Author's response to anonymous referees and community comments

Anonymous Referee #4

Minor comments:

Lines 18-20: "lidar signals suffer from the uncomplete overlap […]. This occurs because a part of rays [...] do not pass the receiver field stop".

This sentence literally states that field stop always acts as a field diaphragm. This is true if the optical system is properly designed otherwise any optical device all along the optical path — from the telescope to the detector— can act as a field diaphragm. I suggest clarifying this in the text.

We agree with the reviewer's remark. We have changed the sentence to "The overlap function of a lidar system can be defined as the ratio between the power scattered by a scattering volume at a given range that reaches the photodetector (excluding transmission losses) and the power scattered by the same scattering volume that reaches the telescope aperture (Comeron et al. 2011)"

Lines 30-50:

I would suggest rewriting this paragraph to show chronologically how the advances in overlap function retrieval have been implemented from the first tries and its inconveniences to the current state-of-the-art. This may help the reader to identify the framework in which this paper arrives.

We appreciate the suggestion of the Referee. We have included additional references to different approaches to the estimation of the overlap function.

Line 224: "This formula allows one to assess the errors committed when an erroneous lidar ratio is used".

Does the lidar ratio affect the method described in Wandinger and Ansmann, 2002? If not, would the iterative method be more convenient? I do not find a clear comparison between the iterative method and the new one.

Yes, the lidar ratio affects indeed Wandinger and Ansmann method; it also requires an estimated or measured lidar ratio and, as happens with our proposal, if there exist an error in the guess it will produce an error in the overlap profile estimation.

Although using a different, explicit (as opposed to iterative) formulation, the method presented in this paper relies on the same basis than the one by Wandinger and Ansmann and respective results are indistinguishable for a sufficient number of iterations in the iterative method. We have added a sentence in the paper stating this fact at the end of section 4:

"Although using a different, explicit, non-iterative formulation, the method presented in this paper relies on the same basis as the one given by Wandinger and Ansmann. The reader can check that, for the same measured data and assumed lidar ratio, both methods, for a sufficient number of iterations in (Wandinger and Ansmann 2002), yield indistinguishable results"

Moreover, we have added at the end of section 2 the following sentence:

"Although based on the same principles as the iterative method proposed in (Wandinger and Ansmann 2002), the formulation of Eq. (15) has the advantages of not requiring iterations (admittedly, not a decisive issue with the current computing technology) and, more important, of providing insight on the effect of the assumed aerosol lidar ratio on the retrieved overlap function (see section 3) and on the systematic error incurred when the differential aerosol transmission at the emitted and Raman wavelengths cannot be neglected (see appendix B)"

Major comments:

Line 27: "The overlap function is usually zero […] grows up to one."

I do agree with this sentence. If so, why the retrieved overlap functions in Fig. 2 and Fig. 3 are larger than 1? Indeed, their behavior is the opposite: instead of increasing from 0 to 1, they decrease from a maximum (~1.05 in Fig. 1 top) to 1.

The statement in line 27 is in fact true if the system is ideally aligned. If that is not the case, as we can conclude that happens in the cases presented in the paper, the overlap function can show more complex profiles. In particular, if the laser beam axis and the receiver axis are slightly convergent, it can rise to a maximum value and then to stabilize to a smaller constant value for far ranges. Because we take conventionally as 1 this stable value, the maximum value can be higher than one. To make this clear we have:

a) replaced throughout the paper the term "full overlap" by "stable overlap"

b) We have re-written the paragraph starting in line 27 in the original manuscript. Now it reads (lines 26-29):

"In a perfectly aligned system, the overlap function is zero at the telescope aperture level and progressively grows up to a constant value, where all the backscattered radiation collected by the telescope aperture, or at least a constant proportion of it, reaches the photodetector. In practical cases, misalignments may make the overlap function dependence on range depart from the ideal behavior just described."

b) emphasized that the stable overlap value is normalized to 1:

"We assume as well that at that range the overlap function has attained a constant value that we set conventionally to 1" (lines 80)

c) added an explanation in section 4 (lines 249ss):

