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Interactive comment on “One year of CNR-IMAA multi-wavelength Raman lidar measurements in correspondence of CALIPSO overpass: Level 1 products comparison” by L. Mona et al.

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Received and published: 26 May 2009

On behalf of all authors, I would like to thank the referee 1 for the comments and suggestions, that contribute to improve the quality of our paper. In the following, the referee comments are repeated first (in italic type) and we reply to the respective statements.

» *General Comments* This is a nice study of comparing lidar attenuated backscatter profiles measured by the CALIOP sensor on the CALIPSO satellite and a ground based Raman lidar located at Potenza, Italy. The paper describes the techniques used to derive attenuated backscatter profiles from the ground based Raman lidar and how these are used to compare with similar attenuated backscatter profiles from CALIOP.

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These comparisons are made using data acquired at night. This paper does represent a useful contribution to the area of evaluating space based lidar measurements of attenuated aerosol backscatter. The methods for the analyses of the Raman lidar and CALIOP data are described well. However, although I could understand what the authors did, the language and use of English in the paper could use substantial improvement. I started to make changes to the English usage but quickly realized that this beyond the work expected for a reviewer. The authors need to have this paper revised by someone more familiar with English usage and who can make the necessary changes. I would recommend publication after the authors have addressed the changes below. Most of these are essentially minor changes that will clarify the procedures and results presented in the paper. I would particularly stress items 5, 18, 19, 20 and 21.

Following the suggestion of the referee, the paper will be revised by someone more familiar with English usage for the final version.

» Specific comments 1. The title could be improved. Suggest changing “in correspondence” to “coincident”. Also the title indicates multi-wavelength Raman lidar; the analyses presented in the paper only make use of the 532 nm wavelength, so I don’t see the need to emphasize the multiwavelength nature of these measurements.

In agreement also with referee 1 suggestions, we change the title in: One year of CNR-IMAA multi-wavelength Raman lidar measurements in coincidence with CALIPSO overpasses: Level 1 products comparison. Regarding the word “multi-wavelength”, this is a fundamental capability of our instrument. In the current paper, we use our measurements of backscatter and extinction at 532 nm, but for next study concerning level 2 data also measurements at 355 and 1064 nm will be used. We decide to keep the word multi-wavelength in the title for a sort of continuity with the next to come papers

» 2. (abstract, line 1) change “is operative” to “...has operated. . .”

OK.

» 3. (abstract, line 6) “Dedicated measurements have been performed...”

OK.

» 4. (abstract, last line) I don’t understand the last part of the last line. This seems to say that the differences are larger when passes at 80 km away are used, but I can’t tell what this last part of the line means.

We re-write the sentence : Finally, the mean differences are on average lower at all altitude ranges for the closest overpasses (at about 40 km) respect to the 80 km overpasses.

» 5. (abstract) The abstract should indicate that these are nighttime measurements. (abstract). The abstract should not give the impression that the CALIPSO measurements underestimate the attenuated aerosol backscatter. See item 20.

In the final version the following sentence has been added: “Night-time cases are considered in order to take advantage from Raman capability of the ground based lidar.” It was not our intention to give the impression that our main result is that CALIPSO observations are biased. For this reason, thanks to the referee’s comment, in the final version of the paper, the words over-under estimation and bias are avoided.

» 6. (page 3, middle of the page) change to “...ground-based elastic/Raman lidar and/or High Spectral Resolution Lidar (HSRL) measurements are necessary, since these techniques allow the characterization of atmospheric aerosols”. Suggest adding the following reference Hair, J. W., C. A. Hostetler, A. L. Cook, D. B. Harper, R. A. Ferrare, T. L. Mack, W. Welch, L. R., Izquierdo, F. E. Hovis, 2008: Airborne High Spectral Resolution Lidar for Profiling Aerosol Optical Properties, Applied Optics, 47, doi: 10.1364/AO.47.006734.

OK

» 7. (page 6, first paragraph) While the statements in this paragraph are true, these analyses are not discussed elsewhere in this paper and so are not relevant to this

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discussion.

In section 3.1 we present the measurement strategy. Even if in this paper only aerosol attenuated backscatter profiles measured by PEARL at 532 nm are used, the measurement strategy was thought taking into account the multi-wavelength capability of our lidar system. In addition, a future paper concerning the comparison of level 2 data in terms of aerosol optical properties at both 532 and 1064 nm is planned. For these reasons, we consider important to underline here the multi-wavelength capability of our system.

» 8. (page 6) This should indicate that these measurements were made at night.

Yes. In the results section it is reported that nighttime cases are selected for the analysis. However, following the referee suggestion, in the abstract of the final version, it is reported that results are based on nighttime measurements.

» 9. (page 9, middle of the page) should read “The vertical resolution of this modeled radiosounding is obviously higher and temperature gradients. . .”

OK

» 10. (page 10, top paragraph) The mean difference looks to be closer to -1% than 1%; should the 1% in line four be -1%?

Yes, it is -1%.

» 11. (Figure 4b). The MODIS image doesn't really show the dust well. I don't see the need to include figure 4 in this paper. The case for Saharan dust would probably be more convincing if aerosol or total depolarization profiles are shown.

Unfortunately no depolarization measurements are available from ground-based measurements for the shown case. As additional material about the Saharn dust case, vertical profiles of lidar ratio and Angstrom exponent are added and discussed in the final version of the paper. The MODIS image instead is removed as suggested by the

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referee.

