paper would be sufficient for the publication of an introductory paper about a new algorithm, the situation of this retrieval system, having been used for so many years, is different. I'm not convinced that overdue documentation is a sufficient motivation by itself. The journal guidelines call for "novel" concepts and "substantial conclusions". While I recognize that the authors have legitimate claims that LIRIC was a ground-breaking retrieval concept in 2002, the question of whether it is novel and unique now, in 2016, is a gray area.

As for conclusions, the only conclusion I see is the statement that the algorithm is "robust" because it produces similar results from several different sets of lidar measurements in a single scene. The accuracy and robustness of the algorithm has apparently been better demonstrated already by the long list of previous papers discussed in the beginning of section 7, which according to the manuscript include evaluation of uncertainties, direct validation against measurements, and comparisons with other retrievals, and cover a variety of aerosol situations, all of which are missing or relatively weak in this paper.

The LRS algorithm has been (and still is) under continuous development since 2002. Current version of the algorithm (that is presented in the paper) was mostly completed to around 2012. (Around this time, the name "LIRIC" had appeared for the first time). From this time, active dissemination of the algorithm among EARLNET teams had begun. Thus, the presented materials are mostly novel. As to examples of the LIRIC applications under different aerosol conditions, many of them are presented in other publications of the paper co-authors: we choose to refer to this works and not duplicate the plots (please see the revised section 7). The revised section 7 also discusses the LIRIC validation.

Fortunately, the long and successful history of the LIRIC algorithm presents the opportunity for more in-depth analysis; there is no reason to rely on two isolated events, both similarly dominated by coarse non-spherical particles. The second part of Section 7 provides a bullet list of "lessons learned" from the operational phase of LIRIC. For the most part, these are not actually discussed or supported in the text, but these could form a possible basis for more in-depth analysis that would provide this paper with novel substantial conclusions that would bring it up to the quality level that we should expect for such an important milestone paper.

We have substantially revised Section 7 to meet your comments. Please see the attached revised manuscript.

On a much more minor note, the language and grammar are somewhat irregular, but I think this would be much improved after careful copy-editing, and it is not enough to interfere with the meaning.

We believe that we have improved the language and grammar to some extent when we responded to your comments. (Please see the attached revised manuscript.)

Specific comments

Pages 12761-64 (Introduction): The relationships between LIRIC and these other related algorithms (POLIPHON, GARRLIC) is not clear. More information that would help to distinguish LIRIC from these other algorithms is desired; that is, in what ways are the algorithms and assumptions the same or different? Specifically, the introduction should make it clear if there are circumstances or reasons why users should choose LIRIC over one of the other algorithms.

We have added the following clarification:

"Note that LIRIC technique should not be regarded only as a basis for new algorithms (e. g. POLIPHON or GARRLIC). LIRIC might be superior to them for many aerosol scenarios: it allows one, for example, to distinguish between fine and coarse spherical fractions (unlike POLIPHON) or distinguish between spherical and non-spherical coarse particles (unlike GARRLIC)."

Page 12764, lines 17-19. This line describes GARRLIC (Why is it not named explicitly?). Does GARRLIC supersede LIRIC or are there reasons or circumstances where LIRIC would still be more appropriate? This information should be included in the introduction.

Please see the respond just above.

In accordance with your comment we named the algorithm explicitly:

"The aerosol model and mathematical basis of the LIRIC algorithm became the prerequisite for further development of algorithms for parallel processing simultaneous inversion of combined lidar-radiometer measurements, e.g. GARRLIC (Generalized Aerosol Retrieval from Radiometer and Lidar Combined data) (Lopatin et al., 2013) "

Page 12765, lines 21-24. Are error estimates also provided?

Error estimates are not provided:

Currently LIRIC implements the minimization of the cost function by using standard software packet. This software does not provide explicitly the Jacobian matrix, which is required to compute a posteriori covariance matrix and error estimates. Instead of changing this procedure we focus on error simulations (sensitivity studies) considering it as more general and flexible approach: it enables one to estimate the responses of the retrieval results to the noise and/or uncertainties of any particular measurement system.

