Reply to Referee #2

First of all authors would like to thank the reviewer for his/her constructive suggestions and general efforts in improving this paper. All suggested revisions were addressed and responded. Please, find the details in the document below.

General: This paper can be regarded as a milestone in the development of combined techniques based on active and passive remote sensing. The paper is very appropriate for AMT.

However the present version of the paper is not easy to read, the contents not easy to understand.

Minor revisions (but many points) are needed which will further improve the paper.

All the revisions were taken into account. The paper was throughoutly revised and a number of aspects were clarified.

Details:

Abstract: is too long, too general, the first 10 lines of the abstract should be removed, they are appropriate for the introduction, but not for the abstract. Abstract should contain briefly: goal of the paper, techniques and methods used, observational campaigns (if any), main findings (numbers).

The abstract was shortened and revised as suggested by the reviewer.

Abstract, line 13: what do you explicitly mean with aerosol loading (calibration?, at reference height)?

The term "aerosol loading" was replaced with more appropriate "aerosol amount"

Abstract, page 2255, line 6: do you mean . . .. just one or several AERONET sites?

Only one site was meant. The name of the used site was directly indicated "over Minsk (Belarus) AERONET site"

Page 2255, line 13: . . .. Being common atmosphere pollutant aerosols also have. . . I do not understand . . .

Phrase was reformulated to "Also, aerosol pollution affects populations health and ……"

Page 2255, line 22: strange references. . . from 1996 and 2000, are there no better, actual ones, from 2005 or later?

These are cornerstone publications that describe the principles of AERONET and SKYNET. The later papers generally devoted to more specific aspects and modifications and they are cited later in our paper where those specific aspects are discussed.

Page 2256, line 1: South-Eastern SKYNET. . .. Is that in Asia? Please be more precise!

Term "South-Eastern" was changed to "East-Asian".

Page 2256, line 18: There are several aerosol closure field experiments, all better than the mentioned INDOEX (Ramanathan 2001), e.g., LACE 98 (special issue, JGR), TARFOX (special issue JGR), ACE 1, ACE 2 (Tellus special issue), ACE Asia (JGR), and SAMUM and AMMA related campaigns and aerosol closure studies

References to mentioned field experiments were added to the text.

Page 2257, line 20: . . .. from entire data base. . .. What does that mean, please specify!

"Entire database" was changed to "using entire database of AERONET retrievals obtained for ~10 years of observations".

Page 2257, line 23-30: Concerning lidar ratio: start with Raman lidar (reference, Mueller et al., 2007, lidar ratio paper„ then HSRL (reference Burton et al., ACP/AMT 2012, Gross et al., ACP/AMT 2012/13) and then lidar column backscatter /AERONET AOT combination (authors may know best appropriate reference here). . .. Leave out scanning techniques or slope method, nobody is using that!

The text was modified as suggested.

Page 2258, line 1: Please check HSRL observations and aerosol typing (Burton et al., 2012, Gross et al., 2012 or 2013?, ACP? or AMT?). Should be added.

References were added.

Page 2258, line 6: ..rather complex?. . . appears to be no longer true, see Althausen et al. (JAOTech, Polly) or Baars (JGR, 2012, Amazonian Raman lidar observations).

Mentioned references were added. Paragraph was modifed:

"Despite of the achieved progress in non-elastic lidar technology (Baars et al., 2009; Althausen et al., 2009) the bulk of monitoring of vertical aerosol variability is conducted by conventional lidars"

Page 2258, line 29: . . .aerosol vertical mixing. . .? Please specify what you mean here!

Term "aerosol vertical mixing" was changed to "vertical variability of aerosol properties".

All in all: Introduction is very long, could be shorter, more focusing on the goal itself. . ., readers always like to see short and fresh introductions, to present a general intro is always abit boring.

The detailed overview of precedent efforts was included in the introduction in order demonstrate the evolution of the retrieval strategy from basic inversion of only lidar data to synergetic retrievals using a combination of lidar and AERONET retrieval. In our opinion this detailed description is very important and removing it could mislead the reader about origins of the presented approach. Also, we think that less than 2 pages introduction out of 24 pages is acceptable for the paper describing a new methodology.

