

Responses to Review 1:

Science issues:

1. α

The alpha coefficient used in this study to best-fit the molecular backscatter from GMAO to the CALIPSO backscatter profiles is an interesting way to show possible problems of calibration especially during daytime and at 1064 nm. However, the study should also focus on the 532 nm channel in the tropics (region of interest for cirrus clouds in the TTL), where the calibration of the nighttime channel can be impacted by the presence of stratospheric aerosols between 30-34 km. I suggest to add a plot which shows the variation with the latitude of alpha for a given period. I suggest also to keep α to the value determined by the algorithm instead of artificially forcing it to 1. A modification of the algorithm is also required to take in account the attenuation of CALIPSO backscatter profiles below clouds. This possibly could lead to underestimate α . After making those corrections, I would suggest to continue the analysis with α .

We should have mentioned the daily mean α values are quite noisy even from the average of all latitudes. This is because not many profiles meet the criteria that enable the fit. In addition, the GMAO model atmosphere is not perfect, contributing to the error seen in the fitted α . Therefore, in this analysis we are not fully convinced that large seasonal variations are due to the calibration.

Following your suggestion, in the revised figure we break the α time series into six latitude bins. Because the stratospheric backscatters are low, their effects tend not to affect the fitted α significantly. Rather, the backscatters in the upper-troposphere and their noise level have a considerable impact on the fitted α values.

Our algorithm did take into account the attenuation points below clouds by rejecting 2-sigma outliers around the fitted molecular backscatter profile. As shown in the new Fig.1, it works quite well. For daytime noisy profiles, clouds and attenuation points might contaminate α , causing large oscillations in the fitted α . This is the area that needs to be improved, if the α method would be used as an independent monitoring method for CALIPSO calibration. This caveat was described briefly in the discussion section.

2. Comparisons with L2 CALIPSO product

Comparisons with the L2 CALIPSO product are done through the paper but by comparing different types of horizontal averaging. I suggest to make use of the flags available in the L2 CALIPSO product which indicates the horizontal average performed to detect a layer. Therefore, only 5 km averaging should be compared with this analysis.

It is a very helpful suggestion. We re-calculate CALIPSO Level 2 monthly climatology using the flag in the files as suggested with only those clouds detected at 5 km horizontal resolution. The new L2 climatology compare more favorably with the results from this study. We revised the discussions about the results based on the new calculation.

The new results change the cloud/aerosol occurrence frequency and the associated seasonal cycle to some degree, in the direction consistent with our assertion that the cloud detection with different horizontal resolution would alter the observed cloud/aerosol seasonal cycle. The new results also exhibit more consistency with those derived from this study, both of which use the 5km horizontal resolution.

3. Figures and captions

I found generally the figures interesting but the author should improve the captions to make them easiest to read and understand.

We revised most of the figures and figure captions. See our revisions and responses below.

Individual points:

P17265, L25:

However : ' The CLOUDSAT radar and CALIPSO lidar sensitivity are really different (eg. CALIPSO is sensitive to very thin cirrus clouds and not CLOUDSAT, CLOUDSAT can penetrate the insight of deep convective system but not CALIPSO). We have then different view of the same cloud, but complementary. Besides, the limb measurements from MLS compared to the nadir observations of CALIPSO and CLOUDSAT make those dataset even more difficult to compare since there are not observing the same features.

We appreciate the suggested sentences to describe limitations of each sensor, and include a sentence in the revision. Although it's difficult to compare these observations due to sensitivity, sampling and noise differences, there are some useful overlapped sensitivity ranges and common sampling volume, from which we compare these cloud observations. In fact, people have used the collocated lidar and radar data to retrieve cloud particle size and other properties.

P17266, L14-L25:

L14-L25 : This part is used as a motivation of this work, it should be explain better. Since the scheme for detecting clouds from the Level 2 CALIPSO data is based on different averaging (5 km, 20 km, 80 km), the author says that if a statistic is performed out of this, it will mixed spatial resolution and will be probably bias the results. However, the way that the CALIPSO level 2 (L2) 5 km cloud/aerosol product is done allow you to compute the frequence of occurrence of clouds at 5 km, since a flag is used to indicate with which horizontal averaging the detection were performed. If the author compute his statistics out of only the detection made over 5 km, the comparison between his product and the official product will be more consistent.

This is a very good point. Like many CALIOP data users, we used the default layer product reported in the Level 2 (L2) 5-km cloud and aerosol layer products (i.e., 05km_CLay and 05km_ALay), which are in fact the products with a mixed horizontal resolution. We really appreciate your suggestion here that helps improving our analysis.

