

Interactive comment on “A permanent raman lidar station in the Amazon: description, characterization and first results” by H. M. J. Barbosa et al.

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Dear anonymous referee #1,

We are glad that you liked the manuscript and find it scientifically relevant. Thank you for carefully reading the text. We have addressed all points raised, which significantly helped improving the manuscript. Below we give specific answers to each of your comments.

I was missing information on the measured water vapor. The authors describe the capability of the lidar to measure water vapor and also state

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that the intensive measurement campaign performed in 2011 was made to validate the water vapor measurements. But no results at all are shown. Why? Is this feature of the lidar not working or is there a publication only dealing with this atmospheric quantity?

The methodology used for water vapor retrieval, comparison with other instruments and results from our measurements will be discussed in an a publication we are currently working on. The last paragraph of the introduction was modified to make this point clear.

Fig. 11: The upper panel shows clearly some intensity steps of the received signal. This may be due to filter changes etc. However, these steps should not appear in the particle backscatter coefficient (middle panel) but they do. Can you state on this? Obviously the calibration is not independent of this intensity changes but it should be as it is independently calibrated in an assumed Rayleigh atmosphere. The author should check there algorithm or discuss these steps. For example “sudden jumps” in the backscatter can be found at after the first vertical white line or above the “3” of 08/31. There are much more examples, thus possibly the temporal resolution may be too high and noise could influence the calibration.

The variation of the intensity is due to small temperature changes inside the cabinet, as the air conditioning goes on and off trying to keep a constant temperature of 28 degC. This has a cycle of 6min. Text around line 15-20 of page 15 (section 4.1) was modified to clearly state this. These variations in the signal can be seen very clearly, for example, between 0 am and 10 am on 31/Aug. Please see the figure attached to this reply that zooms into the period around midnight of that day. These fluctuations in the laser power, however, are not seen in the particle backscatter coefficient, which is shown in the lower panel.

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The second figure attached to this reply shows the signal at 4.5km (in blue) in a 2hs zoom. Here it is easier to identify the 6-min cycle, particularly looking at the 300m vertical average (in red). The lower panel shows the particle backscatter at the same levels (dashed lines) and there is no correlation with the 6-min cycle. As expected, for an aerosol free region, the backscatter fluctuates randomly around zero. In the same panel, it is also shown the backscatter at 3km. In this case, the backscatter is around 0.75 Mm^{-1} and there is no correlation between the variations here and the 6-min cycle. Therefore we conclude that our algorithm is correctly calibrated independently of the laser power.

Note: the thin white lines (e.g. around label "08/31" in original Fig.11) were an artifact of the plotting routine. A different approach is now used so these white "missing" lines are not seen anymore.

At least the authors should discuss and estimate the errors of the backscatter profiles in these plots.

The Klett algorithm involves ratios of integrals and it is not simple to propagate the uncertainty in such situation. If the referee can provide a reference that shows a methodology for that, we will be glad to include it in our manuscript.

Beside of that the center and lower panel show exactly the same except for the scale. Therefore I suggest to combine these panels and give two scales, one for extinction and one for backscatter. This also that some quick reader may interpret the extinction values as an independent measurement.

You are correct: the scaling factor is the lidar ratio. We have removed the extinction plot in this case.

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Figure 12: The shown extinction and lidar ratio panel is in the current state not ready for publication. There is clearly a problem in the height region below 1 km due to overlap fluctuations. This can be seen in rather low extinction and even better in unrealistically low lidar ratio values. The author should really quality check these 2 panels and leave out regions at which the measurements are not trustworthy. Beside of that there are also sudden jumps in the lidar ratio, e.g. on 09/03, which seem to be not from atmospheric variability. What happened there? Possibly as suggested above also a temporal averaging would be useful to avoid too much noise (probably half hour means would be enough). Also errors of the final products should be discussed and at least estimated.

This point is under investigation and we will post a new comment in the online discussion specifically about this.

Correlation of aerosol depth/ Fig. 13: I do not see the reason for correlation 4435 nighttime profiles of Klett and Raman backscatter profiles as they are highly dependent on each other. This correlation is no proof that there is a good agreement and the statistical relevance is not given as the profiles are not independent.

The correlation shown in figure 13 is not of the backscatter but of the vertically integrated particle extinction (aerosol optical depth). For the Raman analysis, the extinction is calculated independently of the backscatter, while for the elastic analysis it is not.

