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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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Review of "A new method for retrieval of the extinction coefficient of water clouds by using the tail of the CALIOP signal", by Li, Hu, Huang, Stamnes and Yi

Recommendation: Accept subject to some revision Review by Robert Wood, University of Washington

Overview:

This paper presents a very interesting method to estimate the extinction coefficient of liquid water clouds from space using the attenuation and hence vertical gradient

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Interactive Discussion

Discussion Paper

of the lidar backscatter. Extinction is a critical parameter since it is proportional to the droplet surface area. Thus when combined with visible/near IR estimates of the effective radius, extinction provides a direct estimate of the cloud droplet concentration, one of the key microphysical variables of liquid clouds.

The retrieval looks to offer a promising approach to estimating extinction in liquid clouds but I have some significant reservations as to its current ability to do so. Chiefly, these are:

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.

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).

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

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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).

Minor comments:

1) Is equation (1) exact or approximate? Does it assume extinction is constant with height?

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?

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

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

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?

6) Why are many areas of land in Fig 6 gray? Are there no water clouds over many regions of the land? This seems strange to me.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28151, 2010.

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