

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 #1

Received and published: 14 April 2009

GENERAL COMMENTS.

This work by Mona et al. is relevant, important and timely. It helps address an important issue in the determination of the effects of aerosols on the global radiation budget by providing an assessment of the accuracy of data from the CALIPSO (Cloud-Aerosol Lidar Infrared Pathfinder Satellite Observations) mission.

Because the uncertainties in the effects of aerosols on the global radiation budget impose limitations on the performance of Global Climate Models, global measurements of aerosol profiles are needed. To ensure coverage over as much of the planet as possible, especially over the oceans, measurements from satellites are required.

C194

CALIPSO is the first satellite lidar mission dedicated to the long-term, global measurement of profiles of aerosol properties. However, being a simple, elastic-backscatter lidar, CALIPSO's primary measurement is of attenuated backscatter profiles and assumptions relating the particulate backscatter and extinction coefficients are required in order to retrieve profiles of aerosol extinction. Except where CALIPSO can determine the layer transmittance directly, these retrievals use model/climatological values of the lidar ratio (the ratio of extinction to backscatter). Although CALIPSO's algorithms (see authors' Vaughan et al. 2005 reference) use measurements of the depolarization ratio and color ratio to infer aerosol type and, hence, lidar ratio, this ratio is known to vary even for a given aerosol type. Therefore, independent validations of the CALIPSO data products are essential. CALIPSO's initial L2 data releases (e.g. of layer and extinction products) *will* be wrong in some cases, because of the necessity of using model lidar ratios for a rather limited number of aerosol types. This, presumably, is one reason why researchers have been asked to participate in the CALIPSO validation plan. (See Section 3.2.2 of the authors' Winker et al. 2004 reference.) Their results will allow the current models, values and algorithms to be tested, extended and improved.

As Mona et al. point out, the validation process has two stages. First of all the CALIPSO attenuated backscatter profiles (the Level 1 data products) need to be checked to ensure that they are not corrupted by instrumental or other effects. The second stage involves an assessment of the representativeness of the lidar ratio used by the CALIPSO analysis algorithms in the retrieval of the Level 2 data products, an assessment to which Raman lidars (and High Spectral Resolution Lidars) are ideally suited. This current work addresses the first stage. The authors state that the second stage will be addressed in a subsequent paper.

The authors provide a clear description of their methodology in comparing the CALIPSO profiles with their ground-based profiles and take care to separate cases where cirrus clouds, which can complicate the comparison, were present from those where they were not. Their analyses appear to be careful and correct, although several

C195

points need clarification.

My main concerns relate to the continual descriptions throughout the article (Abstract, Results and Conclusions) of the CALIPSO lidar signals as being “overestimates” or “underestimates” or “biased”, when the authors, in several places, give well-argued and plausible explanations for the differences between the CALIPSO lidar and scaled PEARL signals in terms of different atmospheric conditions at the different locations sampled by the instruments. Having apparently reached this conclusion in the Results section, they then return in the Conclusions to use of these apparently unjustified descriptions. Given that the authors’ arguments in terms of different atmospheric profiles are detailed and convincing, whereas their explanations in terms of possible problems with the CALIPSO instrument are not always supported by the results they show, and other explanations discussed in the next section have not been excluded, the authors, at this stage, should describe the differences as just that, “differences”, or that the CALIPSO signal is larger than or smaller than the signal from the scaled PEARL signal in some height interval. The validation work that the authors are doing is important, but it is equally important that the conclusions they draw be well founded.

This and additional concerns are discussed in the next section (Specific Comments). However, once these concerns have been addressed satisfactorily, and the technical errors listed in the final section of this report corrected, I would have no hesitation in recommending that this valuable contribution be published.