Therefore, what this scatter plot shows is that the fixed lidar ratio used in the Klett analysis is in good agreement with the average lidar ratio computed independently by the Raman analysis. Moreover, it also shows that true (and variable) lidar ratio during that week did not vary too much from the mean value. The good correlation also shows that there are no large systematic errors in both analysis routines.

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Finally, the ultimate comparison is against Aeronet (Fig 16). This, however, is only available during day time. Therefore, we need Fig 13 so that comparing Aeronet AOD with Klett AOD during daytime makes sense.

In my opinion it would be to use profiles (for example also averaged) from different scenarios (conditions) which are really independent from each other. This could be done even from the one week of measurements, but then much less points would appear.

It is not clear what the referee meant. If we used different periods for the Raman and Klett analysis, how would it be possible to compare them?

Why are not all formulas numbered?

This was a mistake. Now all formulas are numbered.

Abstract: Line11/12: I would avoid using the root mean error in the abstract when talking about a comparison. Without further explanations it is not easy to understand this issue only from the abstract. Therefore I would recommend either to speak about a linear correlation (instead of comparison) or just leave out this value as 0.06 is anyway not small compared to 0.02.

Agreed. The text of the introduction was modified to include the correlation and remove the RMS.

774/line 10: You state that the MPI Hamburg added instrumentation including lidar. What kind of lidar was provided, since when it is measuring and where is it located? Is a comparison to your instrument possible/planned?

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What MPI-H provided was a micro rain radar (MRR by Metek) and a ceilometer (CHM15k by Jenoptics). The paragraph was rewritten to make this point clear.

774/line 28: what beam is parallel? The laser beam or the incoming light? What do you mean? Please write more exactly!

The light collected by the telescope goes through the iris (field stop) and is then collimated by a pair of lenses before it finds the dichroic beam splitters. We modified the paragraph to make it more clear.

775/19: It is not clearly what is meant with this sentence: "For the AN data this is between 5 times its resolution and half its scale, and for the PC data this is below 15MHz" Could you please rephrase and write more extensively what is meant and for what it is needed.

We use Analog-to-digital (ADC) converters that have 12-bits. Therefore, there is an intrinsic resolution for each selected scale. For example, using the 500mV scale the values read by the ADC appear in steps of $500\text{mV}/4095 = 0.122\text{mV}$. This is the resolution explained in the previous paragraph of the manuscript. Voltages readings up to 5 times this value ($5 \cdot 0.122\text{mV} = 0.61\text{mV}$) cannot be used. Moreover, voltages reading above 50% of the ADC scale are not linear with the amount of detected light.

For the photoncount mode, after the PMT detects one photon there is a characteristic dead time during which it cannot detect another photon. Therefore, the values in the PC cannot be used without correction if the rate of arrival of photons is too large (increased probability of photons arriving in the dead time window).

Therefore, for performing a fitting between the AN and PC channels we have to select a range of altitudes for which both are good. The paragraph mentioned here and the one before were modified to make this clearer.

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775/23: I understand that the time delay has to be an integer, but you should also test, what happens if you use 9 bins delay instead of 10. Especially for Raman extinction retrievals in the near field 1 bin difference could make the difference. This comment is just for you to have in mind, no need to change in the manuscript.

Because our system has a very high full overlap (1km aprox), the near field cannot be trusted anyway. We will, however, do the testing as suggested. Thank you.

776: Please describe all variable in the formula for the Residual J. I.e. “sigma” and “n” is not described.

We have rewritten the equation to make it clearer:

$$J(t, \tau) = \frac{1}{n} \sum_{i=1}^n \left(\frac{C(z_i, t, \tau) - \hat{C}(z_i, t)}{\sigma_i} \right)^2 \quad (1)$$

σ_i is the standard deviation of C , which is the corrected PC signal. We calculate σ_i as the square root of C (since we verified it has indeed a Poisson distribution). The summation $\sum_n$ is over the points used in the linear fit for the determination of $\hat{C}$.

If instead of σ_i we used the square root of the total variance (i.e., combining σ_i and $\hat{\sigma}_i$) and used the number of degrees of freedom instead of “n”, then J would just be the reduced χ^2 . As we just want to minimize J this is not important.

However, it must be noted the J is indeed very close to the χ_{red}^2 since σ_i is much larger than $\hat{\sigma}_i$ and $ndf \sim n$. Therefore, as the variance is slightly underestimated the optimum values of J are slightly larger than 1.

776/19: In my opinion it would be worthwhile to write a short introduction sentence for this chapter. Why do you make the electronic noise test C400

and what do you expect (i.e. cite a paper where problems with the analog detection are published).

