Dear Editor,

We are very grateful to the two reviewers for their appropriate and constructive suggestions and for their proposed corrections to improve the paper. We have addressed all the issues raised and have modified the paper accordingly. Below is a summary of the changes we performed and our responses to the reviewers' comments and recommendations.

Summary of the changes

Responses to reviewer 1

…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 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.

The reviewer is right. The idea is not new, it can be found, for example, in the book of Twomey. In the revised manuscript we have added corresponding comment and additional references for earlier publications of Thomason and Osborn, 1992; Chaikovskii and Shcherbakov, 1984.

In the revised manuscript we call our technique "Linear estimation" and refrain from calling it "new technique".

In the end of section 1 the paragraph is modified as:

"…One way to assess this possibility is to attempt to approximate the particle bulk properties by a linear combination of the input optical data (extinction and backscattering). The corresponding weight coefficients can be determined by expanding the PSD in terms of the measurement kernels (Twomey, 1977). Thomason and Osborn, 1992 used this approach to estimate aerosol mass with the multiwavelength SAGE II extinction kernels. The interpretation of lidar measurements using the linear estimate techniques was explored in early studies by Chaikovskii and Shcherbakov, 1985. The potential of this approach for treating elastic-Raman multiwavelength lidar measurements was studied by Donovan and Carswell, 1997 under assumption of known refractive index. The technique was further explored in recent publications (De Graaf et al., 2009, 2010) where different aerosol models were used to invert optical data without prior information about the particle refractive index.

In this paper we propose a modified technique, which here and below we refer to as "linear estimation" (LE). In difference with the mentioned above approaches the complex refractive index is derived as a part of retrieval procedure together with the bulk aerosol characteristics. In addition, in order to improve stability of solution we provide not a single solution but a family of solutions closely reproducing the measurements. To validate LE, we apply it and the full inversion algorithm (Veselovskii et al., 2009) to the same data and compare the results. Finally, we apply LE to an extended series of lidar measurements to evaluate height-temporal variations of the particle bulk parameters."

And in the end of section 2 we added the paragraph:

"Thus the main difference of described in this section algorithm from the approach presented previously by Donovan and Carswell, 1997; De Graaf et al. 2009, 2010, is that we consider not a single solution but a family of linear solutions corresponding different inversion intervals $r_{\min}$, $r_{\max}$ and different complex refractive indices. The average of solutions in the vicinity of the minimum of discrepancy (26) is considered as most probable estimate of particle parameters."

Page 7503, Equation 5: My understanding of the earlier references is that the principle components analysis is used to invert the KK^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?

Yes, we tried this technique, but didn't find significant improvement. All presented results were obtained without lesser components removal. However, we don't exclude that removing of lesser components has potential to improve the inversion further. We don't discuss this subject in the current paper, because the research is not finished yet (the results depend on size distribution, input errors and the number of optical data used).

*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.*

We have added the phrase, explaining that Mie theory is used for the kernels calculation:
"To get kernels $K_l(m,r)$ in our study the Mie computations are used, thus the particles are assumed to be spherical. This approach can also be generalized to treat the particles of irregular shape, by using the kernels corresponding to the ensemble of randomly oriented spheroids (Mishchenko et al., 2000, Dubovik et al., 2006)"

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 in the paper

The retrieval for each vertical bin was done independently. Corresponding comment is added to the manuscript on page 7510.

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.

The only smoothing we use is the smoothing of input data. We did not smooth the kernels, as Donovan and Carswell did. However when we consider discretized kernels, we perform the

integration inside the interval of discretization. Normally we used 100 log-scaled intervals for every inversion window. Corresponding comment is added to the text.
" In our computations we normally use N_v=100 radii logarithmically distributed inside the inversion interval."

*Page 7506, line 5: "**F** can be calculated using the detailed size distribution with very large Nv" 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?*

Increase of N_v above 100 didn't improve the retrieval. LE approach allows to calculate $\boldsymbol{F}$ very accurately, but still there is uncertainty related to the existence of null-space and to the unknown value of refractive index. So finally the uncertainties of parameters retrieved with LE and with regularization technique (using 5 base functions) are close.

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.