"Because we have arbitrarily normalized the profile to the reference height, where the overlap function has reached a stable value, values greater than one, as shown in figs. 2 and 3, at lower ranges are possible and reveal a non-perfect alignment, in particular, a slight crossing between the laser beam and the receiver field-of-view axes, leading to a loss of energy from the far range (see for example fig 1(a) in (Kokkalis 2017) with laser tilt A_{tilt}, half-width laser beam divergence LBD and receiver field of view $RFOV$ fulfilling the conditions $A_{tilt} + LBD > RFOV$ and $A_{tilt} - LBD < RFOV$)"

Lines 124-149: Section "Influence of the lidar ratio".

This section discusses the influence of assuming the lidar ratio. However, since elastic and Raman signals are available, the lidar ratio profile can be retrieved by independently estimating the extinction and backscattering coefficients. Why is it not used? Would the noisy retrieved lidar ratio profile be worse than the assumed constant lidar ratio? I suggest comparing the overlap function obtained assuming the lidar ratio and the one obtained with the retrieved lidar ratio profile. This comparison may be performed in section 4, contributing to the discussion, and clarifying the assumption.

The lidar ratio would have to be estimated in the stable overlap region (section 3 implies that it cannot be estimated in the zone subject to the overlap correction, as explained in the paragraphs added in the conclusions section). This makes it difficult to assure that it remains constant down to the varying overlap region, which would in turn introduce an uncertainty in the overlap function retrieval. We discuss the problems associated to the estimation of the lidar ratio needed to retrieve the overlap function in lines 278ss of the conclusions:

"Section 3 also cautions against trying to derive a lidar ratio using the corrected-for-overlap signal. Actually, one could be tempted to think of the following procedure: an overlap function is retrieved using a guessed aerosol lidar ratio; with that overlap function, the Raman signal is corrected and an aerosol extinction coefficient is calculated, which, divided by the aerosol backscatter, gives a new lidar ratio, which is turn used to retrieve a new overlap function, and so on. However, Eq. **¡Error! No se encuentra el origen de la referencia.** shows that this procedure does not converge, for, if a too low lidar ratio is used as first guess, the overlap function will be enhanced in the range with aerosol; when correcting with this enhanced overlap function, the Raman signal will be suppressed, which will give rise to an aerosol extinction coefficient lower than due and, consequently to a lower new lidar ratio. A similar reasoning goes on if the guessed aerosol lidar ration is too high. The determination of the required lidar ratio from Raman inversions needs atmospheric regions with both significant aerosol load and stable overlap. However, in cases with regions where both conditions are fulfilled, using the retrieved lidar ratio for overlap estimations requires assuming that the type of aerosol is uniform down to the ground. Moreover, as seen in section 3, in aerosol loaded scenarios, errors in the lidar ratio determination yield greater errors in the estimation of the overlap profile. A more conservative approach is to stay with situations with low aerosol load at low altitudes and use the aerosol backscatter profiles derived with the Raman method (e.g. fig. **¡Error! No se encuentra el origen de la referencia.**). together with a sun- or lunar-photometer AOD measurement, and find the aerosol lidar ratio that, multiplied by the integrated aerosol backscatter coefficient would, would yield the AOD measured by the photometer. However these techniques are out of the scope of this paper, which aims only at presenting the explicit formulation of the overlap function and discussing the effect of the assumed lidar ratio on the retrieved profiles."

Line 150-215: Section "Results"

Following my comment on Line 224, I miss a direct comparison between the new method and the iterative one (Wandinger and Ansmann, 2002). I strongly suggest including it. Are the methods equivalent?

Yes: both methods are equivalent.

There are many features to be compared: computing time

Both methods are very quick in modern computers

, cost of assuming lidar ratio versus iteration,

Both methods assume a lidar ratio value

result stability,

We have not observed any unstability in the iterative method (Wandinger and Ansmann).

possibility to obtain uncertainties,

We have performed an uncertainty analysis with the Monte Carlo method (see lines 200-)

and so on. I want to clarify that my suggestion of including this assessment is not to decide if the paper should be published or not but to provide future readers with an impartial perspective of the impact of the new method.