SPECIFIC COMMENTS

Abstract, main body of article and Conclusions. The continual use of words like “underestimation”, “discrepancy” and “bias” to describe differences between CALIPSO and the ground-based data are unjustified, as explained above. The authors are comparing what are basically, apart from a calibration factor, raw, range-corrected, measured profiles from CALIPSO, with data from their system that have been processed using certain assumptions (Section 3.2). Given that other studies, like those with the NASA

C196

airborne Cloud Physics Lidar flying directly below CALIPSO (McGill, M. J., et al., 2007, *J. Geophys. Res.*, 112, D20201, doi:10.1029/2007JD008768) have shown agreement of the profile shapes, the authors need to consider carefully whether they can conclude that CALIPSO has systematic over- or underestimations in parts of its profiles or not. If the authors are proposing an instrumental effect with the CALIPSO lidar as the cause of the lower signal in the PBL, but not throughout the rest of the profile, then they must show how that effect would create the observed height-dependent differences in signals. Note that, given CALIPSO’s large distance from the surface, any transmitter-receiver misalignment errors would not cause signal differences just in the PBL. Similarly, they need to explain how such height-dependent behavior could be caused by multiple scattering (e.g. Page 8446 lines 2 – 23) or specular refraction (P8444 L25) if they consider these effects to be significant.

As the authors state more than once, the most likely cause is the sampling of different atmospheric volumes. E.g. Page 8446 lines 2 – 23. Indeed, Fig 8 clearly shows an elevated aerosol layer between ~ 2.5 and 4.5 km that is missing above the ground-based lidar, while there appears to be a stronger aerosol layer up to 2.5 km above the ground based lidar than is below the CALIPSO path. Also, later (e.g. on P8448 L18-25) they give good reasons to expect differences in the atmospheric profiles in the PBL. The PEARL site seems to be in an elevated mountain valley that can trap local pollution whereas the CALIPSO ground track is closer to the coast on lower and flatter terrain where there is either less pollution (or it is less likely to be trapped). Given this systematic difference, one should not expect that there should be similar signals in the PBL at both sites. The authors may just have to accept that, despite all their careful and dedicated efforts, their site is unsuitable for comparing with the CALIPSO signals in the PBL, at least in terms of assessing CALIPSO’s performance, and they may need to limit their studies to where the aerosol columns are likely to be similar when measured over enough samples. Interpretations of CALIPSO’s performance based on comparisons of the PBL signals may need to be done by colleagues at other sites closer to the CALIPSO ground track and with similar aerosol columns.

C197

Page 8431 line 25. CALIPSO was launched in late April 2006, but atmospheric profile data are only available from mid June that year.

Page 8432 line 26 – 28. Although the authors do not specifically say so here, this section, read in conjunction with section 5.1 could give readers the impression that multiple scattering and specular reflection (from oriented crystals in clouds) are errors in the lidar signal. It should be noted that this is not the case. However, analysis algorithms need to account for all the effects (i.e. a correct forward model is required). Some lidars (e.g. Multiple-Field-Of-View lidars and Wide FOV lidars) use multiple scattering to advantage in order to derive additional information on the cloud. Also, multiple scattering is measured by all lidars, including ground-based Raman systems (e.g. Wandinger, Appl. Opt., 37, 417-427, 1998) although it is more significant in space-based systems because of the larger distance from the clouds and the resulting larger footprint, despite the small fields of view used. Similarly, specular reflection is not an error and also provides additional information. (See comments on section 5.1 below.)

Page 8433 lines 3 – 5 are not clear. Why are “misleading assumptions needed”? Are the authors referring to potential errors introduced by assuming model or climatological lidar ratios that are range-independent within layers? If so, the sentence might be written more clearly as something like “By, first of all, comparing our ground-based measurements with the CALIPSO Level 1 data products, we can distinguish any potential problems and biases already contained in the calibrated CALIPSO lidar signals from any errors and uncertainties that might result from any invalid assumptions or approximations used in the optical properties retrieval algorithms.”

Page 8435 lines 8 – 13. “With these resolutions, in night time conditions, ...” Do these quoted statistical errors / uncertainties represent the calculated (expected) errors that result from the signal counting or digitizing statistics, or are they based on the standard deviations in typical measured profiles? The reader needs to understand whether these errors include variations in the atmospheric aerosol column over the measurement period or not. Does the sentence need to be modified to include the

C198

words “and assuming that the atmosphere and aerosol profile remain constant” after the “night time conditions”?