Page 2260, line 10-18: again, very general, and only insiders understand what is stated. .

This paragraph was added since the authors always receive the questions about relevance of LiRIC. GARRLiC and AERONET algorithms and the approaches used.

Therefore, we considered that providing this inside information is highly useful and important for those who may use and compare algorithms in the future.

Page 2263, line 17: Is the usage of climatological data (temperature and pressure profiles to compute Rayleigh scattering) sufficient? I would expect that you need actual weather prediction model data (forecast data) or actual radiosonde obs. of temperature and pressure profiles. For an accurate consideration of 355 nm Rayleigh scattering and backscattering. .

Unfortunately measurement station at Minsk does not have an additional sounding capability to determine molecular scatter profile more accurately. The model used, however, is utilized within the European Lidar Network on a regular basis. All known uncertainties of the model are well studied and accounted in the covariance matrix of the corresponding measurements. In addition we would like to note that vertical profile of Rayleigh scatter in GARRLiC algorithm is loaded from external file and could be flexibly adjusted if more accurate data are available.

Page 2264, line 6: You describe the previous version of the forward model, but what are the differences to the operational version, which is applied for GARRLIC?

As described in the text, the GARRLiC is based on previous AERONET/PARASOL retrieval developments. In these regards it uses similar forward model that is modified for accounting of lidar data. The main differences are listed in the **Section 3. Modifications employed in the "forward model".**

Could you please provide more details. In this way the differences between LIRIC and GARRLIC become more clear. Does GARRLIC make direct use of sky radiances, i.e., basic sun photometer (raw) data?

There seems to be no difference between LIRIC and GARRLIC regarding lidar data input (just elastic backscatter signals at three wavelengths)?

General remark; Why not presenting a table with all input (which lidar signals, which atmospheric and aerosol assumptions, which are height dependent, which are height- independent, aerosol-mode-dependent, aerosol-mode-independent, and finally with all the products retrieved), one table column for LIRIC and one column for GARRLIC. In this way a very clear contrast between LIRIC and GARRLIC becomes visible. Many people will work with LIRIC (or GARRLIC) later on, and will appreciate such an overview table.

The main difference between these two approaches from the point of usage of passive observations is that GARRLiC uses "raw" photometric data, while LiRIC relies on the AERONET inversion products, i.e. LiRIC does not modify the retrieved columnar aerosol properties, while GARRLIC does. **Figure 1** containing chart that compares LiRIC and GARRLiC algorithm inputs and outputs was added. **Table 1** provides detailed description of the characteristics retrieved by GARRLiC.

Page 2265, line 11: Cattrall et al. is mentioned. What did Schuster et al. (2012) commented in this direction. He stated some contradicting remarks concerning the Cattrall approach. Any comment here?

Clarifications were added in the text.

Page 2265, line 12: Is that also in agreement with Mueller et al. (2010a,b, 2012, JGR), SAMUM observations? I do not know any publication where it was demonstrated how well this spheroidal approach works in the case of lidar (180 deg scattering angle). So at least this issue must be handled with caution.

Mentioned references were added, paragraph was reformulated. Additional references describing usage of spheroidal model in lidar retrievals were added. Some discussion concerning usage of spheroids for modeling lidar backscatter observations was provided:

"It should be noted that the studies by Müler et al. (2010, 2012) outlined some potential issues in ability of spheroidal model to reproduce accurately some specific features of the backscattering observations obtained. More recent comparisons of detailed Raman observations with LiRIC retrievals (based on spheroid model) by Wagner et al. (2013) and with AERONET retrieved columnar aerosol properties by Müler et al. (2013) provided notably more positive conclusions regarding the potential of using spheroids for modeling aerosol backscattering properties.

Though uncertainties in interpretation of the lidar observations using spheroids exist, all above studies are in consensus that using spheroids as model of aerosol particle instead of spheres provide significant improvements in interpretation of desert dust observations. Moreover, at present, polydisperse mixture of spheroid is the only physical model used rigorously in operational aerosol retrievals and, based on accumulated results and experience, there are numerous efforts dedicated to improving spheroid model or identifying more accurate alternative model."