As suggested, we re-calculated the Level 2 cloud/aerosol frequency to use only the layer features detected at 5 km resolution, which is reflected in the changes in Figs. 10, 11, 12 and 14, and the corresponding text.

We reworded the sentences in the Introduction to reflect what we learned about the L2 algorithm from the workshop in the 2010 A-train meeting in New Orleans:

“For the CALIPSO L2 layer products (i.e., 05km_CLay and 05km_ALay), we use only the features detected at the 5-km horizontal resolution. The CALIPSO L2 algorithm employs a detection scheme with variable horizontal lengths on the attenuated total 532-nm backscatter [Vaughan et al., 2009]. By taking advantage of feature signal strength and spatial correlation, the L2 algorithm is able to detect weak features, such as thin cirrus, polar stratospheric clouds (PSCs), and aerosol layers, using an adaptive multi-profile averaging scheme to search for coherent features in consecutive profiles within 5, 20, and 80 km in distance. It starts a feature search with the 80-km window along track, and then proceeds the searching with the 20-km and 5-km window. Each detected cloud/aerosol feature is associated with a flag that contains information on the searching window size(s) whereby the feature was obtained. In the L2 algorithm the detected cloud/aerosol layers must meet a requirement for minimum thickness.”

P17267 :

Enhancements of noise (ex. Daytime over land due to surface reflectance, or SAA) as well as calibration issue (eg. stratospheric aerosols in the calibration zone) will probably lead to underestimate the frequency of occurrence of clouds/aerosols. Even it is announced in p17267, L16, the problem of the calibration are enough explain in this paper.

Agree.

P17268 :

L5. ('version 2.0': :and version 2.02)

Revised.

P17269 :

L3 : At this time, a better explanation of the calibration is required (Powell et al., 2009). In fact, the calibrations of all the channels are based on the calibration of the nighttime channel at 532 nm, assuming no aerosols between 30- 34 km. But even if this assumption is true at midlatitude, the presence of aerosols in the tropics can represent between 5 to 12 % of error on the calibration coefficient (Vernier et al, 2009). The atmospheric background model does not exclude only the attenuation effect (not a calibration issue) of stratospheric aerosols which is most of the time less than 1-2% (Vernier et.,2009) but rather the attenuation effect of all ice clouds which make difficult the retrieval of clouds below. The citation in l8 is not appropriate. In Vaughan et al., 2009 a method is proposed to correct the attenuation below clouds that should be consider in this study.

We modified these discussions accordingly.

BUT I found the iterative method interesting since it has the potential to detect bias due to calibration through the alpha coefficient. It should be performed by implanting a correction of the attenuation due to clouds. In fact the standard deviation approach will surely help you to detect and subtract clouds but probably not take out the attenuation effect below clouds. It would lead to underestimate the alpha coefficient, since low value due to attenuation will remain in the profile after subtracting all the features.

We modified our analysis slightly, using the 2σ approach to screen the outliers, instead of the 3σ approach in the early paper, which produces more robust screening on cloud/aerosol. It seems to improve the results slightly. Additional figures are included in Fig.1 to illustrate how well the 2σ approach can reject the outliers, including the strongly attenuated measurements below a thick cloud layer.

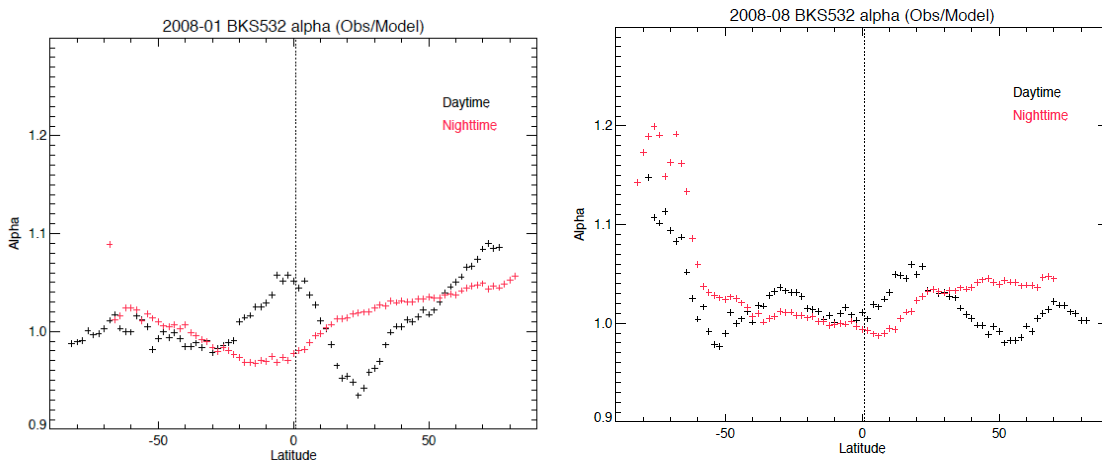
P17271 :

