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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.

J. Li et al.

lijiming1984@gmail.com

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Response to reviewer's (Dr. Zhien Wang) Comments:

Jiming Li et al. (Author)

We are very grateful for the Dr. Wang's detailed advice and comments, which were very helpful and have led to significant improvements of this paper. In addition to our point-by-point responses to the comments shown below, we added some detailed discussions in each section of the revised paper

Major issues:

Question 1: The critical assumption of the method is homogeneous extinction coefficient of the clouds. Actually, it is not true. Most of low-level water clouds have cloud liquid water content profiles close to adiabatic profiles, which

cause cloud extinction coefficient changing with height accordingly. This could be a significant error source for the slope method and need to be quantified. The vertical dependent cloud extinction also cause that mean extinctions has different biases for clouds with different optical depths.

Response: We agree with Dr. Wang. As Dr. Wang pointed out, most of low-level water clouds have cloud liquid water content profiles close to adiabatic profiles. That is, many clouds have linear increases in liquid water content with height. But, the droplet number concentration within the cloud is approximately constant. As a result, the particle radius and extinction in water clouds both increase with height above cloud base. However, because boundary layer clouds frequently exceed CALIOP's detection limit of effective optical depth ($\eta\tau$<3), the lidar signal can be completely attenuated within a penetration depth of about 100 meters for most boundary layer clouds with modest and low extinction values. So, in this study, only the mean microphysical properties at the top part of the water cloud can be derived from the new method without regard to the light condition and the vertical changes of extinction coefficients

within the top 100m is relatively small compared with the mean extinction coefficient value. The result can help retrieval of the droplet number concentration of water cloud, which has less vertical variation. Our future work will focus on the retrieval of droplet number concentration by combining this method with Bennartz's study (Bennartz, JGR 2007). We already added this discussion in the introduction and conclusion sections.

Question 2: The key of the method is to de-convolute original signals to get corrected one. It will be nice to have a more detail discussion on general procedure to do that. The result presented in Fig. 3 is hard to understand: why corrected signals at and above the peak are larger than the originals.

Response: we added some details about the de-convolution process and rebuilt equation 4. Please see paragraph 2 of section 2.2. If we consider the $\beta^3_{current}$ as the current peak return signal of water cloud, $\beta^3_{corrected}$ as the corrected peak return signal of water cloud. Then, based on the equation 4-7:

$$\beta^3_{current} = \beta^1_{corrected} \times F_4 + \beta^2_{corrected} \times F_3 + \beta^3_{corrected} \times F_2 + \beta^4_{corrected} \times F_1$$

$$\beta^3_{corrected} = \beta^3_{corrected} \times F_4 + \beta^3_{corrected} \times F_3 + \beta^3_{corrected} \times F_2 + \beta^3_{corrected} \times F_1 + ...$$

So,

$$\beta^3_{corrected} - \beta^3_{current} = (\beta^3_{corrected} - \beta^1_{corrected}) \times F_4 + (\beta^3_{corrected} - \beta^2_{corrected}) \times F_3 + (\beta^3_{corrected} - \beta^4_{correc}$$

We can get: $\beta^3_{corrected} > \beta^3_{current}$. The signal above the peak can be inferred from a similar process.

Question 3: Section 3.2 compared day and night difference, but it is important to notice that day and night difference is different than diurnal cycle. CALIPSO data are not able to provide diurnal cycle of clouds; this is why surface based measurements are important. The first two paragraphs in this section need to be modified accordingly. The day-night differences are different than different locations. For example, we know that stratocumulus dominated

regions experience large day-night difference (Leon et al. J. Geophys. Res., 113, D00A14, doi:10.1029/2008JD009835). Thus, discussion in this section can go more detail by considering regional difference. The paragraph from page 28163 to 28164 spend more effort discuss on cloud distribution and number concentration, which are not included in Figs. 5 and 7. Without showing these, it is harder to follow the discussion.

Response: The section 3.2 was re-orgnaized. We picked up four main stratocumulus regions and added a discussion about the regional difference in this section (see section 3.2) following Dr. Wang's suggestion.

Minor issues:

1: The introduction could be better organized.

Response: We already re-orgnaized the introduction.

2: Page 28154, L3: equivalent droplet radius → effective droplet radius.

Response: It was revised already.

3: Page 28154, L13-15: Refs are needed here; lidar and radar are not in situ measurements.

Response: We already re-orgnaized the introduction and added some related references, revised similar errors .

4: Page 28154, L19:2000 → 2001.

Response: We already revised similar errors .

5: Page 28155, L11: take "light" out.

Response: It was revised already.

6: Page 28155, L19: change backscattered to scattered.

Response: It was revised already.

7: Page 28155, L20: Can you provide results better than ground-based observation at a given location?

Response: We already revised inappropriate sentences.

8: Page 28155, L22: take "and described in some detail" out.

Response: It was removed.

9: Page 28156, L13:β and β_0 already defined above.

Response: It was revised.

10: Page 28157, Fig.1: it will be good to only plot data below 15km.

Response: We already re-plotted the figure 1.

11: Page 28159, Eq.(3) Add index j to F.

Response: It was revised.

12: Page 28162, Fig.4: it will be good to over-plot means and standard deviations in the figure.

Response: We already re-plotted the figure 4.

13: Page 28162, L19 and L21: with the accuracy you have, there is no point to give mean extinction with two decimal numbers.

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

14: Page 28163, L18-19: Do you discuss similarity in terms of pattern or

magnitude?

Response: The section 3.2 was re-orgnaized. We also added some detailed discussions.

15: Page 28163, L22: take "effective" out.

Response: It was removed already.

16: Page 28165, L8-10: The reason is simple: cloud mean LWCs or LWPs for clouds with the same thickness are decrease with cloud temperature decrease. Therefore, there is weak multi-scattering effect at colder cloud in general. On the other hand, ice cloud depolarizations are controlled mainly by ice crystal shapes.

Response: Thanks for your advice. We already added your suggestion in the revised paper.