Page 2265, line14: Toledano et al. show nice cases of dust and smoke (SAMUM, Tellus 2011)

Reference was added in the **Section 6**:

"The values of single scattering albedo (see fig. 18) at all single layers are in the ranges of typical values for dust and smoke aerosols (eg. Toledano et al., 2011)."

"… SSA shows good agreement with AERONET retrievals and with climatological (Dubovik et al., 2002a) and observed (Toledano et al., 2011) values."

Page 2268, line 1: what does . . .. normal distribution . . .. mean here?
Term "normal distribution" was precised as "Gaussian distribution"

Page 2268, **first paragraph** as a whole: sounds like: from tail to head. . .! .. not necessary! . . ..for what? I was thinking this was just what you want and need: vertical profile information from lidar in AERONET retrieval!
AERONET retrieval requires profile of the aerosol concentration defined in the range from altitude of the measuring sight up to 40 km. Since lidar does not provide profile measurements in this entire range, an extrapolation is needed.

Page 2268, line 14: strange form of writing. . ., better: for h > h-max and h < h-min .
Expression "$h_{min}<h<h_{max}$" changed to "$h>h_{max}$ and $h<h_{min}$".

Page 2269, line 6: you mean power of received signal. . .. and not power of the laser pulse
Phrase corrected to "...the power of the laser pulse returned to a receiver decreases as square of the distance during beam propagation in the atmosphere..."

Page 2269: Eq.(13) is in contradiction with **Eq.(12)**. If no aerosol particles are present above h-ref in **Eq.(13)**, then c(h) for h>h-ref in **Eq.(12)** should be zero, an exponential decrease of c(h) above h-max does not make sense. Or is h-ref different from h-max?
Yes, h_{ref} is different from h_{max}, it is usually chosen at 2 or 3 kilometers higher than h_{max}. Consequently, there is no contradiction in equations. Mentioning of this particularity was added to the paragraph describing h_{ref} (beginning of **Section 3.3**):
"This reference altitude is chosen from the altitudes higher than h_{max} under the assumption, that amount of the..."

Page 2270/71: please check **Eqs.(15)-(18)**, link between W, epsilon, and C in these formulas something seems to be wrong after all the substitutions to get **Eq.(18)**.
Typo in **Equation 18** was corrected.

Page 2269, Eq.(18): what is s-p?
Description was added to the text: "Here, the first group unites N_{meas} sets of independent measurements (with different level of accuracies) and the second represents a priori constraints. It unites N_{prior} sets of known a priori data sets (s^*_p) used as a priori values of characteristics $s_p(a)$."

Page 2271, lines 20-22: instead of i=1,2,3, shouldn't it be k=1,2,3..?
Indices were corrected.

Page 2272, line 20: What does that mean: A is the accumulation of the signal?
Description was added to the text: "A is the number of lidar profiles used for the time-averaging"

Page 2272, Eq. 20: v instead of ω ?
Equation 20 was corrected.

Page 2275, section 5.1, first paragraph: How is sphericity and non-sphericity explicetly considered here
Paragraph with detailed description of the sphericity accountancy was added to the **Section 5.1**:
"Each of the aerosol components was modelled as a mixture of polydisperse spheres and spheroids following equations (5)-(6) with faction of spherical particles (C_{sph}) of 10%, faction of non-spherical particles was 90% correspondingly. The same C_{sph} for coarse and fine aerosol modes was chosen due to the limited sensitivity of the measurements to the shape of smaller particles."

Page 2275, line 3: Two realistic scenarios. . ..? Which one, how are they defined. . ., simulated. . .
Paragraph was reformulated to make the definition of scenarios clearer:
"Two scenarios with clear vertical separation of fine and coarse aerosol components were used. The fine mode was assumed to represent the background aerosol with specific vertical distribution, while coarse mode distribution had a thick layer approximately at 3 km. Both modes had significant amount of aerosol in the layers close to the ground and monotonous decrease over the altitude. Such distributions were chosen to mimic the particularities of aerosol vertical distribution usually found in real lidar observations."

Page 2278, line 15: Ok this example is well defined and easy to understand, but in all other cases of mixtures, I am unable to get a good idea about all the numbers in the figures describing the mixtures, they are to my opinion not consistent.
Mixture cases were described explicitly in the **Section 5.1**. Also additional info on aerosol mixtures was added to the **Table 3**.