L 20 : The term 'best' is not appropriate here. Fig3. I would suggest to have look at the latitude cross section of alpha for January 2008 since the same period is used later on. It could be a good indicator of the problems of the nighttime calibration in the tropics. L19-21 : This is not exactly true : - CALIOP V 2.01 : - June 2006- August 2008 process with GEOS 5.01 - CALIOP V 2.02 : - September 2008 process with GEOS 5.01 - October 2008- February 2009 process with GEOS 5.20 This could explain maybe the two abrupt changes observed.

We appreciate the insights on CALIOP data processing, and modified the discussions accordingly.

As suggested, we look into the latitude-dependent alpha for Jan and Aug 2008 [see the attached figure below], as well for other years. These plots for different years are similar to the ones for 2008 as shown below. The large 532nm alpha values at the SH high latitudes in August are likely due to PSCs that are not accounted in the fitting with the GMAO model atmosphere.

In addition, we compared GEOS-5 and Aura MLS data and noticed some significant error in GEOS-5 H₂O, O₃, and geopotential height data. Thus, the small latitude-dependence of the nighttime 532nm alpha might be caused by error in the GMAO model as well.



P 17272 :

L 20-24 : I don't understand why alpha is put to 1. Since it seems to be a very good indicator of problems of calibration, then I recommend keeping this coefficient throughout the study and evaluating the difference between the L2 CALIOP detection features and the detection method developed here.

At this point we believe the calculated alpha has captured both CALIOP calibration and GEOS-5 errors, but cannot separate them clearly except the large drop in the daytime 532-nm data. Therefore, in the rest of our analysis we stick with the CALIOP version 2 data. We included a sentence to justify this in the revised paper.

Figures :

Fig 1. The abbreviation 'N' for night and 'D' for day should appear bigger at the middle of the figure and not in the x absciss. A legend should be add to say what represent '+' the data and the black line (GMAO model) . The comment : 'the backscatter is noiser: : ' should be put in the text.

We revised this figure and its capture accordingly.

Fig.2 A legend is required here (-black:day, grey :night) , the x absciss caption should be put in black when it corresponds to the nighttime orbit and grey for daytime. It would really improve and make easy the lecture of the plot.

We revised this figure accordingly.

Fig.3 Legend also. The sentence:'there is a little variability: : ' should be put in the text. I suggest here to make for January 2008 (month used later on) a latitudinal plot of alpha for studying the problem of the nighttime calibration channel in the tropics that could impact the detection of very thin cirrus clouds or aerosols at the tropopause. The evolution of alpha between 10N-10S could also be plotted.

See our response above. We re-drew this figure in the revised paper.

Fig.4. Letter D for day and N for night should be bigger

We revised this figure accordingly.

Fig.5. Same remark

We revised this figure accordingly.

Fig.6-8. Those figures are interesting but required more explanations in the text to be understood. The legend can be improved by writing what represents the lines.

We revised the discussions on the PDF method. and also refer more reading to Wu et al. [2009].

Fig.9.

From here, the α coefficient should be kept and not artificially put equal to 1. I suggest also plotting here the cloud/aerosols detected by the L2 CALIPSO data only with 5 km horizontal average, since a flag is available to distinguish between 5, 20 or 80 km. It will make more sense to compare it with your method of detection based on the same horizontal average.

We didn't change the alpha value because it could reflect error in the model atmosphere as well. The figures are revised accordingly.

Fig.10. What do you mean by 'the overlapping portions in the L2 data are removed'. I suggest here also to use only the 5 km average from CALIPSO L2. Captions on colorbars should be included.

There was an issue related to the L2 cloud layer product that within a given profile cloud layers may overlap with each other. We wrote a piece of code to correct this problem. This turns out to be not significant after we use the features detected only at the 5km resolution. Thus, we removed this sentence.

Fig.11. same remark than fig.10
Colorbar description is included.

