AMT-2019-107: Retrieval of Temperature From a Multiple Channel Pure Rotational Raman-Scatter Lidar Using an Optimal Estimation Method (Gamage et al.)
Response to Referee 2 (**Anonymous Referee #1)**
26 September 2019

The Referee's comments are in blue, are responses are in black. Red sections have been added to the manuscript.

Since the proposed technique seems to be universally applicable to temperature rotational Raman lidar systems, more references to the state-of-the-art of this technique and other existing systems would certainly be interesting for the readers and should be included.

We will add the following paragraphs to the introduction of the paper:

Behrendt (2005) provides a comprehensive overview of the traditional rotational Raman lidar temperature calculation method. Over the years the traditional temperature method has been improved by advancing the instrumentation capabilities and by improving the estimation and calibration techniques. Here are some examples of innovations in this area since the Behrendt (2005) review.

Studies by Radlach et al. (2008) and Weng et al. (2018) introduce changes to the individual Raman lidar systems to improve the temperature measurements. Radlach et al (2008) introduced a new high-resolution rotational Raman lidar system with a receiving system that uses multicavity interference filters in a sequential setup to improve the efficiency of the elastic and rotational Raman signal separation. Together with the filter adjustments they have made noontime temperature measurement with uncertainties less than 1 K up to 1 km for 1 min integration time. Weng et al. (2018) introduced a new PRR lidar system that effectively detects two isolated N_2 molecule PRR line signals and elastic backscatter signals. With this new system, temperatures at any given time can be obtained without calibration.

The accuracy of the traditional Raman temperature estimations highly depends on the estimation of the calibration function and calibration coefficients. Zuev, Vladimir V., et al. has investigated the use of nonlinear calibration functions to improve the accuracy of the traditional Raman temperature estimation and somehow compensate for the heavy assumption of single PRR line. The results showed the $1/T$ term expressed in a form of a quadratic function of log of the ratio of the PRR measurements is the best for practical use. Another study by He, Jingxi, et al. proposed a new calibration method for PRR lidar temperature profiling based on the different temperature sensitivities of Stokes and anti-Stokes PRR lines. In this paper they reconstruct the expression of the differential backscatter cross section according to the temperature dependencies of each component and form a temperature factor and a calibration factor in the intensity ratio. This new method has reduced the temperature error by ~50% compared with the commonly-used calibration methods in conditions of low signal-to-noise ratio (SNR).

Temperature profiling from Raman lidar backscatter measurements can also be improved using retrieval schemes based on OEM as we introduce in this paper. A previous study by Yan, Qing, et al. has also proposed an optimized retrieval method for traditional Raman temperature profiling. The proposed method allowed independent alternating solutions to high- and low-quantum-number PRRSs, where high-quantum-number PRR lidar returns are used to solve the channel constant, and low-quantum-number PRR returns with high SNR are used for retrieving temperature profiles. The results showed that the effective temperature retrieval height greatly improved from 17 to 25 km under clear weather conditions and better than 5 K can be obtained up to 25 km.

You mention two assumptions on page 8, line 25/26. What do you mean with "well known"? How critical are these assumptions? When highlighting which assumptions are NOT needed (see abstract, introduction, conclusions), you should be fair and also mention which are needed.

"Well known" was a poor choice of words, we meant the *a priori* extinction profile is based on a backscatter ratio measurement by the lidar, and that the overlap is based on estimates from clear sky measurements compared to the expected overlap (please see our response to your comment on page 15 in the Minor Comments section which explains this in more detail). To estimate an extinction profile requires the assumption of a lidar ratio.

As part of our response to Referee 1 (Christoph Ritter) comments we have added the following paragraph to the paper that explains our approach of the overlap and extinction retrievals.

"The effect of geometrical overlap and particle extinction on the signals are strongly coupled and hence retrieving both parameters simultaneously with the given data channels is not possible unless at least one of the effects is highly constrained. We assume that particle extinction is well known from the backscatter ratio outside clouds, and that overlap is well known above the height of full overlap, i.e. above 6 km (Dinoev et al., 2010).We use this knowledge to define a transition height, 6 km in clear skies or at the cloud base height, whatever is lower. Below this height overlap is retrieved, and above this height particle extinction is retrieved. The *a priori* overlap function is estimated from measurements in clear sky conditions. A 50% standard deviation is used for geometrical overlap below the transition height and a constant standard deviation of 10^{-3} is used above this height, constraining the geometrical overlap to the *a priori* values above the transition height. For particle extinction, a standard deviation of 10^{-6}km^{-1} is used below the transition height to constrain the retrieval, then a 50% standard deviation is used above this height, allowing the OEM to retrieve exclusively the particle extinction. The *a priori* covariance matrices for both particle extinction and geometrical overlap are determined using a tent function with a 100m correlation length."