776/19 ff.: In my opinion Figure 5 and 6 are not really necessary. You write 5 lines for 2 Figures which show nothing than the expected. So it could be better to put these figures in to the supplement material or simply leave out, and just write 1-2 sentences more in the paragraph. Especially Figure 6 is not needed as usually no dark effect for photo counting systems are expected. If you decide to still show figure 5, I would recommend to also plot the curves with an moving average of 15 bins or so. Just now, because of noise it is impossible to “see” something between the different channels, especially if there are some minor oscillations. Nevertheless, it is very good that you have performed all these tests and that the results are so positive, thus the text should definitely stay in the manuscript.

776/23: I would shift this paragraph to the end of the section as the topic change is very immediately and this paragraph has nothing to do with “signals”

This reply is for the three comments above.

All agreed. We removed the figures 5 and 6 and added some extra sentences about the noise evaluation. Paragraphs were rearranged as suggested.

777/25: As you describe the molecular procedure intensively it would be also good to state the values you use for “N” and “sigma”.

Done. Molecular density in a standard atmosphere is $2.5469 \times 10^{25} \text{ m}^{-3}$ and σ_m^{std} for 355 and 387 nm is 2.7589 and $1.9211 \times 10^{-30} \text{ m}^2$ respectively.

778/12: Again, please state which value you use for the depolarization factor.

Done. For 355 nm and 387 nm it is 0.0306 and 0.0299.

778/eq. 4: Is the notation really correct? When I insert eq. 3 into eq. 4, I have $K\epsilon^2$. I guess K has to be left out in one of the equations.

Indeed, the K in eq.3 was not correct.

781/17: overlap function not factor as it is not constant. Please state which method describe by Wandinger and Ansmann you have used and which lidar ratio was applied and why.

We used the iterative approach, with a LR of 55sr, and this is now clarified in the text.

783/3-5: What is done with the apparent cloud top? I hope it is not used for the statistics and analysis. I cases of thick clouds, simply no cloud top should be determined.

The apparent cloud top is not taken in to account for the statistics and analysis. These values are flagged in the algorithm and stored for future analysis. We added a sentence to make it clearer.

783/14: I do not understand step 4. Can you explain more detailed what is compared and for what are you looking for?

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In this step we compute the difference between the raw signal at maximum and the raw signal at the previous minimum. This gives an idea of the peak signal amplitude. To define if it is significant or not, we compare that with the difference between the raw and filtered (obtained in step 2) signals at the maximum. If the maximum is a real maximum, then the 3-point average will not reduce it too much. Therefore, when we divide the first value by the second (like a signal to noise ratio) we will have a large value for a real peak, and a very low value for a noise peak.

The text for all the steps were modified to make this more clear.

784: line 2: What happens, if thick aerosol layers are present (e.g. from biomass burning), are they classified as cirrus? As the background particle signal between 19 and 20 km should be very low, this could easily happen - please comment/discuss this.

For the final determination of the clouds base and top, we compare the particle backscatter at the cirrus altitude with the particle backscatter between 19-20km. To consider the layer as a cloud, the value inside the layer must be greater than the reference value + 2 standard deviations of the reference value (mean between 19-20km). Therefore, if the elevated aerosol layer need to be really thick to be miss classified as a cloud layer. It is, however, still possible.

The best way to differentiate an aerosol layer at high altitudes from cirrus clouds would be to look at the depolarization ratio, of course. Our system, unfortunately, does not have such a channel. However, it must be noted that H. Baars [Phd thesis iFT-Leipzig, 2011] operated a system with depolarization channel in the Amazon for almost 1 year and he did not reported aerosols at these altitudes. All aerosols that he found were trapped below about 6km.

Therefore what the referee mentioned do not affect our measurements too much. Nonetheless, we are currently working on an improved version of the algorithm to sep-

C403

arate those based on the signal shape and characteristics. This will be published within an article about cirrus clouds that we are currently working on.

784/14: Why have you used a lidar ratio of 55 sr. I would expect a higher lidar ratio in the UV for BBA and Baars, 2012, also reported typically higher values as you also do in Fig. 12. Can you comment on that?

Baars (2012) reports an average lidar ratio of 62 ± 12 sr during the dry season and the authors used 60sr for obtaining the extinction from the backscatter profile. However, the authors also report lower LR for fresh BBA than for aged BBA. As the fire counts and back trajectories indicated possible sources very close by, we used a slightly lower value. It should be noted, however, that not much difference should be expected from changing 55 to 60 sr, otherwise there would not be such a good agreement between the Raman and Elastic AOD.

