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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new method for retrieval of the extinction coefficient of water clouds by using the tail of the CALIOP signal" *by* J. Li et al.

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Response to reviewer's (Dr.R.Wood) Comments:

Jiming Li et al. (Author)

We are very grateful for the Dr. Wood's detailed advice and comments, which helped us improve this paper significantly. In addition to our point-by-point responses to the Dr. Wood's comments provided below, we also added some detailed discussions in each section of the revised paper.

Major issues:

Question 1: Vertical gradients: The methodology is appropriate for clouds in which the extinction is constant with height. However, it is well known that this is not the case in most liquid clouds, which instead have extinction

coefficients that increase strongly with height. This is because to first order, many clouds have linearly increasing liquid water content with height and an approximately constant cloud droplet concentration. Thus, the particle radius in many liquid clouds increases with the one-third power of the height above cloud base, and the extinction increases with approximately the 2/3 power of the same height (see e.g. Boers and Mitchell 1994, Tellus). Unfortunately, this general behavior means that the retrieval algorithm as it stands is somewhat flawed. I would encourage the authors to consider building into their algorithm this "null hypothesis" for the behavior of extinction rather than the one of no vertical gradient that they (implicitly) assume.

Response: We appreciate the insightful suggestion made by Dr. Wood. In future studies of droplet number concentration and other properties, we will start looking at doing the retrievals with the vertical changes in mind. The lidar signal comes mostly from about 100 m of the cloud top, where the vertical changes are relatively small. Therefore, in this study, the mean extinction information overwhelms the vertical

change of backscatter: if there is a 20% change in extinction coefficient for the top 3 optical depths, the optical depth error associated with the assumption of constant extinction is less than 0.1 [that is: 0.5*ln(0.2)] while the backscatter change due to attenuation is huge (more than 100). But we do appreciate Dr. Wood's suggestion of making the correct assumption about vertical variation and improve the accuracy of retrievals of other properties.

Question 2: Saturation of signal: While I accept that this method may be useful for retrieving extinction coefficients in clouds with modest and low extinctions, I am concerned that the estimates will saturate for higher extinction coefficients. Can the authors convince me that this is not the case? The coefficients retrieved in the paper seem rather low to me (I recall the cloud droplet concentration estimates derived by combining with MODIS effective radii were rather low, e.g. Hu et al. 2007a).

Response: This method will be limited to the performance of the transient response and SNR: the memory at 100 m from cloud top is about 0.3% of the cloud top scatter (Y. Hu, K. Powell, M. Vaughan, C. Tepte, C. Weimer, M. Beherenfeld, S. Young, D. Winker, C. Hostetler, W. Hunt, R. Kuehn, d. Flittner, M. Cisewski, G. Gibson, B. Lin, and D. MacDonnell, "Elevation information in tail (EIT) technique for lidar altimetry," Opt. Express 15, 14504-14515 (2007)). That transient response is almost the same as the signal from cloud backscatter (0.25% of cloud top scatter if the effective extinction coefficient is 3) and there is the chance that we can recover the true backscatter for that situation. Thus, a reasonable estimate of the limit of this method is that the cloud effective optical depth should be less than 3 for the top 100 m. Considering that multiple scattering can help reduce the attenuation and enhance the detectability, we can estimate that the top limit of extinction coefficient retrieval from this approach can be around 60 per km if we have good SNR (nighttime measurements, lots of averaging,...). On the safe side, the limit is 30 per km. On the other hand, extinction coefficient from the Hu et al. 2007 study is less sensitive to the transient response since that method depends only on the depolarization ratio.