As stated before, we have added the sentence at the end of section 4 (lines 258ss) has been included:

"Although using a different, explicit, non-iterative formulation, the method presented in this paper relies on the same basis as the one given by Wandinger and Ansmann. The reader can check that, for the same measured data and assumed lidar ratio, both methods, for a sufficient number of iterations in (Wandinger and Ansmann 2002), yield indistinguishable results"

Anonymous Referee #3

My recommendation for the paper is to proceed and publish as is.

Thank you very much

Anonymous Referee #2

- Some comments related to the scientific content:

Line 64: The expression of the molecular lidar ratio Sm = 8π/3 sr is an approximation, whereas it depends on depolarization and refractive index at each wavelength (see Bucholtz A., Rayleigh-scattering calculations for the terrestrial atmosphere, APPLIED OPTICS, Vol. 34, No. 15, 1995). Which is the influence of this assumption on the overlap estimation?

We have modified the formulas to include this dependence of molecular lidar ratio. Its effect (very small) is illustrated in fig. B1 of appendix B

Line 98-99: About the sentence "we neglect the difference between the aerosol extinction coefficients at λ0 and λR". This assumption could be acceptable in the case of the pure rotational Raman, but not in general, like for 532 nm in this paper, where the vibro-rotational Raman at 607 nm is used. The authors should explain better.

An appendix (B) has been added to the paper to address this issue.

Line 164-165: see previous comment.

An appendix (B) has been added to the text to address this issue.

Lines 169-173: The authors say that "the 355-nm has a sudden fall below approximately 400m. For this reason, in this particular case of optical alignment we should distrust the overlap function retrieval below that height". Why did not the authors try to improve the alignment and use a better measurement?

This undesired feature in the system was discovered while preparing this work and we are still finding out the cause of the issue.

Figs. 2 and 3:

Did the authors performed a comparison with the overlap functions obtained from the Wandinger and Ansmann method?

Although using a different formulation, the method presented in this paper relies on the same basis than the one by Wandinger and Ansmann and results are indistinguishable. We have added a sentence in the paper stating this fact (Lines 258ss):

"Although using a different, explicit, non-iterative formulation, the method presented in this paper relies on the same basis as the one given by Wandinger and Ansmann. The reader can check that, for the same measured data and assumed lidar ratio, both methods, for a sufficient number of iterations in (Wandinger and Ansmann 2002), yield indistinguishable results"

The overlap function should not be higher than 1, contrary to what the figures show, suggesting that the overlap function contains further kind of corrections. This behaviour is present also in low aerosol load conditions that should be used to retrieve the overlap function, so affecting the extinction and backscatter retrieval that, as known, is particularly critical at low range. In this context, how the authors use the overlap function, especially at low range, if the overlap function presents the reported dependence on the lidar ratio?

Because we have arbitrarily normalized the profile to the reference height, where the overlap function has reached a stable value, values greater than one, as shown in figs. 3 and 4, at lower ranges are possible below the region where profiles stabilize to unity and reveal a non-perfect alignment, in particular, a slight crossing between the laser beam and the receiver field-of-view axes, leading to a loss of energy from the far range.

This overlap function corrects overestimated (because of less than perfect alignment) signal values at low ranges.

We have added a clarifying paragraph (lines 249ss):

"Because we have arbitrarily normalized the profile to the reference height, where the overlap function has reached a stable value, values greater than one, as shown in figs. 2 and 3, at lower ranges are possible and reveal a non-perfect alignment, in particular, a slight crossing between the laser beam and the receiver field-of-view axes, leading to a loss of energy from the far range (see for example fig 1(a) in (Kokkalis 2017) with laser tilt A_{tilt}, half-width laser beam divergence LBD and receiver field of view $RFOV$ fulfilling the conditions $A_{tilt} + LBD > RFOV$ and $A_{tilt} - LBD < RFOV$)".

In addition throughout the document we have replaced the term "full overlap" by "stable overlap" and emphasized that the stable overlap value is normalized to 1:

"We assume as well that at that range the overlap function has attained a constant value that we set conventionally to 1" (line 80)

Which is the full overlap altitude of the system used by the authors in the paper?

