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Atmospheric Chemistry and Physics Discussions

# *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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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.

Anonymous Referee #1

» GENERAL COMMENTS. This work by Mona et al. is relevant, important and timely. It helps address an importantissue in the determination of the effects of aerosols on the global radiation budget byproviding an assessment of the accuracy of data from the CALIPSO (Cloud-AerosolLidar Infrared Pathfinder Satellite Observations) mis-



sion.Because the uncertainties in the effects of aerosols on the global radiation budgetimpose limitations on the performance of Global Climate Models, global measurementsof aerosol profiles are needed. To ensure coverage over as much of the planetas possible, especially over the oceans, measurements from satellites are required. CALIPSO is the first satellite lidar mission dedicated to the long-term, global measurementof profiles of aerosol properties. However, being a simple, elastic-backscatterlidar, CALIPSO’s primary measurement is of attenuated backscatter profiles and assumptionsrelating the particulate backscatter and extinction coefficients are requiredin order to retrieve profiles of aerosol extinction. Except where CALIPSO can determine the layer transmittance directly, these retrievals use model/climatological valuesof the lidar ratio (the ratio of extinction to backscatter). Although CALIPSO’s algorithms(see authors’ Vaughan et al. 2005 reference) use measurements of the depolarizationratio and color ratio to infer aerosol type and, hence, lidar ratio, this ratio is known tovary 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 validationplan. (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. Thesecond stage involves an assessment of the representativeness of the lidar ratio usedby 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 theCALIPSO profiles with their ground-based profiles and take care to separate caseswhere cirrus clouds, which can complicate the comparison, were present from thosewhere they were not. Their analyses appear to be careful and correct, although several 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-arguedand 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.

In the current study of comparison between PEARL and CALIPSO measurements, we found some differences and through the analysys reported in this paper we give some plausible explanations for them. 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.

Following the suggestion of the referee, differences observed in the PBL are not re-



ported in the abstract of the final version of the paper, since an explanation based on different location sampled is reported in the main text. The conclusions section is completely rewritten in the final version of the paper, following the referee’s suggestion.

» 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 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 justin 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 refection(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 groundbased 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 P8448L18-25 ) 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.

As reported above, following the referee's suggestion we remove terms like underestimates from the text in order to avoid the impression that there are errors due to the instruments. For what concerns the comparison in the PBL, we completely agree with the referee: to invetsigate into details CALIPSO performances in the PBL, data obtained by ground-based stations closer to CALIPSO ground track are needed. This point will be addressed in future work of the full EARLINET network. On the other hand in the current paper we are showing a methodology for the comparison. In the PBL we found some differences case by case, but on average we did aspect a mean difference close to zero with a large standard deviation. This is not the case and we investigated this point and found out a reasonable explanation based on the locations of CNR-IMAA and of the CALIPSO ground track. For this reason, we decided to include the PBL comparisons and the related discussion in this paper where both first results and methodology of the comparison are presented.

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

OK.

» 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 mul-



tiple 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.)

The referee comment is correct. Multiple scattering and specular reflection are not errors but effects that can be useful for obtaining additional information. However, it is important to quantify and consider them for a correct determination of particles optical properties. In order to avoid the impression that these two effects can be considered as errors, the sentence is re-written in the final version of the paper as follows: "This is essential to identify, if that is the case, the contribution of specular reflection and multiple scattering effects, and possible biases due, for example, to low accuracy at some altitude ranges because of low SNR, and to the calibration procedure".

» 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."

We modified the text accordingly to referee's suggestion

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

The reported errors result from the signals’ statistics. Each calculation (also error calculation) are performed on the temporal integrated signal, therefore an almost constant atmospheric aerosol profile is assumed. At this point of the paper we reported the characteristic of the typical profile we produce within EARLINET. For the aim of the network, a typical integration time of 30 minutes was established and the temporal window is chosen in correspondence of atmospheric conditions as much stable is possible. For CALIPSO validation purposes obviously we cannot chose the temporal window as we like because we need a temporal collocated profile. In this context, if the 1 minutes range corrected signal acquired by PEARL shows a large temporal variabily of the atmospheric conditions in correspondence of CALIPSO overpass, a shorter temporal window (typically 10 minutes) is applied for the aerosol backscatter and extinction retrieval.