Page 8438 line 6 Eq. (3). Note that the CALIPSO ATBDs (e.g. the authors’ Vaughan et al., 2005 reference) define particulate transmittance differently. In those documents it includes a multiple scattering factor because, as the current authors point out, multiple scattering can be a significant issue in space-lidar signals. The authors should explain how this is included in their calculations or justify why it is not used here. (See authors’ Winker 2003 SPIE reference.)

Section 3.2 Attenuated backscatter comparison (Pages 8437-8) The authors’ method of calculating their CALIPSO-like attenuated backscatter (CLAB) profiles needs more detail. Do the authors calculate their average CLAB profile by multiplying their average (i.e. molecular plus average particulate) backscatter profile by a two-way transmittance factor calculated using Eq. 3 and the average extinction profile? Or do they average the transmittances calculated separately using several extinction profiles? Note that under conditions where the backscatter and extinction profiles change significantly during the averaging interval, these two calculations will give different answers.

It is informative to simulate three lidar signal profiles in which particulate extinction and backscatter are constant with height in the PBL of height (Z_{mix}) 1km, and the particulate optical depths are 0.05, 0.05 and 0.5, and the lidar ratio, also constant with height is 40 sr. Averaging the individual attenuated backscatter signals (to represent the averaging of individual, measured CALIPSO profiles) gives a quite different result from averaging the backscatters and multiplying by the two-way transmittance calculated from the average extinction profile and Z_{mix} (the “simulated” profile). Although the profiles have similar values at the top of the PBL, they have different shapes and the “measured” profile is about 30% less than the “simulated” profile at the base of the PBL. i.e. $AVERAGE(backscatter * Transmittance_squared)$ is, in general, not the same as $AVERAGE(backscatter) * EXP(-2 * AVERAGE(extinction) * Z_{mix})$.

C199

Note that the CALIPSO algorithms make a significant effort to separate signals of quite different magnitudes by processing the data at several different horizontal scales and by only averaging signals of comparable magnitudes (See Vaughan et al. (2005) reference for full details.)

Does the same effect also affect the Raman lidar signal itself? Do the authors accumulate counts for 30 minutes or do they measure over smaller sampling time intervals, which they process separately then average? (These would give an indication of the variability between samples.) Given that the CALIPSO “Quicklooks” show significant changes in along-track aerosol loading adjacent to Potenza on several occasions, this effect could be significant in the comparisons made in this paper. Could the authors please give more detail on their methods and an assessment of the likely significance of any changes in the atmosphere during their sampling periods?

Page 8438 line 18. “. . . the ozone profile is not highly variable . . .”. Is this true in the PBL at a “polluted” mountain site as the authors describe Potenza? Do any local variations make a significant difference to the transmittance anyway? Also, does any allowance need to be made for the different receiver filter pass bands at 532 nm, which is 37 pm for CALIPSO and 500 pm for the ground-based lidar? (E.g. C-Y She, Appl. Opt., 40, 4875 – 4884, 2001)

Page 8439, line 6. – I cannot see the point of using a Standard Atmosphere, as the atmospheric profile at any location or time can be expected to differ from a standard that has neither seasonal nor latitudinal variations. The authors already use the ozone profile embedded in the CALIPSO Level 1 file. Why then not use CALIPSO’s embedded meteorological profile as this is presumably the profile used in the calibration of the CALIPSO data and for retrieval of later products. Figures 2 and 3 would be far more useful if the USSA profile were to be replaced by the corresponding CALIPSO meteorological profile.

Page 8443 line 1. “PEARL vertical profiles resolution is degraded to the CALIPSO

C200

lower resolution through linear interpolation . . .”. This is not clear to me. In Section 2, the authors state that their Raman system has a vertical resolution of 60 m for backscatter and 60 - 240 m for extinction. By comparison, the vertical resolution of the CALIPSO 532-nm Level 1 data is 30 m in the lowest region above the surface, and 60 m above 8.2 km up to about 20 km. Do they therefore mean that the resolution of the CALIPSO data is degraded to that of the PEARL data rather than the other way around?