Fig.12. The legend should include the grey and black continuous line (color is maybe required ?). The description of the figure (increase) should be included in the text.

We modified this figure to color one, and included the legend for all the lines. The description about the increase trend is moved to the text.

Fig.13. ok

Fig.14. Legend (grey: L2 data, black : this analysis).
We revised this figure accordingly.

Fig.25. One figure should be enough to characterize and explain why the sub-graph start after the noisy daytime part of the pdf. For the other graphs, I suggest to keep only the relative difference of the pdf (day-night), since it is discussed in the text but the sub-figures are too small.

The curvature of these PDFs is also an interesting character of cloud/aerosol statistics, in addition to the occurrence frequency and the day-night difference. It is important to keep them together with the day-night difference.

Responses to Review 2:

1) It is not clear what the ultimate purpose of this work is. The authors state, at the beginning of Section 5, that they have developed a ‘research algorithm for cloud/feature detection . . .’, but I am not sure what that algorithm is. Features of the algorithm are described piecemeal, spread over several pages. They imply the diurnal and geographic variability in ‘sigma’ will cause unwanted biases in cloud occurrence, but it is not clear what constitutes significant or insignificant variations in signal statistics. Before diving into statistical analysis, the algorithm should be briefly described, along with its intended use and fundamental limitations. The algorithm seems aimed at detection of thin clouds in the UT/LS, but this is not made very clear. Statistical analyses of signals throughout the troposphere are presented, and the other reviewer is confused on this point. The algorithm, as presented, is relatively simple and unsuited for accurate cloud detection lower in the atmosphere where it is necessary to account for cloud and aerosol attenuation.

We revised the abstract and introduction to make our motivation clearer. In addition, we re-structured the paper to improve its readability. As stated in the abstract and introduction, the research algorithm is developed to enable further comparative studies with other A-train sensors, particularly with Aura MLS in the upper troposphere.

As seen in our response to review #1, the estimated alpha values have large seasonal and latitudinal variations. In the revised figure we divide the alpha time series into six latitude bins, so as to help interpreting whether the alpha variability is due to the CALIOP calibration, the GEOS-5 model atmosphere, or contaminations of cloud/aerosol.

In particular, we are interested in impacts of the measurement noise on the feature detection in the UT/LS region where feature signals are weak. Like other researchers, we may need to apply more aggressive detection thresholds for feature detection in future studies, but in that case, potential influences of the noise must be carefully dealt with. We hope through the method developed in this study provides us with the confidence in applying these detection thresholds, and the error analysis revealed by the alpha method is illuminating.

However, we disagree with your conclusion on our algorithm being “unsuited for accurate cloud detection lower in the atmosphere”. The attenuation effect will affect only a small portion of the feature detected, i.e., the bottom of a feature layer, only if the upper layer is optically very thick. In many optically thin cases, our algorithm works well and can capture the majority of features. As shown in Fig.9, comparing the features detected by our algorithm using 532-nm TOT to the L2 results, we don’t see the obvious flaw as you suggested.

2) Statements are made such as “a threshold of > 3 -sigma is needed to minimize the false detection rate.” The authors should state what their acceptable false detection rate is. They should also make it clear that this statement applies to their detection algorithm and is not a fundamental requirement. There are other ways of suppressing false detections.

We have revised the statement to reflect your point, and included some discussions on the estimated false detection rate.

3) The authors should also be more clear as to when they are describing characteristics of results obtained with their algorithm vs. characteristics of the standard CALIOP cloud products. The standard CALIOP detection algorithm avoids using 532 nm perpendicular signals and 1064 nm signals for cloud detection to avoid many of the problems discussed in this paper.

We have been focusing on the comparison of the feature detection from the 532nm TOT with the L2 layer product. In our research algorithm, we don't treat the 532nm TOT signal fundamentally differently from the other channels, since we will need to understand them together in terms of measurement noise. As mentioned above, we plan to apply the algorithm to studies jointly with other A-Train data, and these noise characteristics must be dealt with the same caution when depolarization and color ratios are analyzed.

On the other note, although the 532nm PER and 1064nm signals are not used in the L2 layer products, the feature detection and noise handling must be dealt with in other CALIOP standard products, such as depolarization and color ratios.