» Page 8438 line 6 Eq. (3). Note that the CALIPSO ATBDs (e.g. the authors’ Vaughan etal., 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.)

Regarding CALIPSO level 1 data the fundamental document is, “Calibration and level 1 data products, in CALIOP algorithm theoretical basis document” Hostetler 2006, where



the particulate trasmittance is defined as reported in our paper (see Hostetler 2006 , page 25). In the final version this reference is added at this point. However, the multiple scattering contribution to the extinction and therefore to the trasmittance is obviously embedded in the acquired CALIPSO signal and its range corrected signal, i.e. the attenuated backscatter. The multiple scattering contribution is estimated in CALIPSO algorithms after feature type identification: when the type of aerosol or cloud is identified the MS correction is fixed and the optical properties of the layer can be determined. However this procedure regards only level 2 data, Level 1 data instead contains the multple scattering contribution in the trasmittance term. The current paper investigates Level 1 data therefore no multiple scattering corrections to the signals have to be considered.

» 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 (Zmix) 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 Zmix (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 thePBL. i.e. AVERAGE(backscatter*Transmittance_squared) is, in general, not the same as

AVERAGE(backscatter)*EXP(-2*AVERAGE(extinction)*Zmix). 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?

As reported above, all the quantities are calculated using the signal tipically integrated over 30 minutes. As for satellite lidar measurements, the atmospheric variability may generate sistematic errors in ground-based lidar measurements of aerosol if signals are first cumulated and then analysed: a detailed description of this kind of errors is reported in Ansmann et al., 1992. Here a 10% error for the extinction coefficient inside aerosol layers and thin cirrus is reported and is considered not negligible for the backscatter coefficient determination inside the cirrus. For this reason, if the false color image of the 1 minutes range corrected signal acquired by PEARL (like that reported in fig 6b) shows a large temporal variabily of the atmospheric conditions in correspondence of CALIPSO overpass, a shorter temporal window is applied to the aerosol backscatter and extinction retrieval. For such cases, a good compromise between the need to reduce the sistematic error due to the temporal integration and the need to mantain a good SNR is obtained with a temporal window of 10 minutes.

» 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, whichis 37 pm for CALIPSO and 500 pm for the ground-based lidar? (E.g. C-Y She, Appl.Opt., 40, 4875 – 4884, 2001)

The ozone concentration in a polluted site can be highly variable in the PBL as the referee noted. But for the attenuated backscatter calculation, ozone contributes only through the trasmittance term. There was a typo error in the submitted version of the paper where the reported estimated ozone contribution to CLAB was wrong. Considering a typical ozone vertical profile we found that T2 for ozone term contributes for less than 0.5% below 10 km to the attenuated backscatter. Therefore the variation in the ozone profile in troposphere will lead to negligible changes in the attenuated backscatter calculation. The stratospheric ozone contribution instead is larger (1-3%), but at these altitudes local variations can be neglected. This point is corrected and clarified in the final version of the paper: The ozone term contribution on the CLAB calculation is lower than 0.5% below 10 km and within 3% above. Considering that the changes in the tropospheric ozone will not affect significantly the CLAB calculation and that the stratospheric ozone is not highly variable, differences due to the ozone profile used for the CLAB calculation can be considered negligible. In addition, the different receiver filter bandwidths do not introduce significant difference in the calculation of the molecular Rayleigh cross sections because both filters are enough narrow to include only a small part of the pure rotational spectrum of the molecular nitrogen and oxygen. In particular, the CALIPSO bandwidth is very narrow and practically only a little part of the rotational Raman spectrum is observed together with the Cabannes lines. Calculation have been performed by the authors to estimate the differences in the “effective” molecular cross section resulting from the use of filter with 37 pm and 500 pm bandwidth. These differences are lower than 0.1 % in a temperature range starting from 200 K to 298 K.

» 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.

