

## ***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.***

### **Anonymous Referee #2**

Received and published: 16 April 2009

#### 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. 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

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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.

#### 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.

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

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

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.

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.

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 are 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 Spec-

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tral Resolution Lidar for Profiling Aerosol Optical Properties, Applied Optics, 47,doi: 10.1364/AO.47.006734.

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 discussion.

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

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

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

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.

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

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

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

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.

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

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.

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.

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).

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

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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 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.

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

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.

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

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