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Interactive Comment

Interactive comment on "Extinction coefficients retrieved in deep tropical ice clouds from lidar observations using a CALIPSO-like algorithm compared to in-situ measurements from the Cloud Integrated Nephelometer during CRYSTAL-FACE" *by* V. Noel et al.

V. Noel et al.

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Answer to Interactive Comment - Anonymous Referee #2

Main Comment

The Reviewer's main concern is that the relevance of the present results to the CALIPSO observations are not presented. The Reviewer notes that this is important since 1) the CALIPSO footprint (˜100 m) is much larger than the CPL footprint (˜1 m), hence CALIPSO observations will be much more affected by multiple scattering effects; and 2) observations from the CPL show higher small-scale variability than those from the CIN. First, the Reviewer correctly notes that in the present paper the limited multiple scattering in CPL observations (thanks to the small field of view) allows for a simplified equation when retrieving the extinction-to-backscatter coefficient. When analysizing actual CALIPSO observations, it will not be possible to neglect the impact of multiple scattering in the Deep Convection algorithm. In its reviewed state, the current study silently assumed that multiple scattering effects would be properly accounted for during the analysis of CALIPSO observations through the use of an appropriate multiple scattering factor. This parameter is retrieved operationally, using algorithms and observations which are neither used nor presented in the present study. As such, the validation of CALIPSO's multiple scattering parameter is outside the scope of the Deep Convection algorithm itself. Nevertheless, the Reviewer is right to conclude that this as a limitation of the current study when seen as a validation of the CALIPSO algorithm. The fact that the correctness of the paper's conclusion (i.e. that the CALIPSO Deep Convection algorithm produces consistent extinction coefficients) depends on the availability of an appropriate multiple scattering correction factor is now acknowledged in the paper (Sect. 3.2), and also mentioned at length in the discussion (Sect. 5). Secondly, the Reviewer argues that it is unusual to find a higher small-scale variability in CPL observations than in CIN observations, remarking that 1) the lidar's footprint is much bigger than the nephelometer's and 2) "retrievals typically wash out small scale variability". The lidar's footprint is indeed much bigger than the nephelometer's, by several orders of magnitude. The instrument footprint is not the only parameter that can influence the observed variability in extinction retrievals, however this is an insightful remark, for which the authors have no specific explanation yet. This remark is now mentioned in the text (Sect. 5), and a note has been included to explain that small-scale variations in CPL extinction retrievals should be treated with caution as they may not reflect a physical behavior. The meaning of "retrievals typically wash out (...) variability" is unclear to the authors given the context. In the authors' understanding, typical extinction retrievals do not wash out variability, instead they usually

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end up with a vertical profile that diverges at greater ranges, as the signal-to-noise ratio decreases while the distance from the telescope grows. In this configuration, the signal variability actually increases during the retrieval process. This is notably the case when using the forward solution to the lidar equation (Eq. 2), as in the current paper. This effect is mentioned in the text (Sect. 5) to explain the very high variability in CPL retrievals as the lidar signal penetrates deep into the convective clouds. The Reviewer might have implied that typical retrievals usually include significant averaging, which smooths out small-scale features while improving SNR. However, in the present case the number of colocated data points was severely limited, and the number of profiles used for extinction retrievals was not significant enough to smooth out the variability.

Specific Comment

p. 10658 l. 15 : The Reviewer refers to a section of the paper where the differences in extinction observed from both instruments are tentatively explained by the possible entry of the WB-57 in a cloud-free region, and asks wether supplemental WB-57 data can be found that support this explanation. Following this remark, total water mixing ratio measurements from the Lyman-α hygrometer were compared to the extinction coefficients. It was not possible to correlate the drop in the CIN extinction coefficients with a decrease in water mixing ratio, thus disproving the cloud-free region explanation. This observation is now mentioned in the text and the cloud-free hypothesis has been removed. This remark is similar to the Reviewer #3's comment #6. The authors would like to thank both Reviewers for their constructive suggestion.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10649, 2006.

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