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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #1

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The paper describes the characteristics of the first permanent lidar station in Amazonia and shows first example results of it. It is a good basis for further publications dealing with observations from this lidar station and thus is of scientific relevance as it is a key region on for atmospheric research. The publication is well written and the figures and tables are of high quality.

The paper contains a lot of technical information as the authors have done a very deep instrument analysis before publishing first results. Such an analysis is very worthily and shows the seriousness of the work done. For example, the effort to compare the operational radio soundings and the ones at the lidar site is very useful and gives a

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good reference for many future activities.

However, since the atmospheric part is quite short I would leave out some graphics which are not really needed and only describe the outcome in the text and/or put these figures to supplement material. This would make the paper more “handy” and increase its value (in my opinion).

Major 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 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?

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

Figure 12: The shown extinction and lidar ratio panel is in the current state no ready for publication. There is clearly a problem in the height region below 1 km due to overlap

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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 jump 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.

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. 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.

Beside of that I have some specific comments, mostly very minor and of technical nature, but some points are science-relevant and need some intensive clarification and statement:

Why are not all formulas 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.

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

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.

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.

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

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 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 ore 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 write1-2 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

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very immediately and this paragraph has nothing to do with “signals”

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

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

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

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.

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.

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

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.

784/14: Why have you used a lidar ratio of 55 sr. I would expect a higher liar 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?

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.

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

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expected. . .. If you have measured cirrus at 20 km it would be very interesting to show this case.

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?

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

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

790/18: Raman instead of raman

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.

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.

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

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

Figure 5: Averaging needed

Fig. 6: Could possibly left out.

Fig. 7/Caption: relative AIR density?

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

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

Fig. 12: I do not like the color scale. For the interesting “regions” it is impossible to see

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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?

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

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.

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