Page 12765, line 25. Is this additional information provided by the Raman measurements used by LIRIC? (I think not, but this should be clarified in the text.)

We have added the following clarification:

(Current version of LIRIC algorithm is not designed for using Raman lidar data.)

Page 12767, lines 10-22. Description of the column aerosol parameters retrieved. It would be very helpful to indicate which of these parameters form the independent "state" variables in your retrieval and which of them are derived from the state variables. This long list of variables in which some of them are clearly not independent is confusing.

We have added the following clarification:

[Parameters (1)-(4) are the independent "state" variables whilst parameters (5)-(8) are derived from the state variables.]

Page 12769, line 13. "Level 1.5 or Level 2.0 AERONET data are acceptable as input data in LIRIC." Does this mean as a replacement to Module 2, or as inputs to Module 2?

We have added the following clarification:

(These are inputs to Module 2.)

Page 12769, line 15 (and elsewhere): here, the subscript k indicates the aerosol mode, I guess? It would be good to make sure all the symbols are described close to where they are used, throughout the paper. There are several places where it was difficult to find the definitions.

The subscript k (indeed to indicate the aerosol mode) has been already defined, page 1267, lines 13-14.

Page 12769, line 23 "for the sensitivity test". Since this hasn't been mentioned yet, there's no way to know what this means. Please briefly describe the sensitivity test and how it relates to the rest of the algorithm here or at some earlier point.

We added the clarification here

"The sensitivity test (see Sect. 6) was designed to estimate the responds of the retrieval results to measurement errors and/or uncertainties of input data."

Page 12771, Equation 4. The left-hand side shows ck(h) as one of the dependencies of Lj,1 but this equation does not show the dependence on ck(h), and it isn't explained for another two pages. Is there a way to make the flow clearer and easier on the reader, perhaps by giving some forewarning in the accompanying text about how the dependence on ck(h) comes in? **And/or including ck(h) instead of h as the dependency of the beta and sigma terms in the right-hand side of Eq (4) and following equations?**

We have modified Equation 4 (as well as eqs. 6 and 8) by replacing ck(h) by h in the left-hand side

Page 12772, line 16. Is the parameter μ assumed to be a known quantity, or is it also determined by the retrieval? Either way, how is it determined?

We added the clarification here

Parameter μ is instrument characteristic that is assumed to be known quantity, i.e. it is not updated by retrieval procedure.

Page 12773, Eq 14-17. The column parameters (phase functions, single scattering albedos, etc.) used in these equations should be defined immediately after the equations.

although these parameters were defined before (page 12767, lines 18-20), we added the explanations after Eq. 17 for the sake of reader convenience:

"where $\hat{E}_k$ is aerosol optical thickness for the kth aerosol mode; $\varpi_k(\lambda)$ is the single scattering albedo for the kth aerosol mode, and $P^k_{x,x}(\lambda,180^0)$ are the elements of the backscattering matrix."

Page 12774, Eq 18-19. It would be helpful to describe these equations in words also, since the verbal description is much simpler than the equation… that the column concentration is the height-resolved concentration summed over the column.

+ Page 12774, Eq 18. Define the delta term, "where delta-v is…"

We have modify these lines as

$$\hat{\mathbf{C}}^{*V} = \mathbf{Hc} + \mathbf{\Delta}_V \quad , \qquad (18)$$

where $\mathbf{H}$ is convolution matrix for summing the height-resolved concentration over the column; $\mathbf{\Delta}_V$ is the vector of $\hat{\mathbf{C}}^{*V}$ uncertainties.

Page 12774, Eq 19. Should there be a delta (error) term in this equation also?

Yes, you are right. We have corrected the equation as

$$\mathrm{C}_k^{*V}\left(c_k\left(h_i\right)\right) = \sum_{i=1}^{I} c_k(h_i)\Delta h_i + \Delta_{V,k} \quad . \qquad (19)$$

Page 12777, line 9-11. Is this suggesting that the Lagrange multipliers are determined separately for each retrieved profile? Or are they constant for a given measurement system? Would it make sense to document the Lagrange multipliers in use in this study (maybe in the appendix)?

