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:

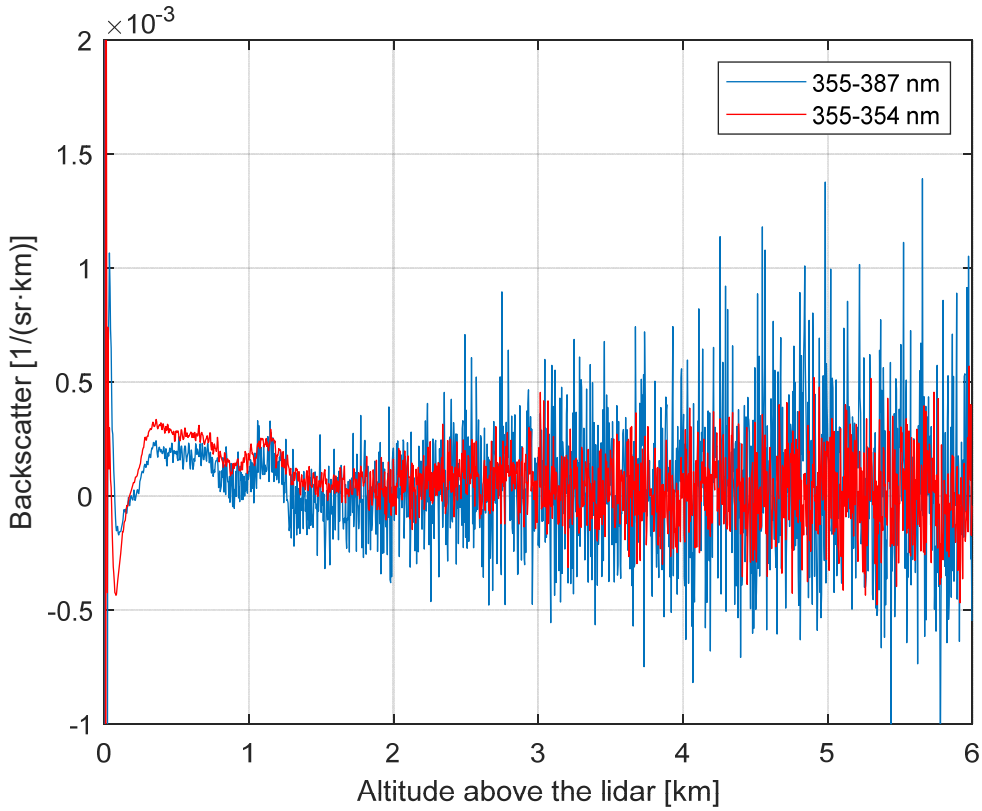
1. On the one hand the overlap function value is arbitrarily fixed to 1 at the reference height. This may correspond to a stable overlap condition, but not necessarily to a full overlap one.
2. Because of this if there is a slight crossing between the transmitted beam axis and the receiver field-of-view 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.

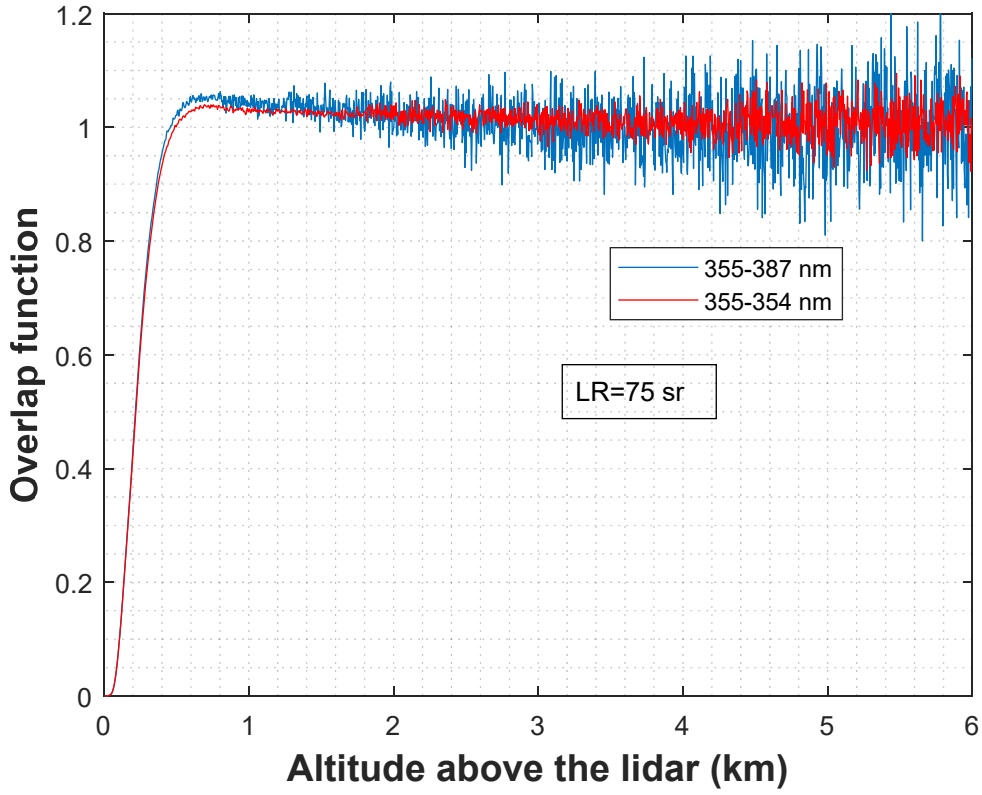
This undesired feature under 400 m in the system was discovered while preparing this work and we are still trying to find out the cause of the issue, as we have recognized in the paper by showing this effect in figure 2.

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.