P8443 line 25. (effect of specular reflection from ground influencing low altitude measurements) What do the authors mean? How can a signal from the surface affect the signal from the overlying atmosphere? In view of the complex topography, in creating their averaged CALIPSO profiles, did the authors ensure that they filtered out the surface return in each individual profile so that they averaged only atmospheric signals? If not their average CALIPSO signals could be contaminated. Is this what they mean?

Page 8443 lines 27 – 28. Given the spatial separation and spatial and temporal variations in cirrus, as is obvious in the completely different cirrus profiles seen at the two locations, there is little value in saying or implying that the agreement is poor between and 8 km and 11 km in the cirrus region, unless you add a comment “which is not unexpected given the spatial and temporal variability of cirrus and the relatively low number of cases (16) for comparison”.

Page 8443 last line – Page 8444 line 1. “CALIPSO slightly underestimates the direct, ground-based measurements.” In the copy of the Figure 7 supplied for review, the CALIPSO signal is, in fact, slightly larger – not smaller- than the ground-based signal in the region between 2.5 km and 5 km, and apparently the same (within the noise) from 5 km to 8 km. “Underestimates” is, therefore, incorrect on two counts. The authors should just use “larger”, “smaller” or “different”.

Also, surely it is the CALIPSO Level 1 signal that can be described as being “direct” as, apart from the calibration factor, it is just the directly-measured, raw signal, whereas

C201

the signal from the ground-based lidar has been processed to permit comparison with CALIPSO, so cannot be described as being direct.

However, what the authors say in the next two sentences is very reasonable and a very good justification for their subsequent separation of cirrus and non-cirrus cases presented in the following sections.

Page 8444 lines 19-20. "... space-borne lidar measurements of ice clouds ... when observed by lidar at zenith or nadir ...". Omit either "space-borne" or "zenith".

Page 8444 lines 19 – 27. "... these well-known effects of space-borne lidar ..." It has been well known since the early days of lidar, that specular reflection can be detected by any lidar, including ground-based lidars, directed normal to horizontally aligned ice crystals in clouds. (e.g. Gibson, J. Atmos. Terr. Phys, v29,657-660,1977; Platt, J. Appl. Meteorol., v16,339-345,1977; Platt, *ibid.* v17,482-488,1978; Sassen, *ibid.* v16,425-431,1977) While specular reflection hinders the retrieval of extinction profiles through cirrus clouds with oriented crystals, it does assist in the detection of such crystals, which is of considerable interest to researchers into the microphysics of cirrus clouds (e.g. Platt 1978). There was, in fact, argument put against the change of CALIPSO's pointing angle to 3 degrees off nadir for just that reason. (Different researchers have different interests.) So "space-borne" could be replaced with "lidars pointed near the nadir or zenith" in lines 19 and 27. Also, the authors do not explain how this effect would produce the apparent differences in the profiles in Fig. 7.

Page 8445 lines 13 and 16. "Cirrus removing procedure" is a confusing term and used in a different sense from page 8446 line 25. The authors give a clear and precise description of what they are doing in the previous lines. Why not use that description instead – e.g. cirrus attenuation correction or rescaling? The cirrus is not being removed- the signal is merely being rescaled to correct for its attenuation. Line 16, try "a method to correct for the cirrus attenuation".

Page 8445 line 17. Lamquin et al. reference. This is also how the CALIPSO feature

C202

finder estimates the apparent transmittance as described in the Vaughan et al. and Young & Vaughan references and the earlier references therein. This, however, is the *apparent* transmittance and includes the effect of multiple scattering as seen in the equations defining particulate transmittance. Nevertheless, it is the correct variable to use to rescale the signal below the cloud, provided it is calculated correctly. (See comment below regarding line 23.)