We added the following explanation of the test

"The set of Lagrange multipliers is provided to LIRIC's users along with software package. However, we do not consider this set as the ultimate one allowing its modification to meet user's specifications".

Page 12778, line 10. '"glues" signals' and '"dead-time" correction'. These bits of jargon are not explained.

We modified the sentence as follows:

"glues" signals (i.e. synthesizes single signal) for the upper and lower troposphere, which were measured with different receiving systems, as well as provides the "dead-time" correction, i.e. the correction for the finite time resolution of the photo-counting system.

Page 12778, line 21. What do "real measurement conditions and technical features of the lidar system", etc., actually mean and how do they affect setting parameters? This sentence is too vague.

For further explanation, we modified the sentence as follows

~~Real measurement conditions, technical features of the lidar system and the accuracy of columnar aerosol parameters retrieved from the radiometer measurements (Dubovik et al., 2000a, b) are taken into account in setting parameters of the module~~.

"The user can upgrade default instrumental noise parameters to meet real measurement conditions and technical features of the lidar system; the accuracy of columnar aerosol parameters retrieved from the radiometer measurements (Dubovik et al., 2000a, b) is also taken into account in setting parameters of the module. "

Page 12779, line 2. "Basically" is a vague word. In colloquial usage it means "I'm not saying exactly what I mean but the difference is unimportant", but that's not appropriate for a journal article. Please consider rewording to more precisely say what you mean here. Does this mean that the retrieval scheme was initially developed for AERONET but there are some differences? If so, what are they?

We have changed this sentence as

"The LRS technique uses the aerosol model that was initially developed in AERONET to describe column-averaged aerosol properties and generalized it to the case of the height-resolved aerosol concentrations."

Page 12779. There is a concern that even if the spheroid model is sufficiently applicable to AERONET retrievals, that it may be significantly less accurate for lidar retrievals since optical properties at 180˚ phase may be particularly poorly captured by the spheroid model. Any comments about this?

The limitations of spheroid model for lidar applications are discussed in Appendix B (page 12791, lines 6-14). Besides, LIRIC involves the use of the same aerosol model as AERONET.

Section 6.1. The EARLI09 intercomparison was a great opportunity of course, but it's disappointing that this paper has only one or two example cases. All the intercomparison shows is that the inversion is relatively stable in one particular measurement scenario and does not demonstrate that it is correct (since there is no independent "truth" measurement discussed). Yet, the appendix hints that there are retrievals available for at least 8 years of data which must cover a much larger range of scenarios. More in-depth analysis with a greater variety of aerosol scenarios is needed.

As it is was mentioned above, many examples of the LIRIC applications under different aerosol conditions have been presented in other publications of the paper co-authors: we choose to refer to this works and not duplicate the plots. (Please see the revised version of Section 7)

Page 12780, line 8, “Figs. 4a and 5a”. By referencing only the “a” panels, do you mean to specifically refer only to the fine mode (and if so, why not discuss the others), or do you mean to say Figs 4a-c and 5a-b?

+

Page 12780, line 8, “close agreement in structure and magnitude”. Please quantify in the text. It’s difficult to read numerical values of the figures.

In response to your comments we have modified the sentence as

“It is evident from Figs. 4a-c and 5a-b that retrieved ck(h) profiles have similar structure over the troposphere except for the lower layer.”

Figures 4, 5, 7, 8, 10, 11, 12. Please expand the axes where appropriate and add minor ticks wherever possible. It is very difficult to read anything quantitative off these graphs. This is particularly important where you are making the point that the relative standard deviation is “small” but from the graph all that can be reliably seen is that it is somewhere between 0 and 100%. Also, I hope the figures will be bigger (relative to the text) in the final published pdf. The authors can try to make sure of this at the galley stage.

We will discuss with the production office the possibility to upgrade the plots.

Page 12780, line 10. “only when values of the aerosol concentration become negligible”. This seems contradictory. The aerosol concentration is not negligible in the layer below 1 km, where there seems to be a significant amount of disagreement, especially in the 4c profiles and in 4a for the “hh” profile. The overlap effect is probably the explanation for this, and it is not unreasonable, but the results should be described accurately in the discussion.

