Review "**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.

The paper presents an interesting new method to retrieve cloud mean extinction near water cloud top. The method could have important applications for space and airborne lidar applications. The paper needs some revisions to address the following issues before publication.

The major issues:

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.
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.
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.

Minor issues.

1. The introduction could be better organized
2. Page 28154- L3: equivalent droplet radius-- > effective droplet radius
3. Page28154, L13-15:Refs are needed here; lidar and radar are not in situ measurements
4. Page 28154, L19: 2000->2001
5. Page 28155, L11: take "light" out
6. Page 28155, l19: change backscattered to scattered

7. Page 28155, l20: Can you provide results better than ground-based observation at a given location?
8. Page 28155: l22: take "and described in some detail" out.
9. Page 28156, L13: β and β_0 already defined above.
10. Page 28157, Fig. 1: It will be good to only plot data below 15 km.
11. Page 28159, Eq. (3): Add index j to F.
12. Page 28162, Fig. 4: It will be good to over-plot means and standard deviations in the figure.
13. Page 28162, L19 and L21: With the accuracy you have, there is no point to give mean extinction with two decimal numbers.
14. Page 28163, L18-19: Do you discuss similarity in terms of pattern or magnitude?
15. Page 28163, L22: take "effective" out
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.