Maybe it would be interesting to explain in the abstract and conclusions that (how much) the results are independent from the selected *a priori* temperature profile.

Similar to Sica and Haefele (2015) , we trust our retrievals upto a certain cutoff height where the response function falls below a value of 0.9. Thus, our retrieval depends on maximum of 10% of the *a priori* temperature profile and that dependency matter only in the heights where the signal strength becomes weaker. One can choose different values of response function to define the cutoff height.
We will add the following sentences to the conclusion explaining the dependency of the temperature retrievals from the selected *a priori* temperature profile.

The OEM retrieved temperatures depend nearly to 100% of the measurement. At the cut off height, the measurement contribution falls below 90% . Temperature retrievals above the cutoff height depend to more than 10% on the *a priori* temperature profile and are not considered an independent measurement.

Page 9, line 4: There are always aerosols in the troposphere. How few are acceptable?

In our study we define the clear and cloudy conditions in the atmosphere based on the backscatter ratio profiles we calculate using RALMO elastic and PRR measurements. When there are no clouds or thick aerosol loads found we have observed the backscatter ratio is less than 2. We consider such cases to be cloud free and retrieve the overlap function up to 6 km.

I think the term "digital measurements" for photon counting signals is odd. Also the analog signals are digitized. Thus, I suggest that you write "photon counting" throughout the text.

We will change the term digital measurements to photon counting.

You are correct, this error was also pointed out by Referee 1. We made a mistake in the analog measurement units. Our response here is the same as to Referee 1:
"The RALMO analog raw data are sampled by Licel counters, and then converted to counts (ADC) . However, it does not change the unit of the analog signal. Thus, we have made a mistake in the analog signal units in Figures 1, 7,12 and 18. Units for the analog signals are now corrected to the units of mV."

Minor comments:

“Lidar constants” and “coupling constants” are terms which are not commonly used. Please define/explain or avoid. E.g., page 1, line 5: “ratio of efficiencies” is better I think since the laser power, transmitter efficiency, telescope area are the same for both channels and thus cancel and become irrelevant (if I understand correctly).

Lidar constant /lidar system constant (Liu, Z., Voelger, P., & Sugimoto, N. (2000), Tao, Zongming, *et al*. (2008) ,Winker, D. M., and M. A. Vaughan, Kovalev(1994), Vladimir A., and H. Moosmüller (1994)) refers to combining instrument and physical constants into a single constant. While individual instrument parameters can often not be determined, such as the optical efficiency of the system, the overall value of the constant can be estimated using the return photocount profiles (e.g. Sica *et al.* 1995). The term coupling constant, the ratio of the two lidar constants, not a common term. This ratio was called a coupling constant by Sica and Haefele (2016), since it mathematically coupled the lidar equations for physical separate counting channels in their forward model. In this study a coupling constant is used in the forward model to couple the measurements from the two PRR digital/analog channels. We would prefer to define and use the term coupling constant in the manuscript rather than ratio of efficiencies. We will make sure these terms are well defined.

Page 1, line 21: “. . .2 flights per day”. One could add: “only at selected sites worldwide”.
We will add this.

Page 1, line 22: Please add references to other combined Raman lidar systems. There are several which measure water vapor and temperature.
We have added the following references:

1. Mattis, Ina, et al. "Relative-humidity profiling in the troposphere with a Raman lidar." *Applied optics* 41.30 (2002): 6451-6462.
2. Wang, Yufeng, et al. "A detection of atmospheric relative humidity profile by UV Raman lidar." *Journal of Quantitative Spectroscopy and Radiative Transfer* 112.2 (2011): 214-219.
3. Reichardt, Jens, et al. "RAMSES: German Meteorological Service autonomous Raman lidar for water vapor, temperature, aerosol, and cloud measurements." *Applied optics* 51.34 (2012): 8111-8131.
4. Behrendt, Andreas, et al. "Combined Raman lidar for the measurement of atmospheric temperature, water vapor, particle extinction coefficient, and particle backscatter coefficient." *Applied optics* 41.36 (2002): 7657-7666.