In response to your comments we have modified the sentences as

“The relative deviations increase mainly when values of the aerosol concentration become negligible. The discrepancies are also possible in the near-surface atmospheric layer due to overlap effect (e. g., for the Hamburg lidar system, Fig. 4a)”

Page 12780, line 15. “Specificities of the inverse operator” again sounds like vague jargon. Even though there is apparently more information in the Appendix, it would be better if the language were made more precise here too.

We removed “specificities of the inverse operator” from the sentence:

“Also some differences in the retrieved concentration profiles $c_k(h)$ are due to measurement errors and uncertainties in aerosol modeling ~~as well as specificities on the inverse operator (see Appendix C)~~.”

Page 12780, starting at line 16, through the end of section 6.1. The purpose and implications of this section are not completely clear. Given that you are doing a maximum likelihood retrieval and have input measurement error covariance matrices, isn't the retrieval error part of the standard output? So, is this sensitivity test being done just once for this paper to analyze and illustrate a specific question? It seems not, since this is described as one of the modules of the software. So, please explain why it's better to do the error estimates using this sensitivity study. Do you not trust the measurement error covariance matrices? If not, does that have implications for the maximum likelihood retrieval? Are the results of this sensitivity test consistent with the error output of the maximum likelihood retrieval?

As we explained before in response to the previous comment (to Page 12765, lines 21-24), current LIRIC implementation minimizes the cost function by using standard software packet that does not provide explicitly the Jacobian matrix. Thus we cannot compute a posteriori covariance matrix and error estimates. Instead we choose to focus on error simulations (sensitivity studies) considering it as more general and flexible approach.

Same section: it seems difficult to draw any general conclusion from the sensitivity analysis presented, since it is for only one particular aerosol case dominated by the coarse non-spherical mode. Since this is entirely simulated, there is nothing preventing repeating this analysis for a variety of aerosol situations: low and high loading, dominated by fine mode or the coarse spherical mode, and with multiple layers in the column with the same or different aerosol types. A more in-depth analysis with multiple examples or a statistical treatment would be much more valuable.

This section presents an example of sensitivity test implementation rather than comprehensive review of the sensitivity studies. We tried to present the most important aspects of the implementation and application of the LIRIC algorithm. Trying to not unnecessarily increase the size of the article, we covered some aspects as brief as possible. The revised version of the Sect. 7 includes brief review of other sensitivity studies for LIRIC that were performed by this paper co-authors.

Page 12781, line 13. Is ±20% the standard deviation or the full range?

It is full range. We added the explanation in the text:

"… ±20% (the full range) "

Section 6.2, page 12781, line 21, "Formally we deal with redundant input information and, hence, the number of input data set can be decreased". Alternately, could you also use this "extra" information to retrieve additional information that is currently assumed? For example, height-dependence of the size distribution?

You are right; however, implantation of this option assumes modification of the accepted aerosol model, which is beyond the scope of this paper. To clearly point out, that we do not assume modification of the aerosol model, we have changed the sentence as

"…, hence, for the accepted aerosol model the number of input data set can be decreased"

Page 12782, line 6 ff. Here there is a second example, which is good to see, but again this is dominated by the coarse, non-spherical mode. As I said above, it would be good to have examples with a wider variety of aerosol situations.

Please see the responses to the general comments

Page 12782, Line 15-16. How was the particle depolarization ratio derived from the retrieved concentrations?

The particle depolarization ratio was derived using Stokes parameters that were computed within AERONET spheroid aerosol model

Page 12782, Line 19-21. Since the retrieval also uses the perpendicular and parallel channels, the calculation of particle depolarization ratio from the retrieval is not independent of the calculation from the measurements, correct? Does this really show that the aerosol model is reasonably accurate as stated here, or is this just a reflection of the fact that the retrieval is constrained to reproduce the measurements? That is, the retrieval solution is one that by necessity reproduces the measurements, and may still do so even if the aerosol model is not a good representation of reality. A more convincing argument would be welcome here.

The lidar measurement of depolarization ratio included additional calibration measurement that was not used by retrieval procedure. The fact that direct computation of the depolarization ratio (using retrieved aerosol parameters) reproduced this independent calibration measurement support the adequacy of the of aerosol modeling.