We prefer talking about "stable overlap". Right now, according to our calculations, the altitude for stable overlap is around 4 km. Below that range the overlap has a small enhancement (see response to previous question).

- Some suggestions related to the text:

Line 31: I suggest replacing "To overcome" with "To reduce", because the problem is not removed.

Done

Line 71: In eq. (2), maybe, it should be O(Rm) instead of O(R).

We have eliminated $O(R_m)$ (O(R) in the original).

Anonymous Referee #1

-- The authors use an aerosol free region as a reference point and then start calculating the overlap below using an analytical approach. The problem is that according to Eq 18, the error due to the lidar ratio is cumulative with height. The point b, c, and d on page 5 are not correct and should be update in order to highlight this. By starting the overlap calculation at an aerosol free region, one carries along a systematic error accumulated from all the previous altitudes until the actual full overlap is reached. But then it doesn't make sense to use this technique because the accumulated error can be simply too large. This effect can be partly seen in figures 2 and 3 where the overlap function is constantly and significantly > 1. even above the full overlap range (2-3km).

Respectfully, we disagree with the Referee: below the Rm altitude, and above the altitude, corresponding to the maximum aerosol height (see figure 2), in the aerosol-free interval an incorrect selection of lidar ratio value is irrelevant, because the aerosol backscatter coefficient is 0 an it doesn't add to the error (Eq. 18). This is shown by the fact that the different retrieved profiles match from 2 km upwards (see figures 3 and 4) irrespective of the lidar ratio employed. We have modified the text to stress that we mean that there are no aerosol layers between R_T and R_m.

The fact that the retrieved overlap profile goes slightly over 1 points out that a slight misalignment (a small convergence between the laser beam axis and the receiver field-of-view axis) is present. We have added a clarifying paragraph in lines 249ss:

"Because we have arbitrarily normalized the profile to the reference height, where the overlap function has reached a stable value, values greater than one, as shown in figs. 2 and 3, at lower ranges are possible and reveal a non-perfect alignment, in particular, a slight crossing between the laser beam and the receiver field-of-view axes, leading to a loss of energy from the far range (see for example fig 1(a) in (Kokkalis 2017) with laser tilt A_{tilt}, half-width laser beam divergence LBD and receiver field of view $RFOV$ fulfilling the conditions $A_{tilt} + LBD > RFOV$ and $A_{tilt} - LBD < RFOV$)".

The referee includes comments on this same section contained in the PDF. We think that we have addressed them in the paragraph above.

To circumvent this issue, I recommend one of the following alternatives.:

The authors could use a measured aerosol lidar ratio profile between the aerosol free region and the full overlap range. In that way they avoid the accumulation of uncertainty from the previous layers. The lidar ratio above the full overlap region should be known from the Raman inversion (no iterations needed).

It could be possible to apply this technique (perhaps without too critical adaptations) by starting from a height where the aerosol extinction and backscatter coefficients are known. Such values are already available from the Raman inversion (above the full overlap range).

To illustrate the technique, we have preferred using guessed values of the lidar ratio in measurements with clean air conditions. Reasons for this choice are stated in the following paragraph, added at the end of the conclusions section (lines 285ss):

“The determination of the required lidar ratio from Raman inversions needs atmospheric regions with both significant aerosol load and stable overlap. However, in cases with regions where both conditions are fulfilled, using the retrieved lidar ratio for overlap estimations requires assuming that the type of aerosol is uniform down to the ground. Moreover, as seen in section 3, in aerosol loaded scenarios, errors in the lidar ratio determination yield greater errors in the estimation of the overlap profile.”