If we have missed anything else please let us know and we would be glad to add it.

Page 3, Eq. 1: You assume that all PRR lines of one channel are collected with the same efficiency. This is generally not the case but may be true for RALMO. Please add a comment on this.

We do not consider the efficiencies of all the PRR lines are the same. Transmission of the receiver at the wavelength of the PRR line given by $\tau_{RR}(J_i)$ in Eq.1 represents the efficiencies of each detected by the RALMO system. $\tau_{RR}(J_i)$ for RALMO are known (Figure 1, Dinoev, T. S., *et al*) and used in our forward model.

Page 3, Eq. 2: This equation is not found in Penney et al. 1974.

This equation is from Behrendt A (2005) and the original equations are based on Penney et al. 1974. We will fix this in the manuscript.

Page 5, line 11: Why is the background noise B_RR a function of height? I think the "real background" should be height independent. If the baseline is height dependent, detector non-linearity or electronic cross-talk is present, I guess.

In general background noise B_{RR} is a function of height. For some systems this can be independent of height. On Page 9 line 11 we have explained that for RALMO background noise is independent of height.

Page 7, table 2: What do you with "transition height"?

We have fixed this to:

Geometrical Overlap Function	Estimated using the forward model and measurements	50% **below and at** transition height
Particle Extinction	Estimated using measurements	10^{-6}km^{-1} **below and at** transition height

Page 8, line 6: beta_par *is* related

Changed the text.

Page 9, line 20: I would prefer "model parameters" or "model b parameters"

Changed the text.

Page 9, line 21: Please refer to the figure.

This sentence is now changed to "The traditional temperature profiles that will be shown later in this section are calculated using count profiles consisting of glued analog and digital measurements which are corrected for non-linearity and background before processing".

Page 10: line 6: What do you mean with "dominant"?
In lower altitudes signal strengths are higher and as the height increases the signal gets weaker. Then the effect from the electrical offset starts to dominate and analog signal in higher altitudes will have values that are not correct.
As we think the word dominant poor choice of words we will change this to be "becomes larger."

Page 10, line 7: How do you know that the analog signals are linear?
RALMO uses Licel transient recorders for data acquisition with Licel's Hammatsu PMTs. The Licel transient recording for analog measurements are designed/tested for linearity by the manufacturer. These systems are widely used in the lidar community, and we are not aware of any evidence that the analog channels are nonlinear.

Page 10, line 8: "become saturated". What do you mean with "saturated"?
What we are implying here is that the photon counting measurements that are above 10MHz are no longer linear. We have made a mistake in the paper by stating the saturation limit is 2 MHz. We will correct this to 10MHz.

Page 11, Fig 2: I suggest that you write "four signals". It is *two* channels.
Will change the text.

How did you determine "measurement noise"? With OEM? Please clarify.
The OEM does not determine measurement noise. However, we require measurement noise to evaluate the OEM. As given in page 7 line 5, for photon counting measurements which are linear, the measurement variance is equal to the square root of the photon counting measurement (Poisson statistics). For photon counting measurements that are non-linear and for analog measurements we use the auto-covariance method (refer Lenschow *et al.* (2000).) to estimate the measurement noise.

Page 14, line 2: What is coupling constant R_a? How is it defined?
R_a is the coupling constant for analog channels. It is estimated using Eq.14. This has been stated in the paper as " The coupling constants for analog (Ra) and digital (R) channels are estimated by fitting the ratio of PRR measurements with the ratio of the differential cross section (Eq.(14))."

Page 15, Fig 6: How did you obtain the *a priori* overlap? Why is this important since the ratio of the overlap functions cancels?

The *a priori* overlap function used in the OEM scheme is estimated using the OEM retrievals of temperature during clear sky conditions. A few clear day and nighttime measurements were processed in the OEM using a model overlap function given in Dinoev, T., *et al*. (2013) and corresponding overlap functions were retrieved. Then the average of the retrieved overlap functions from the measurements obtained at clear conditions were used as the *a priori* overlap function in the current PRR temperature OEM scheme.