Yes, the size distribution can be calculated and in many cases it brings to the reasonable results, especially when only fine mode is considered. Situation is less favorable for the coarse mode and especially for the bimodal distribution. We didn't include this material in the manuscript because it demands a separate consideration. We plan to consider this and many other issues in a separate paper.

Page 7509, line 5-8: "... we can attempt to estimate $m(\lambda)$ from available observations. Specifically, we can choose one optical data 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 P kernels can be chosen to give extinction and backscatter back. That is, if all N optical data were to be used, then P = 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 p, the particle properties, in terms of g, the measurements.

Yes, the idea is to recalculate backscatter or extinction coefficients back using the rest N_0-1 input data. If we have chosen wrong refractive index or inversion window [$r_{\min}$, $r_{\max}$] the discrepancy (26) is high. So minimization of (26) allows to estimate the refractive index. It should be mentioned that we can't use all N_0 data, because this case the discrepancy is always zero. In the revised manuscript this paragraph is modified as following:

" The input optical data (backscatters and extinctions) are themselves the particle properties and each g_j^* can be recalculated back from the rest of N_0-1data using Eq.(9), as suggested in (De Graaf et al., 2009, 2010). By doing so for each optical data, we get N_0 estimates of $\tilde{g}_j$ that we compare with the observations g_j^*."

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?

In numerical simulation in the section 3 we considered total errors, which are the combination of random and systematical errors (due to null space and uncertainty in the chosen refractive index). From the expressions (21) we can estimate only random errors, and for systematic errors we need to introduce the size distribution, which leads us to the numerical simulation anyway. But the reviewer is right, the comparison of numerical simulations in section 3 with theoretical results in section 2 is an interesting test and we will try it in the future.

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?

The uncertainties represent the total errors summarized in the Table 1. From AERONET retrievals (and high value of the Angstrom parameter) we conclude that aerosol is represented mainly by the fine mode, and the uncertainty of the optical data measurements we estimate to be below 10%. Thus we choose 25% uncertainty for the effective radius and ±0.05 for the refractive index. Corresponding comment is added to the text.

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.

The same averaging was performed for LE and for the regularization retrieval. But in LE approach we obtain the vertical profile of the effective radius from the linear combination of the profiles of backscattering and extinction, which are rather smooth. The refractive index doesn't oscillate much with height, so the profile of effective radius is quite smooth also. We didn't compare the individual solutions, but from our experience the parameters obtained from individual solutions present significant variation (Veselovskii et al, 2010), so we prefer to work with averaged solutions. But the reviewer is right, such comparison may be useful, and we will do it in the future.

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.

In this paper we just touched a little the possibility to reduce the number of input optical data still keeping the reasonable accuracy of retrieval. The possible configurations of input data sets depend on size distribution and it is a subject of a separate research which is in progress at a moment. We give some information on pages 7512-7513 and mention that detailed study of this question is in our plans but is out of the scope of current paper.

Technical Comments:

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

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".
Corrected

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

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

Equation (9): typesetting error. I believe F_g should be Fg. That is, g should not be a subscript but a separate variable in the product F times g.
It is a typographical error, will be corrected.

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.
Corrected

The variable g is not italicized in Eq. (18) whereas it is italicized elsewhere.
It is a typographical error, will be corrected.

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.
Corrected

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).
Corrected, *p* is changed for *l*

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

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

Page 7510, line 9: "minimization of _ in Eq. (20): : :" I think you mean Eq. (26).
Corrected

Page 7509, line 12: The in-line equation for _gj has a typographical error. The minus sign should not be superscripted.
Yes, it is typographical error

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.

Corrected

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

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

…

Responses to reviewer 2

...A disadvantage of the method is its application only in spherical scatterers (Mie theory).

Yes, in this paper we consider the spherical particles only. However, the approach can be generalized by using the mixture of the spheres and the randomly oriented spheroids, thus allowing the treatment of nonspherical particles [Veselovskii et al, 2010]. This work is in progress.

General comments: The authors should elaborate much more clearly, in which part their algorithm approach differs from the work cited and if really this work is a "new" one. If it is not the case, they could probably use the term "modified approach" or "optimized approach".

We have already touched this issue in the response to the Referee 1. In the revised manuscript we refrain from using the term "new approach".