Page 2278, lines 24-28: I am not able to find out, when Re/Im is constant, when the ratio varies? Or with other words: Figure 9: besides size distribution information is missing, what is the fixed IM value in the left plot, what is the RE value in right plot?
More accurate description of figure 9 was added: " …different size and shape. Values of lidar ratios depicted in fig. 9 were retrieved using size distributions mentioned in Table 3 with corresponding optical thickness of $\tau_f=\tau_c=0.5$. To

retrieve lidar ratios of spherical and non-spherical particles parameter C_{sph} was set to 100% and 0% correspondingly. Values of the fixed part of complex refractive index were set as 0.05 for imaginary part and 1.55 for real part for the cases with changing real and imaginary parts correspondingly. Specifically, fig. 9 …"
Constant values of Re/Im were added to the corresponding subplots of the **Figure 9**.

Page 2279, lines 3-6: If the solution is ok (without error), why should there be a sensitivity of the aerosol contribution to the wavelength?

More detailed description was added in the paragraph describing spectral sensitivity of the refractive index:

"…Second, the retrieval error of the refractive index increases from shorter wavelengths to longer ones for the fine mode. The tendency for the coarse mode is opposite. Such behavior could be explained by the fact that the efficiency of scattering by small particle reaches the maximum values when size parameter is comparable with the wavelength, thus scattering of small particles is more pronounced at the short wavelengths, and scattering of the big particles is more pronounced at long ones."

Page 2280, line 13: lidar ratio as function fine or coarse mode, what about lidar ratio as function of spherical/non-spherical, as in Figure 9? Figure 10 shows strange spectral dependencies, or?

Each of the aerosol modes contains the mixture of both spherical and non-spherical particles with a 10% faction of spherical particles. Additional paragraph was added to the description of forward modeling (**Section 5.1**) to make it clearer. The dependencies on **Figure 9** were retrieved for two different values of spherical/non-spherical faction C_{sph}. The GARRLiC differentiate only the contributions of fine and coarse modes, each containing both spherical and non-spherical particles, so, unfortunately, requested information for the **Figure 10** couldn't be presented. **Figure 9** was intended to illustrate the variability of liidar ratios and their dependencies on aerosol parameters using spheroid model forward simulations. More accurate description of **Figure 9** was added.

Page 2281, line 3-4: How do you handle C-sph/C-nonsph? . . .. remains unclear. . .

Detailed description of the aerosol model concerning spherical non-spherical particles mixture was added in the beginning of the sensitivity study (see. **Section 5.1**):

"Each of the aerosol component was modelled as a mixture of poly-disperse spheres and spheroids following equations 5 and 6 with faction of spherical particles (C_{sph}) of 10%, faction of non-spherical particles was 90% correspondingly. The same C_{sph} for coarse and fine aerosol modes was chosen due to the limited sensitivity of the measurements to the shape of smaller particles."

Page 2282, line 26: generally in the middle between. . . this is not the case in Figure 14, lower left plot.

Explanation of observed particularity was added:

"Two trends observed in retrievals of the imaginary part of the refractive indexes should be outlined: high absorption of the fine particles in the dust case and very low absorption of the coarse particles for the smoke case (see lower part of the fig. 14). Such retrievals could be explained by very low optical thickness of the minor modes ($\tau_f = 0.19$ for the dust case and $\tau_c = 0.04$ for the smoke case). As it was demonstrated by the sensitivity study, such low contributions of the minor modes could lead to high estimation errors in their complex refractive index."

Page 2283, line 12: where are these unnatural lidar ratios. . .? Please be specific. I found the lidar ratios in **Fig. 10** even more unnatural.

Paragraph was reformulated:

"Retrievals of lidar ratios shown in fig. 16 demonstrate notable differences between AERONET and GARRLiC values. The main difference is located at shorter wavelengths. These differences are probably caused by the significant differences in the sensitivities of both data sets, and by the differences in assumptions. Specifically, AERONET radiometer does not include observations in backscattering direction, and assumption of size independent refractive index may also result in an additional error in the lidar ratio estimation."