We added to the text additional explanations:

"It should be noted that the lidar measurements included additional calibration measurement that was not used by the retrieval procedure."

Page 12782, line 26. "This implies…" While this is probably true, could it also mean that the fine mode concentration is somehow not constrained by the measurement well at all and instead the information about the fine mode is coming from a priori information and the first guess?

We have weakened the statement: "This could imply…"

Page 12783, line 1-2 and Figure 11. "Lidar depolarization measurement is the key factor in the retrieval of the coarse spheroid particle mode." By similar logic, it appears that 1064 measurements are not important for the retrieval of the coarse non-spherical mode. This is fairly non-intuitive. Any comments?

These results are the case specific. Possible explanations is that that 1064 measurements are important for the discrimination fine and coarse modes; in the case under discussion, coarse mode dominates and the question is to discriminate coarse-spherical and coarse-non-spherical modes that makes depolarization measurements of primary importance.

Page 12783, line 18. "Generally, for measurement conditions that characterize the experiment under discussion…" The measurement conditions that characterize the experiment under discussion are fairly

specific. With only one example, this statement is hardly generalizable, so it's difficult to see how it can be very useful.

We agree that the experiment is rather specific; however we consider it as useful example of LIRIC implementation.

Page 12783-12784, first portion of Section 7. This information about prior published analyses of LIRIC is very important, but should be discussed in the introduction. The discussion and conclusions section should be reserved for discussion of results that are supported by the analysis in this paper.

+

Page 12784 (starting on line 23)-12785, second portion of Section 7. Starting here, these points are new and not previously published, so it makes sense to address them in a "discussion" section. However, they are not supported by any analysis in the paper and come out of nowhere as "offline" conclusions. It would be much better to expand the paper to demonstrate and support these conclusions. It may be acceptable to mention one or two of these points as an aside or supplemental information in a more substantive discussion section, but since these appear to be the only conclusions in the paper, this seems insufficient.

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Page 12785, point ii. The first sentence "LIRIC provides rather stable solutions…" is at least partially supported by the paper, but is not very solidly supported given just two example cases with similar dominance of non-spherical coarse mode and no correlative data to confirm the "basic aerosol features". The second and third sentences of this point are completely unsupported. How is a user to choose "suitable parameter settings"? What does this vague phrase mean? What parameters? (Lagrange multipliers? What else?) What were the values for the settings for the cases that were examined and how were they arrived at? And how do the statements about suitable parameter settings and the stability of the parameter settings relate to the statement about LIRIC providing reasonable retrievals?

+

Page 12786, point i and iii. The information about the limitations of the assumptions and the information about the overlap region are vital and very important for readers to know. These would be a valid and valuable focus of part of this paper; however, these have not been discussed before the conclusion section.

We have substantially revised this Section to meet your comments. Please see the attached revised manuscript.

Besides, the overlap region effect was discussed in the paper: P. 12766 from the line 10; P. 12770 from the line 21

Figure 1. This flowchart is quite good and it makes the flow of information in the algorithm very clear. I also appreciate that the boxes are numbered to make it easier to see the best way to move through the

chart. Just a few points: should box “6” have a lower number, since the settings and constraints are a necessary input to the inversion (#5). Also, you might want to add the label “model settings” to the red upward-pointing arrow.

We have upgraded the figure to meet your comments. Please see the attached revised manuscript.

Grammatical comments

Abstract, lines 7-11. This very long sentence is hard to follow. Please consider breaking it into two or more sentences.

Accepted:

~~The LIRIC data processing provides sequential inversion of the combined lidar and radiometric data by the estimations of column-integrated aerosol parameters from radiometric measurements followed by the retrieval of height-dependent concentrations of fine and coarse aerosols from lidar signals using integrated column characteristics of aerosol layer as a priori constraints.~~

The LIRIC data processing provides sequential inversion of the combined lidar and radiometric data. The algorithm starts with the estimations of column-integrated aerosol parameters from radiometric measurements followed by the retrieval of height dependent concentrations of fine and coarse aerosols from lidar signals using integrated column characteristics of aerosol layer as a priori constraints.