» 12. (Figure 5) It may be good to indicate that the sharp spike at the bottom of the CALIPSO profile is due to the ground return.

A comment about this point is reported in the final version of the paper.

» 13. (Page 8) There should be more discussion regarding how the average transmission computed from the PEARL system is applied to the CALIPSO data.

The fundamental point of our study is that CALIPSO data are not modified at all and that from PEARL data we determine a quantity comparable to the CALIPSO attenuated backscatter. The latter is called CLAB, i.e. CALIPSO like attenuated backscatter. The procedure to calculate it is reported into detail in Section 3.2 .

» 14. (Page 11, second paragraph) Do the depolarization measurements of PEARL (and CALIPSO?) show large values characteristic of Saharan dust?

As reported above, no PEARL depolarization measurements are available for the reported Saharan dust case. Aerosol vertical profiles as measured by PEARL are reported in the final version of the paper. In particular lidar ratio and Angstrom exponent values are discussed. Comments about the CALIPSO classification of the observed layer are added too.

» 15. (page 12, line 7 from bottom) should be "... with a complex topography ..."

OK

» 16. (page 12, line 3 from bottom) What is the basis of the statement that specular reflection from the ground impacts the CALIPSO low altitude measurements? This sentence should be omitted unless some reference(s) or other measurements that support this claim are provided.

We mean that it has to be taken into account that the surface return in the CALIPSO 0.3 degree off nadir configuration can influence these low altitudes measurements because

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of its strong intensity. The text in the final version is modified accordingly.

» 17. (page 15, line 1) should be "... up to 5 km ..."

OK

» 18. (page 16, line 10) Here and elsewhere, (such as line 3 from bottom on page 1) there are lines that say there is a "bias" in CALIPSO measurements or CALIPSO "underestimates" which lead one to believe that the CALIPSO measurements are a problem. However, there is little if any strong indication that there are problems in the CALIPSO measurements to support these assertions. Moreover, the authors have not conclusively demonstrated that some of these differences are not due to the PEARL measurements. The authors indicate that the PEARL statistical error is lower than 10%, but they have not addressed what the systematic error sources may be in the PEARL data. Do the statistical errors correspond only to uncertainties in photon counting statistics? How large are the uncertainties associated with computing transmission from the PEARL data and using this transmission to derive an attenuated backscatter from the PEARL data? Also, there should be additional discussion of what the expected uncertainty is in the CALIPSO attenuated backscatter and whether the differences between the CALIPSO and PEARL measurements fall within these expected uncertainties. Therefore, the authors should replace the words suggesting bias and underestimates and replace with the word differences.

It was not our intention to give the impression that our main result is that CALIPSO observations are biased. For this reason, thanks to the referee's comment, in the final version of the paper, the words over- and under-estimation and bias are avoided; the text in order to avoid the impression that there are some biases in CALIPSO data. Vertical profiles determined by PEARL have both statistical and systematic errors. Statistical errors are those resulting from photon counting statistics. Systematic errors on aerosol backscatter with elastic Raman techniques are almost negligible, and however lower than 1%. Further details are reported in Mona et al. 2006 and related references [Ans-

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mann et al, 1992; Ferrare et al., 1998]. Regarding the transmittance, the error resulting from extinction coefficient error is negligible. Systematic error on transmittance due to molecular and ozone terms are discussed in the text (section 3.2) : on average lower than 1% and 0.5% respectively. Therefore the statistical error on the CLAB is lower than 10% and the systematic error lower than 3%. Unfortunately at the moment errors on the CALIPSO attenuated backscatter are not available, therefore a complete discussion comparing observed differences and errors affecting data of both instruments is not possible. However, in the final version of the text, a sentence about the total error on CLAB is added at the end of Section 3.2 is added.

» 19. (page 16, last paragraph) Here again the authors should remove the lines suggesting surface specular reflection causing problems in the CALIPSO returns near the surface unless additional information can be provided to support this claim.

See point 16.

» 20. (page 18, last paragraph) Again, this paragraph refers to an underestimation of CALIPSO measurements when referring to cirrus cloud measurements. Again, this should refer to differences, not underestimation. Also, it should be stressed that it is not possible to assess attenuated aerosol backscatter profiles given the very low number of cases (which should be indicated here).

As reported above, in the whole text of the final version, bias, over and underestimates are substituted with differences in order to avoid to give the impression that we found errors in CALIPSO data. Conclusions section is completely re-written in the final version of the paper. A comment about the low number of data is added.

» 21. (page 19, first full paragraph) Same issue here. The authors give explain the differences between CALIPSO and PEARL in the lowest altitudes at or above the PBL are likely due to horizontal variations in aerosols, which is a very plausible. Therefore, here and in the abstract, the authors should not give the impression that the differences are due to some problem with the CALIPSO measurements. Note that it is not necessarily

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true that the differences between CALIPSO and PEARL will be smaller for when the horizontal distance between the measurements is smallest, if local sources of aerosols (e.g. pollution) create large horizontal variations.

In the final version of the paper, conclusion section is completely re-written. In particular, it reports only results and how we reach them, so other explanations discussed in the body of the paper have been removed.

» 22. (Figure 2) It is difficult to distinguish the blue and black lines; I suggest making the lines darker.

Lines are now thicker.

» 23. (Figure 6) I suggest to display the CALIPSO data between 0-13 km similar to that shown for PEARL; this will make it easier to compare the two measurements.

Figure 6 is modified in the final version of the paper according to the referee suggestion.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8429, 2009.

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