787: back trajectories and fire counts are very interesting but give only the first hint if there could be BBA. For future publications also modeling results should be taken into account to proof/compare the findings.

Thank you for the suggestion.

787/25: Do cirrus clouds really appear up to 20 km? These would be in contradiction to your statement before that between 19 and 20 km no particle backscattering is expected... If you have measured cirrus at 20 km it would be very interesting to show this case.

We have put the general interval of the occurrence of cirrus clouds instead of restricting it to the week analyzed in this paper. This is now corrected to 8 to 16.5km.

C404

We have, indeed, observed cirrus clouds with top altitudes up to 19.8km. From 2 years of observations (50

788/3: "There is a good agreement between these geometrical properties...": I do not understand" what agrees", could you rephrase this sentence and write more explicitly?

Fig. 18 shows the log of the range and background corrected signal where one can directly see the position of the cirrus clouds. The same figure shows the cloud base (black +), cloud top (magenta circle). What we meant is that algorithm base/top altitude makes the contour around the clouds perfectly (at least by visual inspection).

The paragraph was modified to make it clearer.

788: How did you define the tropopause from the radio sonde? Please write down!

It is defined as a thermal tropopause using the radiosonde data. In the interval from 500 hPa to 30 hPa, it is defined as the lowest level at which the lapse rate decreases to 2 degC/km or less, provided that the average lapse rate between this level and all higher levels within 2 km does not exceed 2 degC/km.

We added to the text the term "thermal tropopause".

789/line 18: The conclusion is too strong because there have been approaches over a longer period: : please rephrase.

Acknowledged and modified.

C405

790/18: Raman instead of raman

Changed all around the text.

790/28: Please rephrase sentence, because also the trajectories from the Ocean cross the continent for more than 1 day and thus no marine influence is expected.

It is not clear what the referee means.

At 790/28 we said that we expected to observe BBA particles and not Marine aerosol. The reason is that 1) sea salt is washed out much more easily than BBA; 2) sea salt would not produce the high AOD values as we observed; 3) sea salt does not have a high LR as we observed.

We explained this in the text.

799/Table1: #cirrus clouds detected is a misleading statement. Did you really detect 993 single cirrus clouds during you 1 week of measurements? I guess not. I guess this must be the number of profiles for which cirrus was detected.

Indeed, we meant the number of profiles for which we detected a cirrus clouds.

801/Caption Fig. 2: What is J and tau? Please also write down in words for an easy reading.

J is the residual of the fitting between the corrected PC, which depends on the dead-time tau, and the true count rate. The figure caption was modified to say that.

C406

There are 3 figures concerning the dead time! I think 2 should be enough...

Indeed. Now Fig.2 and Fig.3 are merged into one, i.e., showing the parabolic fit and the histogram only for channel 1.

Figure 5: Averaging needed Fig. 6: Could possibly left out.

As discussed above, both picture were left out.

Fig. 7/Caption: relative AIR density?

Correct, it is air density. Caption changed accordingly.

Fig. 8: what is what? Left side after and right side before? Please write more clearly!

It was indeed confusing. We now refer to the narrow and to the wide field stop periods, which is what is indicated inside the plots.

Fig. 9 and 10: Both Figures show almost the same: therefore I would recommend to show only one of it!

Fig. 9 shows the actual correction necessary due to the overlap (the average correction, in green), while Fig. 10 shows the standard deviation of these corrections (note the different order of magnitude).

C407

Fig. 12: I do not like the color scale. For the interesting regions it is impossible to see a difference between all the blueish colors. Could you try to change the color code or the scaling (more green for example)? Or discrete coloring?

We changed the limits in the color bar to better distinguish the important values. To really look into the details, as the referee wants however, it would be necessary to zoom into a particular day instead of looking at 7 days in a single plot.

Fig. 14: Could you discuss the low Terra value?

The Aqua and Terra values in this plot were used just for completeness. As they only make one image during daytime each, there is not enough statistics to say anything about this discrepancy (Terra has 4 points and Aqua 2 points). A real comparison with satellite products would need a much longer period of time and is out of the scope of the current manuscript.

Fig. 16: I again think that the linear correlation is biased as the single values are not independent from each other. As suggested above it would be useful to lower the number of points to different scenarios or time periods or weight the data points so that the linear correlation is more meaningful.

As explained above, this figure is necessary because it shows that we can use Klett during daytime to compare with Aeronet.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 769, 2014.