The temperature retrieval is not sensitive to the choice of the *a priori* overlap function. Both, a sensitivity analysis and the retrieval diagnostics (the overlap averaging kernels) (see below) confirm this. But the overlap function is still important since the lidar signal for each channel is modelled explicitly, as opposed to the traditional method, which works with the signal ratios. Analogously, in the traditional method, the overlap could be determined using the retrieved temperature profile, a forward model and an extinction profile from an ancillary source.

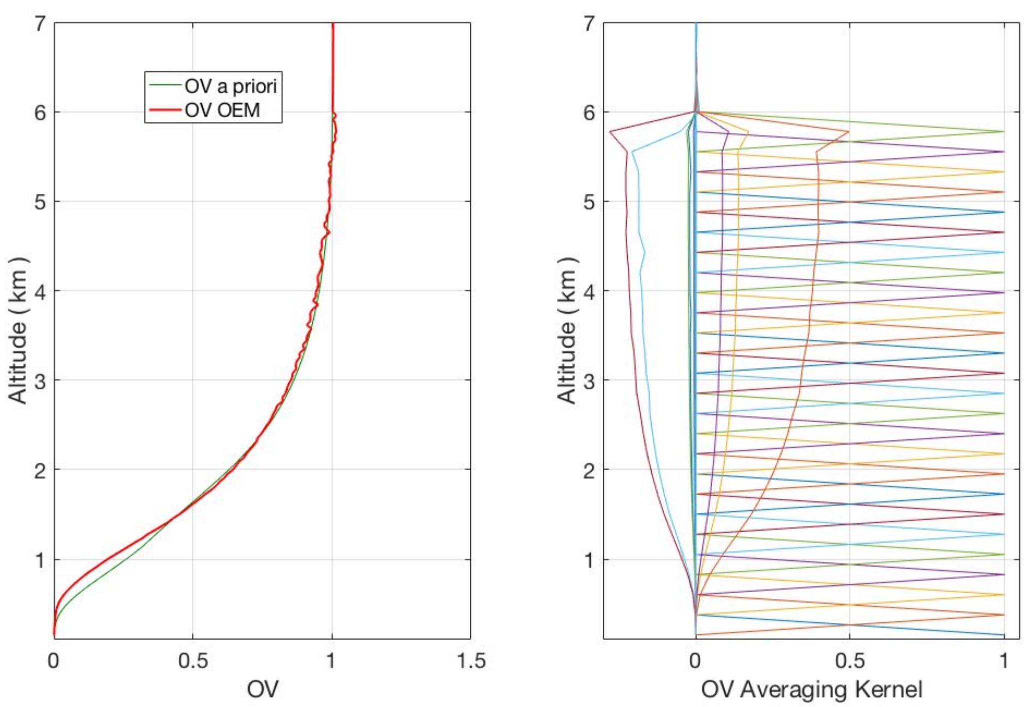

Figure: Left Panel : The OEM retrieved overlap (red curve) and the a priori overlap (green curve) from the clear nighttime RALMO measurements made on 20110909. Right Panel: The overlap averaging kernel.

Figs 1, 7, 12, 17: I would prefer the same scales. JLa -> JL, JHa -> JH? Should be consistent. I think it would be interesting to show the elastic signals for all cases. Why "Eb" for "elastic"?

The reason we are not showing the analog signals in the same height range as digital is that as in altitudes above 15 km analog signals go to background. Thus, the variations in the lower level analog signals are not clearly visible with the extended altitude axis.

We have only showed the elastic signal for Case study 4 as it is a special case. However, we will attach the profiles of elastic signals for the other 3 cases studies as a supplementary document.

Eb is a term that is used in the RALMO system, that refers to the **e**lastic backscatter detected by the **b**ig telescope.

The language still needs polishing/corrections. Here two examples:
Page 1, line 3ff: "assumption for the form of . . ." However, I think the form of the calibration function is not really the point here. Calibration with external sensors (with all the uncertainties related to the accuracy of the reference sensor and to the sampling differences) is usually needed but overcome with OEM.

Thank you. We will go over the text again carefully before submitting. We have fixed the 2 unclear sentences you have mentioned above.

References:

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12. Tao, Zongming, et al. "Measurements of cirrus cloud backscatter color ratio with a two-wavelength lidar." *Applied optics* 47.10 (2008): 1478-1485.
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