

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 and in new Fig.2, 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  $\mu$  and  $\sigma$  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 alpha.

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.