-- The authors assume that the backscatter and extinction cross sections for both molecules and aerosols do not change significantly between the elastic and the vibrational Raman wavelength and use this assumption to simplify a term to go from Eq 9 to Eq 10. This is not correct. For molecules, relative differences of the Raman with respect to the elastic cross section are in the order of 30% (40%) for 355nm (532nm). For aerosols the difference are indeed smaller ~10% (13%) for an Angstrom of 1 but can generally range from zero to 23% (33%) for an Angstrom ranging from 0 to 3. The authors should:

include at least the molecular wavelength dependency of the backscattering/extinction cross sections in their formulas because it is known and well parameterized (proportional to λ-4).

either include the aerosol Angstrom exponent as an additional parameter to the equations along with its uncertainty, similar to how the treated the lidar ratio uncertainty (ideal solution)

or, if this makes things impossible to solve analytically, provide a paragraph with a theoretical analysis of the expected uncertainty due to a varying Angstrom exponent

In Eqs. 10 the difference in the molecular extinction between the two wavelengths was already considered. Nevertheless, we changed this equation as well as Eqs. 12, 13 , 14 and 15 to include the difference in the aerosol extinction values for both wavelengths.

Appendix B states the bounds of the error incurred by not considering this difference.

-- The experimental application faces many challenges, such as unrealistic overlap values above the full overlap range and sharp drops to the Raman backscatter profiles below 400m even though they should be, in theory, overlap independent. From my point of view the paper stands just fine with the theoretical part and an experimental application is not necessary. However, if the authors want to include it then they should make sure that:

the overlap between the Raman and the elastic channels is indeed the same. Different interference filters (IFF) are applied per channel that can lead to overlap-like effects in the signals as the angle of incidence (AOI) of the collected beam on the IFF changes from the near range to infinity. This is especially important for the Rotational Raman channel because changes in the AOI create an effect equivalent to shifting the IFF transmission with respect to the incident wavelength. Such overlap-like effects are expected to be pronounced in a Rotational Raman channel due to the proximity of the central wavelength of the IFF to the Cabannes line and due to the temperature dependence of the Rotational Raman cross sections.

A good experimental way of verifying this is to check whether the 355-387 derived overlap is the same as the 355-354 one, or wether the Raman backscatter at 355-354nm is the same in the near range as the 355-387 one. For 532-607nm the authors could prove that by using an IFF with a different bandwidth in subsequent measurements they get the same overlap function. Without such a verification I wouldn't recommend publishing the experimental part.

We are aware of these possible effects.

Nevertheless, we have already given an explanation (included in the paper, lines 249ss) about the values above 1 in the retrieved overlap profile:

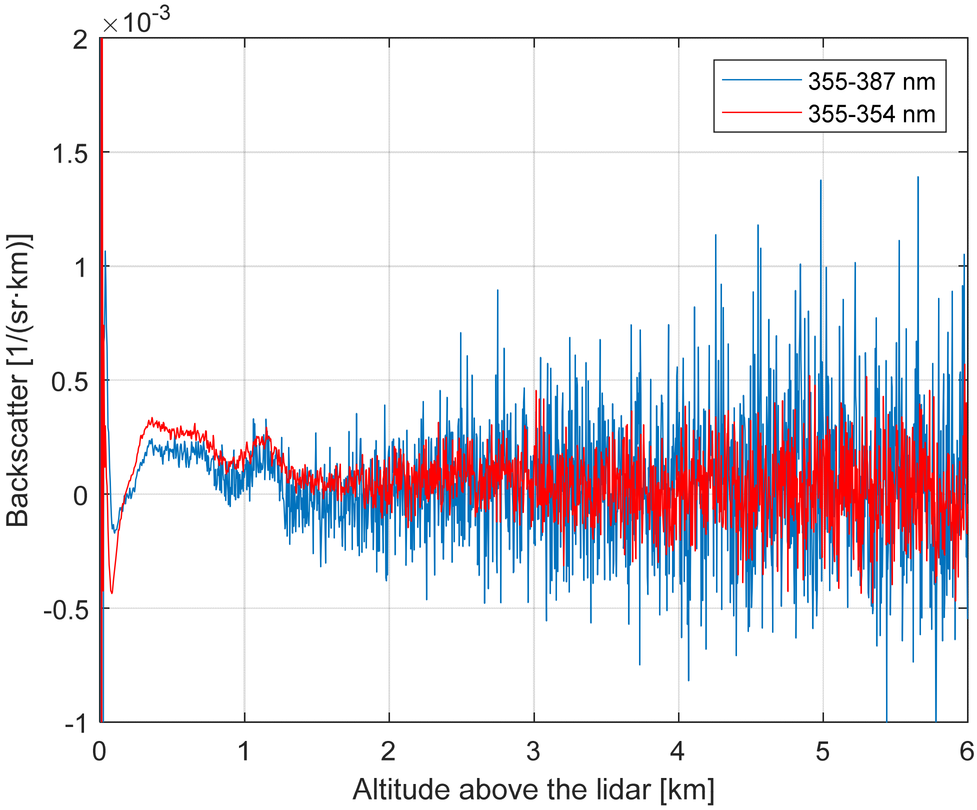
1. On the one hand the overlap function value is arbitrarily fixed to 1 at the reference height. This may correspond to a stable overlap condition, but not necessarily to a full overlap one.
2. Because of this if there is a slight crossing between the transmitted beam axis and the receiver field-of-view axis, then an overlap value greater than 1 can be expected.
3. As shown by equation 18 a too low lidar ratio value would lead to enhance overlap values in the zone with aerosol content.