Page 2283, lines 26-28: Typical lidar ratios (found in Dubovik et al., 2002a, Cattrall et al., 2005) . Are such AERONET-based lidar ratios trustworthy? Better check the literature for Raman lidar observations, may be check Mueller et al. (JGR, 2007), Tesche et al. (Tellus 2009, 2011), Gross et al. (Tellus 2011), and and may Schuster et al. (AERONET, ACP/AMT 2012), and many EARLINET observations done by Mona et al., deTomasi et al., Amiridis et al., Papayannis et al., etc.

Mentioned works were added. Possible explanation of the difference was given:

"Retrieved lidar ratios (fig. 19) are in the ranges of values for dust and smoke aerosols given by Dubovik et al. (2002a); Cattrall et al. (2005). These values, however, are lower than assumptions for dust particles given by Schuster et al. (2012), Groß et al. (2011) or by Tesche et al. (2009, 2011). The lower lidar ratios in this case could have been caused by contamination of the pure dust layers during the long-range aerosol transport depicted in fig. 11. …"

Page 2284, line 5: Is the lidar ratio height independent in the GARRLIC retrieval?

Total lidar ratio at the given wavelength is height dependent due to the different concentration of fine and coarse modes; each mode has it own vertically constant LR. This was reflected in the text, paragraph describing **Figure 22**:

"… GARRLiC uses bi-component aerosol model that may have different complex refractive indexes. This assumption affects estimations of lidar ratios for each mode and therefore …"

Page 2284, second paragraph: What about an impact of used climatological profiles of temp and pressure (via Rayleigh computations)? The largest uncertainty then is in the blue lidar signals. . ., the lowest in the red signals, spectral slope changes, retrieved size distribution changes. . .

The uncertainty of the molecular scattering profile is accounted in the covariance matrix of the corresponding lidar measurement (parameters α_1 and α_2). This proportionally lowers the weight of the lidar measurement (especially on higher altitudes at shorter wavelengths) therefore increasing the weight of the passive measurements (that suffer less from Rayleigh estimation uncertainties) allowing overcoming described effects on the size distribution.

Figure 15. Dates are mixed or plots mixed?

The places of the plots in **Figure 15** were switched.

Figure 16: 30sr and lower, quite low values. . .!

Such values could be explained by contamination of the dust layers by other particles with lower lidar ratios. Explanation was added: "The lower lidar ratios in this case could have been caused by contamination of the pure dust layers during the long-range aerosol transport depicted in fig. 11…"

Figure 19: smoke lidar ratios are strange, 80 to 90 sr at 355-532nm, and then 35 for 1064nm,. . ., dust lidar ratios 30sr(1064nm), 35sr (532nm), 40-45sr (355nm) quite low. . ..!

The smoke is almost transparent for the IR. Such behavior illustrates this fact. Explanation was added:

"Strong spectral dependence of the smoke lidar ratio observed in fig. 19 illustrates the fact that IR light has less pronounced scattering on the smoke particles than light at the shorter wavelengths."

Figure 22: Does it make sense to keep Rayleigh included?

After detailed revision of the text **Figure 22** was deleted.

General impression: I do not see, based on the results presented here, that GARRLIC is better than LIRIC, please comment on that!

Comparison of GARRLiC and LiRIC was not intent to present garlic as "better" algorithm. The aim was to present a successor of the LiRIC which in comparable conditions provides similar results, yet having sufficiently different approach for treating lidar and radiometer data, relying on less assumptions and providing additional information from the same set of the co-located AERONET and lidar measurements.

More accurate description of the figures was added. Small introduction before **Figure 22** was inserted:

"Figure 22 is aimed to demonstrate the consistency between the LiRIC and GARRLiC retrievals in a case where no differences are expected. Both algorithms provide two distinct vertical concentration profiles for different aerosol components and the comparison of profiles retrieved by GARRLiC and LiRIC was made. The main difference is that GARRLiC modifies the retrieved columnar properties of aerosol. In addition, GARRLiC uses bi-component aerosol model that may have different complex refractive indexes. This assumption affects estimations of lidar ratios for each mode and therefore the retrieved vertical profiles. Therefore the demonstration of LiRIC and GARRLiC codes consistency has been performed1215 suing the case with small difference in complex refractive in- dices of fine and coarse aerosol modes (see fig. 15)…"