Page 7509: lines 20-22. The authors assume that the errors are the same for all channels

In principle this approach allows to consider different input errors for different channels and we plan to realize it in the future. However the number of the measurements is small (just five in our case) so optimization of inversion in respect to the measurement errors probably will not bring much improvement. However, when we perform the numerical modeling we introduce the errors independently in every channel, so all possible scenarios are considered.

and refractive index is spectrally independent inside the spectral range considered (355-1064 nm). It is well know that this condition does not apply in real particles, which do present a spectral dependence. Therefore the authors should elaborate more this section, or rephrase it, by saying that "our technique is limited to spectrally independent refractive index values".

Yes, the particle refractive index (RI) can be spectrally dependent introducing the additional uncertainties in the retrieved particle parameters. Still for many typical aerosols the spectral dependence is not very strong. Thus basing on AERONET results (Dubovik et al, 2002) for the biomass burning particles the real part of the refractive index varies from 1.53 to 1.58 in 440 nm – 1020 nm spectral range. For the urban-industrial aerosol the variation of m_R in the same spectral range is from 1.38 to 1.41. Besides, the lidar measurements are not very sensitive to the RI, especially for small particles (this is the reason, why the uncertainty of the refractive index estimation from the lidar data is so high). Numerical simulations performed demonstrate the uncertainties of the bulk parameters retrievals obtained for the spectrally dependent RI are close to the values specified in the Table 1. The most serious effect is provided by the spectral dependence of the imaginary part of the dust particles, this issue (and the way to correct it) was discussed in our recent paper (Veselovskii et al 2010). Thus for many typical aerosols the assumption of the spectrally independent refractive index leads to the reasonable results.

Still there are situations where spectrally dependent RI should be taken into account and further studies in this field are necessary. We didn't include corresponding discussions in this paper, because the task is not accomplished yet and it goes beyond the scope of this paper.

Page 7515: line 21. How the particle depolarization ratio is calibrated in the Turkish system? The authors should be more precise in this issue, since many erroneous values often occur.

In revised manuscript the paragraph is added: "The particle depolarization ratio was calculated from the ratio of co- and cross polarized components of the particle backscattering coefficients.

For the calibration of depolarization measurements the molecular depolarization ratio in an aerosol-free region was used."

Page 7516: What is the uncertainty in the retrieved values of aerosol extinction and backscatter? Error bars should be added. In addition, what is the Signal to Noise Ratio for the lidar signal at these heights? The authors should comment on that.

The random errors in our analysis are estimated by applying the Poisson statistics to the lidar profiles. Some aspects of systematic errors estimation were touched in our earlier paper [Veselovskii et al. 2009]. We estimate the total errors of input optical data at the height of interest to be below 10%. We have also added the following sentence to the manuscript: " We estimate the uncertainty of the particle extinction and backscattering α_{355}, β_{355}, β_{532}, β_{1064} calculation at altitudes below 2.5 km to be less than 10%. ".

Technical comments:

Page 7500: line 15, add "respectively" before "the uncertainties".
We would prefer to stay this sentence unchanged.

Page 7504: line 15, replace "can't" by "cannot".
Corrected

Page 7509: line 19, replace "I" (bold),by "I" (not bold).
Corrected

Page 7510: line 1, replace "0.075 um, 10 um", by "0.075 um - 10 um"
Page 7510: line 2, replace "1.35 um, 1.65 um", by "1.35 um - 1.65 um" and "0.00 um, 0.03 um", by "0.00 um - 0.03 um". Also omit "previously".
Corrected

Page 7511: line 30, replace "didn't" by "did not".
Corrected

Page 7512: line 09, replace "r0=0.2", by "r0=0.2 um".
Corrected

Page
7513: line 20, add "%" after "25".
Corrected

Page 7514: line 11, omit "The top of the boundary layer...2250 m". It does not give any valuable information here.
Corrected
in lines 21-22, please rephrase "The real part...is less than" to a more clear phrase.
Replaced with " The real part of the refractive index slightly rises with height from 1.37±0.05 to 1.43±0.05, the imaginary part m_I is below 0.005 for all heights."

Page 7515: line 24, replace "below altitudes" by "for altitudes below". Page 7515: line 25, replace "didn't" by "did not".
Corrected

Page 7516: line 1, add "and the optical properties of ash particles" after "for this day".