4) The authors seem to be unfamiliar with the physics of the detection of weak optical signals using photomultiplier tubes. They characterize CALIOP signal fluctuations as 'measurement noise', due to detector noise and scattered sunlight (during daytime). They fail to mention statistical fluctuations in the received laser light at low signal levels (so-called 'photon noise') which largely determines the signal fluctuations at night in clean air. Unlike microwave sensors (and the CALIOP 1064 APD detector), where the dominate noise is instrumental (thermal noise) and a constant, additive term, the source of the 'photon noise' is external to the instrument and is multiplicative. On page 17266 (line 8) the authors note the "noise can vary by more than two orders of magnitude within an orbit," which is true for the perpendicular channel, but it should be pointed out here that the other two channels exhibit much smaller variability. For 532-total the day-night variation is only a factor of 2 or 3. Because the dominant (photon) noise is proportional to the square root of the photon rate, the 'noise' varies with signal strength, which varies many orders of magnitude from clear air in the UT/LS to dense cloud tops. This is not unique to CALIOP, but is characteristic of any well-designed lidar system. It is not clear that their algorithm is optimal, given the fundamental nature of the signal noise, and their interpretation of their results clearly does not recognize certain fundamental differences in the nature of optical and microwave measurements.

It should be noted that our paper is aimed to better understand what are the impacts of noise of the calibrated measurements on feature detection. The origins of these noises may be quite different, but we need to characterize their statistics and distributions for further scientific investigations and interpretations of the CALIOP data.

That said, we appreciate the clarification you made here for us to better understand the sources and properties of the CALIOP measurement noise. We have revised the paper to make sure the noise description is in line with the principles of lidar system.

5) Page 17271, line 5-6 states "The cause of the profile-to-profile variability is likely associated with spatial variations of cloud/aerosol/surface albedo." During daytime, variations in column albedo contribute to this variability, but the nighttime signal also exhibits profile-to-profile variability. Given the iterative processing, where outliers are rejected

on multiple passes, I'm not sure what the nighttime results in Figure 2 represent. The variability of the nighttime 'sigma' is probably due to a combination of high energy particle detections, photon noise, and (for 1064 nm) detector dark noise, but not albedo changes. It's not clear whether there might be contributions from weak cloud or aerosol layers. An additional figure illustrating how the iterative procedure works to filter out cloud and aerosol signals, and what cloud and aerosol features remain as residuals, would help the reader to better understand the entire paper.

In the revised paper, we switched Fig.2 and Fig.3 to avoid the confusion.

We should have made it clear that the μ and σ in the old Fig.2 are computed from the data at altitudes > 19 km assuming $\alpha=1$. There was an error in the old Fig.2, which used an old method and file. It has been re-drawn.

As suggested, we included new figures in Fig.1 to illustrate the screening-fitting method for obtaining the α .

6) I'm puzzled by the statement on page 17273: "In addition to the detector noise and other calibration errors, CALIOP backscatter measurements contain noise induced naturally by the atmosphere and surface." I'm not sure what the authors intended here, but any signal fluctuations due to the atmosphere or surface themselves are signal, not noise. It is sunlight reflected by the atmosphere or surface which contributes to (daytime) measurement noise. The actual detector noise (dark noise) for the PMTs is so small as to be negligible in almost all situations. What is not mentioned here is photon noise, which is the primary determinant of fluctuation of the nighttime signals, and particularly for the perpendicular channel. At high altitudes, 532 parallel signals are typically less than 1 photo-electron per shot per range bin, and less than 1/100 photoelectron for the perpendicular channel (see, for example, Winker et al, GRL, 2007).

This is similar to point #4 you made above. We appreciate the clarification you made and have revised the paper to reflect these points.

In summary of these general points, the discussion of the basic principles and objectives must be made much clearer before publication. If the other reviewer, who is obviously quite familiar with CALIOP data, is confused on these points, the average reader will be also. The general approach is interesting and some very interesting statistics are presented, but the interpretation and discussion needs to be corrected and improved.

Minor points:

Abstract, line 17-18. Make it clear that any difference in cloud occurrence frequency between the 532 and 1064 channels is a difference found in the author's application of their algorithm. CALIOP products do not report cloud detections based on the 1064 nm channel.

This has been revised accordingly.

Page 17273, line 19. regarding "features related to detector error in response to the SAA. . ." This is not an example of detector error, but of detector response to high energy particles impacting the detector. This is simply the detection of an unwanted

background signal.

Revised accordingly.

Page 17266, line 4, and 17267, line 2. “For fair cloud/aerosol detection . . .” should probably be “For unbiased cloud/aerosol detection . . .”

Revised accordingly.

Page 17267, line 10. “international” should probably be “interannual”

This sentence is removed.