Page 8445 line 22. "The ratio ..." is the wrong way around. The ratio of the measured signal to the modeled molecular signal gives the apparent (two-way) transmittance. The optical depth is obtained from the transmittance.

Page 8445 line 23. "just below the cloud" As multiple scattering effects can extend quite some distance into the clear air below a cirrus cloud for space lidars (see Winker 2003 SPIE reference), the method the authors use can give a biased result if the signal immediately below the cloud base is used. The CALIPSO algorithms adopt methods that minimize this effect. First of all a minimum clear air distance is established below the cloud and then the slope of the attenuated scattering ratio signal is tracked until it returns to zero (see section 3.2.8 of the Vaughan et al. reference). Not only does this ensure that there is no weak and undetected aerosol in the region, but it would also minimize the effect of any multiple-scattering tail, although this is not explicitly stated in that reference. Note that both aerosols and multiple scattering in the supposedly clear region below the cloud would bias the transmittance estimate too high (and optical depth too low). If this biased estimate were then used to rescale the signal below the cloud, the resulting signal would be too low.

Page 8445 line 26. "After removing clouds ..." I assume that the authors mean "After correcting both the PEARL and CALIPSO observations for cirrus attenuation ...". See suggestion above for lines 13 and 16.

Page 8447 lines 18- 20. How is the 20% expected error on CALIPSO Level 2 (retrieved layer and optical property) data relevant to the difference between the PEARL

C203

and CALIPSO attenuated backscatter profiles, which is a Level 1 data product, as the authors state?

Page 8447 lines 20 – 24. Given the markedly different behaviors in the PBL and in the free troposphere, is there really any value in quoting a “mean difference of the whole mean profile”? The strong negative difference in the PBL will obviously skew the mean to negative values, so the suggestion of an indication of a negative bias in the CALIPSO raw data is questionable. There may well be a negative bias in the CALIPSO signals, for example if there is unaccounted aerosol in the altitude region used for calibration, but that would be a uniform bias at all heights, not just in the PBL as the authors claim to show here.

Page 8448 lines 8 – 15. “. . . the comparison at PBL altitudes is not appropriate due to the distance . . . and local aerosol content . . .”. This reasoning is quite correct. Unfortunately, the reasoning in the next statement is incorrect. One would only expect a large sample to have a mean difference close to zero if there were no systematic differences between the situations being sampled. However, the authors give good reasons on several occasions to expect that this is not the case.

Page 8448 line 16. “Specular reflections from the ground” Are the returns from the surface not more likely to be diffuse than specular, especially as CALIPSO has been operating at 0.3 or 3 degrees off nadir? Also, it is still not clear how such specular reflections would affect the affect the PBL signal anyway. The authors should clarify what they mean.

Page 8449 lines 1 – 5. These standard deviations are much larger than the means making it impossible to make definite statements about the differences, like the (-2 +/- 12)% residual difference indicating the possibility of an error in the CALIPSO calibration procedure. There may well be a low bias in the CALIPSO calibration, as mentioned above, but, unfortunately, the authors can draw no conclusions on this matter from their results. Also, do these standard deviations represent the spread in the data or are they

C204

standard deviations of the means (standard errors)?

Conclusions section. Much of this repeats, almost verbatim in places, a large amount of material from the previous sections. The conclusion section needs to be made more concise. It also repeats statements that the CALIPSO data are over- or underestimates or biased, when the authors already, in the previous sections, appear to have reached the conclusion that the observed differences were most likely to have been caused by differences in the atmospheric columns being measured, not to systematic errors in the CALIPSO level 1 data. In that case the Conclusions section should present those conclusions and not repeat all the apparently already discarded suggestions that the satellite data are biased.

Last lines: The planned study of spatial and temporal variability will be very useful.

Figure 6. It would be helpful if the plots in (a) and (b) could both be expanded in both dimensions to enable the variations of attenuated backscatter profiles with time/distance to be compared more easily. This could provide insight as to the cause of the differences in the PBL signals.

TECHNICAL COMMENTS appear in a separate report.

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

C205