Corrected

Page 7516: line 7, add umlaut in "o" (of Angstrom", and A with o at the top o A). Page 7516: line 9, replace "are" with "were".
Corrected

Page 7516: line 21 and page 7517 line 25, replace "doesn't" by "does not".
Corrected

Page 7526: Fig. 5, please add error bars in profiles.
Corrected

Page 7527 and 7528, please add at the end of figure caption: "Contours show areas of enhanced aerosols volume density".
Corrected

Responses to Rich Ferrare comment

1. (abstract) The paper states that the retrieval technique is "validated" using the results from the full regularization scheme. A more appropriate word would be "evaluated"; a much better means of validation would be achieved through comparisons with other techniques using independent measurements such as from airborne in situ instruments.

Corrected

2. (page 7509, line 20) The retrieval method assumes that the measurement errors for all channels are the same, and that the refractive index is spectrally independent in this wavelength range. These assumptions should be mentioned also in the abstract and conclusions. Regarding the first assumption, is it true that both random or systematic errors are assumed to be the same for all channels? In terms of calibration errors, this is rarely true. In particular, the 1064 nm aerosol backscatter channel likely has a larger relative calibration error than the other wavelengths. I suggest the authors comment on how strong this assumption is and how different measurement errors in the various channels impact the retrievals.

In principle this approach allows to consider different input errors for different channels and we plan to realize it in the future. However the number of the measurements is small (just five in our case) so optimization of inversion in respect to the measurement errors probably will not bring much improvement. However, when we perform the numerical modeling we introduce the errors independently in every channel, so all possible scenarios are considered. We have worked carefully on the influence of random errors on the retrievals and are involved in a detailed analysis of the influence of systematic errors as well. In this paper, thus far, it is mainly the influence of random errors that have been studied.
The influence of spectral dependence of the refractive index on the uncertainty of retrieval was discussed in our response to Reviewer 2.

3. (Section 3) In the estimations of retrieval uncertainties, it appears that the computation of retrieval uncertainties (if not the retrieval itself) assumes that the aerosol size distribution is monomodal. Is this true? What happens if this is not true (which is often the case)? For example, what are the uncertainties when a PSD is used that combines both ro=0.2 and ro=2.0 particles in Table 1? This also has a bearing on the results discussed later in the paper in that the examples show cases where the aerosols appear to be dominated by fine mode particles. It would be nice to know what happens when both fine and coarse mode particles are present in significant numbers.

Yes, in the Table 1 we presented results for the monomodal PSDs. We tried the bimodal PSDs also and the uncertainties obtained were close to the results for monomodal distributions. However for bimodal PSD we should consider possibility of different refractive indices for the fine and the coarse mode and the result also depends on the relative contribution of the fine and coarse mode particles to the total volume. So this subject takes a careful consideration and we plan it as a separate publication in the near future.

We have chosen for illustration the examples when the fine mode dominates because in this case the retrieval is the most reliable. At a moment we are working on scenarios where strong coarse mode occurs.

4. (p 7512, line 24)this should read "The real part of particle refractive index can be retrieved : : :"

Corrected

5. Fig. 3 shows only the impact on volume distribution. Is the impact similar on the other parameters?

Yes, the impact on other parameters is similar

6. (p 7514, line 9) The previous paper (Veselovskii et al., 2009) that compares AERONET and multiwavelength lidar retrievals used data from Aug. 16, 2010 and not August 15 as done in this paper. I am curious as to why the authors did not choose to use the same day (August 16) for the analyses and example shown in the current paper. The results from the current analysis could then be more directly compared to AERONET results on August 16 as shown in Table 1 of the previous paper.

The results for 16 August were presented in our 2009 paper, so we have chosen another day. But actually we tried the comparison for others days also and the results agreed well.

7. Fig. 5 shows the real part of the refractive index increasing with height. This would imply that the relative humidity is decreasing with height. The previous paper showed water vapor profiles and commented on how the refractive index varied with water vapor. It would be nice if the authors could also show relative humidity profiles discuss these in this paper, especially if a different day was chosen (see item 6 above).

15 and 16 August were characterized by low relative humidity (below 65%) so corresponding modification of the refractive index was below the uncertainties of the retrieval. The water vapor mixing ratio on 15 August was almost constant in 500-1250 m range and then it started to decrease, meaning that mixing in PBL was incomplete. Slight rise of m_R with height is probably due to incomplete mixing, though we should keep in mind that the enhancement in m_R is close to the uncertainty of retrieval ±0.05.