Question 3: Lack of diurnal cycle: We know from numerous measurements from both the ground and from space that the cloud liquid water contents in liquid clouds is greater during the night than during the day (e.g. Wood et al. 2002, GRL). So presumably the cloud top extinction must be larger during the night. Yet the results presented here indicate that this is not the case. I find this to be a rather troubling result. I wonder if saturation of the signal (see point 2 above) is an issue such that the increases during the night are not detectable. However, maybe the authors could be encouraged to examine the diurnal variability by restricting their analysis to the major marine stratocumulus regions where we know that the diurnal cycle is strongest. There is a hint in Fig. 6 that values in these areas are actually higher during the night. I wonder if the fractional increase during the night is consistent with the LWP diurnal cycle (see e.g. Fig. 3 in Wood et al. 2002).

Response: This is indeed something that puzzled us as well. There is a remote likelihood that the extinction may have reached the limit of this method. The extinction coefficient from Hu et al (2007), which relies on depolarization ratio only and should have higher limit, also suggest similar numbers. The SNR at nighttime is better and thus we expect that the retrieved values can be higher at night at global scale. In the revised paper, we picked up four major marine stratocumulus regions and studied the extinction differences between day and night. Indeed, the results shown that the cloud top extinctions are larger during the night and the differences also are obvious at these stratocumulus regions. However, the tendency of global mean is opposite with the tendency of the regional mean (please see the section 3.2), we also checked the data carefully and we cannot come up with good a answer to explain the day/night difference at global extent. It still is an area for future studies.

Minor issues:

1: Is equation (1) exact or approximate? Does it assume extinction is con-

stant with height?

Response: The equation 1 is derived from the studies of Platt (1979, 1981), that is:

$$B_c'(\pi, z) = B_c(\pi, z)exp[-2\eta \int \beta(z")dz"]$$

Here, $B_c'(\pi, z)$ is attenuated backscatter coefficient. η is multiple scatter factor. $\beta(z")$ is extinction coefficient. However, we adopt Hu's multiple scatter scheme in our study. Overall, Equation 1 is reliable. Since the cloud top region is the focus in this paper, we assume that the extinction coefficient is constant with height within water cloud top region. To make the paper more clear, relevant references were added in the revised manuscript.

2: Is the method only useful for clouds with optical depths of less than 3? It is stated (p28156, line 15) that "CALIPSO probes clouds to a maximum optical depth of 3"? I can imagine that for the densest marine stratocumulus clouds, optical depths of 3 are reached within a single CALIOP range gate. What happens in this case?

Response: Since CALIPSO probes clouds to a maximum effective optical depth ($\eta\tau$) of 3, for CALIOP signal, this method is only useful for retrieving extinction coefficients in clouds with modest and low extinctions (extinction coefficient maybe below 60 km^{-1}) when layer-integrated depolarization ratio are smaller than 0.35. For water clouds with higher extinction coefficient (extinction coefficient about is 100 km^{-1}), the lidar signal is completely attenuated within only one vertical range bin of CALIOP, so we ruled about these cloud samples.

3: The writing in places is a little sloppy and needs some additional proof-reading for clarity.

Response: We are making editorial changes in the revision to make it more readable.

4: How is the transient response correction (Equation 4) implemented in practice?.

Response: Details about the de-convolution process are added. Please see paragraph 2 of section 2.2.

5: P28162, line 20: Are the authors confident in the extinction coefficient

estimates to 1 part in 10000 as is indicated by the 4 significant figures quoted?

Response: We gave the mean extinction values with two effective figures in the revised paper.

5: Why are many areas of land in Fig 6 gray? Are there no water clouds over many regions of land? This seem strange to me.

Response: The gray areas in Figure 6 indicates the absence of lower water clouds. We used CALIPSO level 2 (333m) cloud product statistics to the occurrence frequency of water clouds and found that the reasons are: 1. Water clouds occur with relatively lower frequency over land, particularly in April and January. 2. In this study, we restricted our analysis to the low level, opaque water clouds that have cloud top heights lower than 2 km. These are the main reasons. In addition: (1) The layer-integrated depolarization ratio of all water cloud samples are smaller than 0.35. (2) Water clouds that have higher extinction coefficients are ruled out. These limitations lead to fewer water clouds samples over land. In view of these considerations, the results over land in Figure 6 are reasonable.

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