The main important concept that lies under our Level 1 comparison approach is that we do not want to manage CALIPSO Level 1 data and at the same time we want to derive Calipso-like attenuated backscatter starting from EARLINET data independently from CALIPSO. So we do not want to use the same molecular profile CALIPSO uses. This is true also for the ozone profile, but to our knowledge there are no freely available ozone profiles over our site. However, as reported above, since the ozone contribution to the trasmissivity term is really small, one could expect that differences related to different ozone profile used are negligible.

» Page 8443 line 1. "PEARL vertical profiles resolution is degraded to the CALIPSO lower resolution through linear interpolation ...". This is not clear to me. In Section2, 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?

PEARL vertical profiles are reported to the same altitudes of CALIPSO vertical profiles through linear interpolation. We rewrite the sentence in the final version of the paper.

» 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 sur-



face 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?

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. The text in the final version is modified accordingly.

» 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".

We modified the text following the referee suggestion.

» 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 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.

The referee comment is correct, in Figure 7 the CALIPSO profile is slightly higher than PEARL one. We correct this error in the text. Moreover, as already reported above, we change over-underestimate with larger or similar as suggested by the referee. The word “direct” refers here to the determination of CLAB from ground based lidar measurements: only the combined elastic Raman lidar tehcniques and HSRL allow the determination of CLAB from ground based measurements without significant assumptions. However, the word “direct” at this point of the text is removed because it could lead to misunderstanding.

» Page 8444 lines 19-20. “ ... space-borne lidar measurements of ice clouds . . . ïĂăwhen observed by lidar at zenith or nadir . . .” . Omit either “space-borne” or “zenith”.

OK.

» 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.

In section 5.1, before analysing cirrus cases, we present all relevant effects that can affect satellite-borne lidar measurements in presence of cirrus clouds, therefore we modify the text as suggested by the referee at previous point. The cirrus cloud cases contribution to Figure 7 is reported as mean profiles of Figure 8 that is largely commented. Both multiple scattering and specular reflection contributions are discussed (Page 8446 lines 12-19).



» 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”.

We change the text accordingly to the referee’s suggestion.

» Page 8445 line 17. Lamquin et al. reference. This is also how the CALIPSO feature finder estimates the apparent transmittance as described in the Vaughan et al. andYoung & 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.

We modify the text in the final version of the paper: “The ratio between the actual attenuated backscatter just below the cloud and the molecular reference provides transmittance term of the cirrus and therefore the optical depth of the cirrus.”

» 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 Winker2003 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.

For the cases in analysis the bottom of the cloud has been calculated by the authors following a procedure similar to the one describe by the referee. Furthermore it has been also checked by comparison with what reported in level 2 calipso layer data. However we remove the word "just" from the text in order to avoid misunderstanding.

» 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.

OK.

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

To our knowledge, an estimation of errors on Level 1 data products is not available at the moment. So the only available reference in terms of errors is the expected error on level 2 data.

» 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.

The influence of the PBL difference on the mean difference on the whole profile is discussed just after this point. In addition, Figure 11 and 12 report mean differences in some altitude ranges to investigate the altitude dependence of the observed differences. Again, the word bias or under-overestimate is substituted with difference of lower-larger in order to avoid misunderstanding.

» 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.

Exactly, if there are no sistematic diferences between the scenario observed by PEARL and CALIPSO, one could expect that a large number of observations would result in a mean difference in the PBL close to zero, and because of the large variability at these altitudes, in a large standard deviation around this mean value. We rewrite the sentence in the final version of the paper to better clarify our statement:

» 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.

This is correct: the surface return is more diffuse than specular. We modify the text accordingly to this comment. We mean that it has to be taken into account that the surface return in the CALIPSO almost nadir configuration can influence these low altitudes measurements because of its strong intensity.

» 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 standard deviations of the means (standard errors)?

In the paper it is already reported that the small observed difference is in agreement with zero. However, thanks to the referee comment, we modify the text clearly stating that because of the large standard deviation it is not possible to draw conclusions about it, As reported at page 8449 lines 1-8, the reported values of standard deviations are exactly the standard deviation and not the standard errors. Therefore these quantities give info about the spread in the data and the variability of the differences (see lines 5-8).

» 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 underestimate-sor 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.

Conclusions section of the paper in its final version is completely re-written in a more concise way following referee's suggestions.

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.

Figure 6 a and 6b are expanded in the final version of the paper.

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