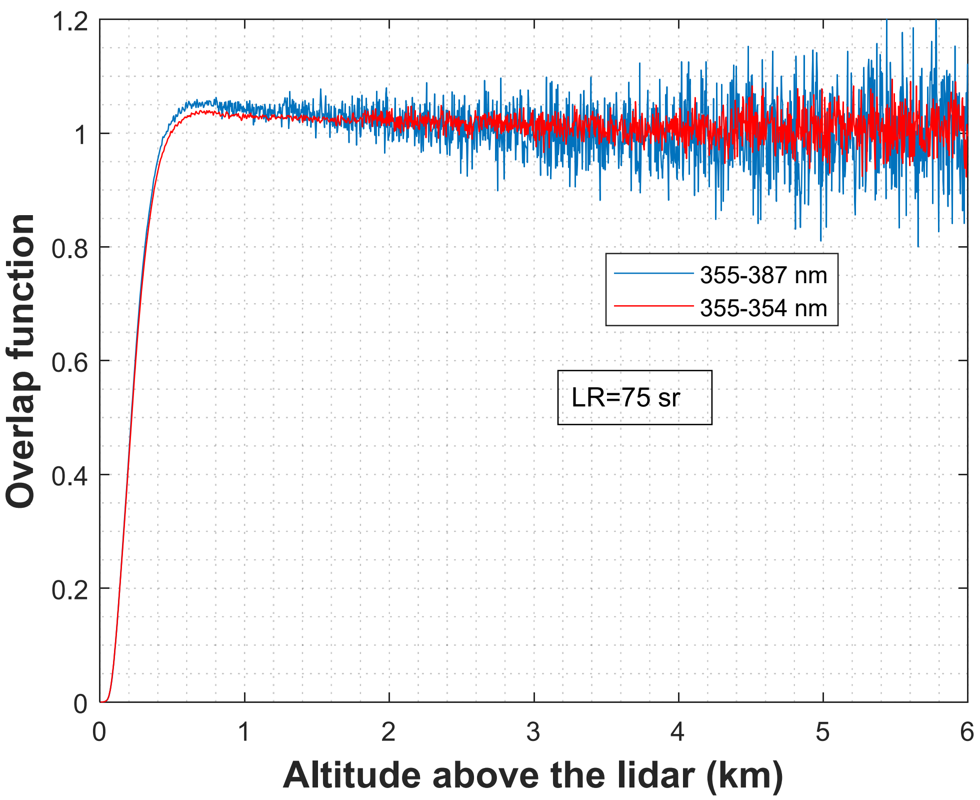
This undesired feature under 400 m in the system was discovered while preparing this work and we are still trying to find out the cause of the issue, as we have recognized in the paper by showing this effect in figure 2.