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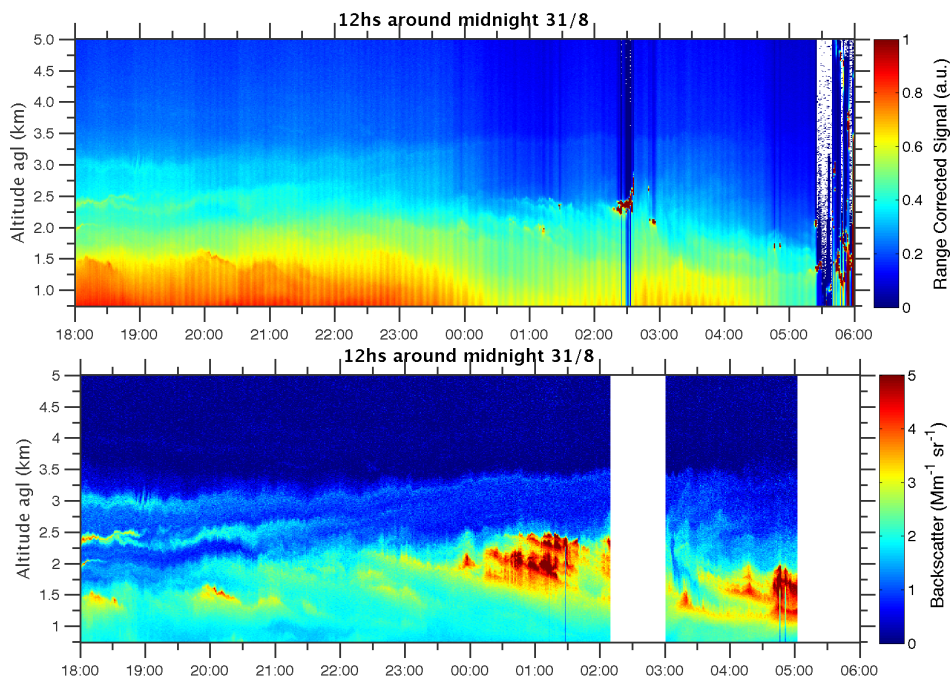


Fig. 1. TOP: Range corrected signal from 18hs 30/aug until 6hs 31/aug. The variations in the signal strength are clear. BOTTOM: Backscatter obtained with the Klett algorithm. No variations here.

C409

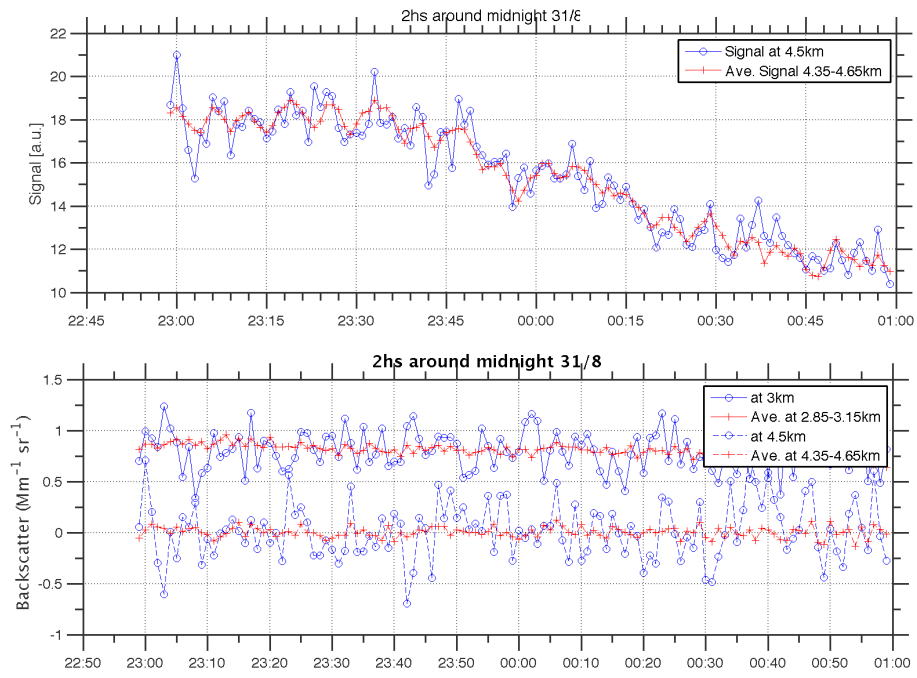


Fig. 2. TOP: Signal at 4.5km (blue) and average signal between 4.35-4.65km (red) are shown from 23:00 30/aug until 1:00 31/aug. BOTTOM: same for backscatter obtained with the Klett algorithm.