8. What are the uncertainties in the lidar measurements shown in Figures 4 and 5 and how do they compare to the uncertainties used in Table 1 and Figures 2 and 3?

The uncertainties in the optical data we estimate to be below 10% at the heights of interest. So for the uncertainties of retrieval we took values from Table 1, corresponding the fine mode and 10% input errors.

9. (p 7516, line 23) The abstract and conclusion should also state that the retrievals as discussed here apply to spherical aerosols and that further work (similar to what was done in the Veselovskii et al., 2010 paper) is required to consider the case of nonspherical aerosols.

Corresponding comments are added

10. (p 7516, line 25) This paper and a previous paper (Veselovskii et al., 2010) indicate that significant uncertainties arise when attempting to retrieve nonspherical particle properties using retrieval techniques that assume spherical particles. This paper indicates at this point that the real part of the refractive index is significantly underestimated in such cases. Given this, and that Figure 6 shows that nonspherical aerosols are dominant above 2 km, I suggest that it is inappropriate to show results of the retrieval (especially for effective *radius) above 2 km as shown in Figure 7 when it is known that the results are most likely incorrect. I recommend Figure 7 should be changed to show results only below 2 km.*

Actually, as it was shown in our 2010 paper, the application of spherical kernels to irregular particles allows to obtain reasonable estimation of the particle volume and effective radius (though the use of spheroids is preferable), this is why we show the results up to ~3 km. The real part of the refractive index is underestimated in the case of nonspherical particles, but we show it up to 3 km to keep the scale the same for all figures.

11. (p 7516 and Figure 7) This lidar also measured water vapor. Can the authors also show water vapor or relative humidity profiles to see if variations in these parameters are correlated to the variation in refractive index (below 2 km) shown in Figure 7?

The lidar measured the ratio of the vapor and nitrogen Raman signals but the calibration was not performed by that time, so we had no absolute values of the water vapor content or RH. Thus we could not use the water vapor measurements for analysis of particles hygroscopic growth.

12. (p 7518, line 9) The conclusion indicates that removing the 355 nm extinction enhances uncertainties. This was not discussed anywhere in the paper. It would be nice if the paper provided more discussion about the impacts on the retrievals of adding or subtracting wavelengths and/or channels.

In this paper we just touched a little the possibility to reduce the number of input optical data still keeping the reasonable accuracy of retrieval. The possible configurations of input data sets depend on size distribution and it is a subject of a separate research which is in progress at a moment. We give some information on pages 7512-7513 and mention that detailed study of this question is in our plans but is out of the scope of current paper.

13. (p 7518, line 12) The paper states that "To demonstrate the efficiency of the method long-term series of aerosol physical properties derived from lidar observations performed in Turkey in May 2010 were processed." The sentence is confusing but seems to say that a long term series of measurements were used to demonstrate the efficiency of the method. Since the paper shows data from only one night, the authors should discuss why this single case does provide such a demonstration. Some readers may feel this is too short of a period for such a demonstration.

One night is insufficient for a full evaluation of the method. However, to our knowledge this is the first time that time series retrievals with the temporal and spatial resolution as shown in this paper have been demonstrated. Also, the absence of oscillations of the effective radius and distinguishable patterns in the volume and mR distribution demonstrate the stability of the retrieval. So while clearly more work can be done and we are in the process of doing so based on a larger body of measurements, the main point of being able to efficiently and stably retrieve time series of aerosol physical paramters is, we believe, persuasively made by the colors images presented.

14. (p 7518, line 18) The conclusion mentions that the full inversion (regularization) technique that uses 3backscatter+2extinction data is required for retrievals of the particle size distribution (PSD) which the current technique cannot obtain. Is it also true that the full inversion (regularization) technique that uses 3+2 data is also required to get more (and sufficiently) accurate retrievals of the imaginary refractive index and consequently single scattering albedo? It would be very helpful if the authors could *discuss the applicability (or lack thereof) of this new technique to retrieve the imaginary refractive index and single scattering albedo.*

Retrieval of the single scattering albedo is a critical question and we expect that LE approach should be able to do it. At a moment we are working on it and perform comparison with the regularization approach. We will present the corresponding results as soon as the research is finished.