Page 12761, line 22: forming = forcing?

Accepted

Page 12762, line 13: delete “basically”

Accepted

Page 12762, line 26: “pointed above” = zenith-viewing

We meant “aforementioned”, not viewing direction. We replaced: “pointed above” by “aforementioned”

Page 12763, line 18: “in tune to” = “according to”

Accepted

Page 12763, line 19: were = are

Accepted

Page 12764, line 18 and throughout the manuscript: I suggest “simultaneous processing” instead of “parallel processing”, since the phrase “parallel processing” has a particular meaning relating to dividing computer instructions across multiple processors. It also seems to imply that the two retrievals (the column retrieval and the height-dependent retrieval) are separable, which is exactly the opposite of what you really want to say. Simultaneous processing is therefore easier to immediately understand in the context that it’s used in here.

Accepted: both “parallel processing” and “parallel inversion” were replaced by “simultaneous inversion”

Page 12764, line 20: “bond experiments” = “closure experiments” ?

Accepted

Page 12766, line 19: “cooperative”. This isn’t the right word. Does “second, complementary receiving system” convey your meaning?

Accepted and replaced “by a second, complementary receiving system”

Page 12768, lines 9, 11: “i-type” and “ii-type” are confusing. I suggest deleting “i-type” in line 9 or replace with “column parameters”. In line 11, replace “ii-type” with “height distribution parameters” or some other descriptive phrase.

Accepted:

1. *First one deals with sequential inversion* of lidar and radiometer data. It is carried out by preliminary calculation of the ~~i-type~~ column parameters defined in sec. 2.1 from radiometric measurements by using the AERONET inversion algorithm (Dubovik and King, 2000), followed by subsequent inversion of the ~~ii-type~~ height distribution parameters by using lidar data with columnar characteristics of aerosol layer passed as *a priori* data (Chaikovsky et al., 2012);
2. *Second option suggests* ~~parallel~~ simultaneous *inversion approach* for retrieving optimal ~~i and ii type~~ parameters of the aerosol model by using a joint inversion procedure from combined lidar and radiometer data.

Page 12769, line 12: delete “i-type”

Accepted

Page 12771, line 10: I don’t understand what the notation 1,u,U means.

We modified the sentence as follows.

$p_j \in 1,2,...,U$ that indicates the type of measurement associated to the j -channel of the lidar and U is a number of the types.

Page 12774, Eq 18 and line 14, sometimes the delta term is delta-v, and sometimes delta-C. Please fix to be consistent.

Accepted: We change line 14

Page 12778, line 16. Typo, missing “e” in “ConcentRetriever”

Accepted

Page 12782, line 12 and throughout the section, also figure captions. When you say “deviations” I think you mean “standard deviations”.

We added the clarification:

“(by “deviations” hereinafter we mean “standard deviation”)”

Page 12782, line 17-19. Here it is D(2) and D(3) but in the figure it is D(1) and D(2). Please fix for consistency.

Accepted and corrected

Page 12794, line 9. “More corrected” = “A more correct”

Accepted

Page 12794, line 10. Replace “deficiency of linear relation” with “that there should not be a linear relationship”, if that is indeed your meaning.

Accepted

Figure 4 caption. Spell out Particle Volume Concentrations.

Accepted

Figure 5 caption. Specify the descriptions of a and b panels.

Accepted: the capture was supplemented by

(a, c) – fine, **(b, d)** – coarse spherical aerosol mode;

Figure 6 caption, last line. A typo. Should be a dash not a division symbol.

There is not typo: we used division symbol as in the figure insets.

Figure 7. The blue dash-dot line should probably be solid for consistency with other panels and other figures.

We have upgraded the figure to meet your comments. Please see the attached revised manuscript.

Figure 10 caption. course=coarse (in at least 2 spots)

Accepted : “course” and “coarsw” were corrected

Figure C1 caption. Spell out “condition number” in the caption.

Accepted:

Cumulative Distribution Functions (CDF) of parameter Cond$\mathbf{U}k,k_$ (condition number)…