Regarding the comparison proposed by the reviewer: unfortunately

1. The signal at the VBR 387 nm channel is noisier than the signal from PRR 354 nm. The design of the PRR channel has been carefully made as it was reported in (Zenteno-Hernández et al. 2021). Therefore, we have more confidence in the overlap retrieved with the 354 nm.
2. Using the PRR channel in eq 9 minimizes the effect of discarding the difference in atmospheric transmission between the elastic and Raman channels that is applied in the practical calculation of eq 15

As an example, we have computed the Raman backscatter coefficient at 355 and the overlap retrieval with both Raman channels for the Dec 1st measurement. The reference height is 6 km.





Given the noise in the 387 nm channel, we give more confidence to the calculation using 354 nm.

Regarding the proposal of using different interference filters at 607 nm, it is not possible given the material limitations of our system. In addition, our system present configuration is no longer that of the end of 2021.

Nevertheless, we still consider that a practical method like the one presented should be illustrated with the available data.

With all the caveats put forward by the reviewer, we think that one of the paper contributions is to formulate the effect of the assumed lidar ratio in the overlap profile retrieval.

They provide information of the expected full overlap of the system per case. This can be done with a telecover test or with a Ray-tracing simulation.

We do not have a tele-cover test close enough to the time of the measurements. Regarding the ray-tracing simulations, they would to rely on parameters in general not exactly known, as discussed in the introduction.

The scope of our paper is an empirical determination of the overlap profile.

More specific comments can be found in the uploaded pdf file with inline comments.

We have included changes in the text, following the referee's suggestions.

Some more specific issues:

Line 45: "reasonable" has been removed here and elsewhere.

Line 97: the aerosol differential transmission has been kept throughout the formulations and explicitly stated when neglected.

Line 144ss: Answered previously in this text.

Line 149: We are not sure to understand the reviewer's comment: We are not writing about the elastic and Raman channels but about the difference between the visible and UV channels. Nevertheless, we do not intend this sentence to be categorical in any way, only to express a common situation.

Line 155: Each channel has an eye-piece as it is reported in Kumar et al 2011. We do not have simultaneous telecover for these measurements.

Line 158: We do not have a vibro-rotational channel at 387 nm anymore. In Zenteno et al 2021 we have reported the low sensitivity of the 354-nm PRR channel to temperature changes.

Line 170: we agree with the reviewer that there is something strange at low ranges that should be further investigated. We have stressed that the overlap recovery should be distrusted under 400 m for all cases.

Community Comment #1

For completeness in the presentation of your proposed overlap method you may want to mention the studies cited below which directly retrieved the overlap function for a similar system. Both studies determinted the uncertainty in their overlap function, including the affect of lidar ratio.

Mahagammulla Gamage, S., Sica, R. J., Martucci, G., & Haefele, A. (2019). Retrieval of Temperature From a Multiple Channel Pure Rotational Raman-Scatter Lidar Using an Optimal Estimation Method. Atmos. Meas. Tech., 12, 5801-5816.

Povey, A. C., Grainger, R. G., Peters, D. M., Agnew, J. L., & Rees, D. (2012). Estimation of a lidar's overlap function and its calibration by nonlinear regression. Applied Optics, 51(21), 5130-5143.

Community Comment #2

The basic method in the following paper is similar with the preprint, and also discussed the influence of lidar ratio. The authors can make some discussion based on the conclusion of the following paper.

Jian Li, Chengcai Li, Yiming Zhao, Jing Li, and Yiqi Chu, "Geometrical constraint experimental determination of Raman lidar overlap profile," Appl. Opt. **55**, 4924-4928 (2016), http://dx.doi.org/10.1364/AO.55.004924

We appreciate the suggestion of Dr. Tan. The paper has been referenced and commented in the text

Author’s comments to editors

At the requirement of some reviewers, we included:

- A new figure 1, including the Rayleigh-fitted signals that were used in the calculations.
- Figure 1 now is figure 2
- Figures 3 and 4 (previously figures 2 and 3) were re-calculated in concordance with comments from AR#2.
- An appendix B has been added at the end of the text, addressing the expected error because of an initial neglection of the difference in molecular lidar ratio values and the differences in the differential aerosol transmission term.