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Atmospheric Chemistry and Physics Discussions

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 3

Major Comment

The Reviewer's major comment is that limitations in the way extinction coefficients are retrieved from both instruments are not discussed enough. He cites several sources of potential uncertainty in both instruments, and suggests to add error estimates to the retrievals.

Regarding the first part of this comment, the Reviewer's suggestions highlight real limitations in the two ways extinction coefficients are retrieved, that were perhaps not made clear enough in the original reviewed paper. The discussion (Sect. 5) now makes clear that limitations exist in both extinction retrieval processes, and exposes them at greater lengths (including the limitations mentioned by the Reviewer). The fact that these uncertainties might explain the differences observed in colocated observations is also mentioned.

Regarding the Reviewer's second point, error estimates were already included in the study as reviewed. For the CIN observations, the error levels provided in the instrument data files were used and included when creating the figures of extinction profiles. The Reviewer specifically mentions a paper by Heymsfield et al. which suggests potentially strong biases in the CIN retrievals. Results from this study do not represent new error estimates, however this study was specifically mentioned in the conclusion of the reviewed paper. This approach (displaying the published error levels and mentioning potential biases) seems like the most conservative and cautious, in the absence of new, re-processed CIN data. Regarding CPL observations, standard deviation was provided in the figures to give an evaluation of the extinction retrieved variability. As the CPL retrieval already suffer from many unknowns also pointed out by Reviewers (such as the lidar ratio variability), home-produced error estimates would be of minor significance and would likely fail to illuminate the results in a meaningful way.

Specific Comments

1) p. 10650, l. 24. The Reviewer refers to a sentence from the paper introduction, where it is stated that "the dominant [radiative] effect of [of cirrus clouds] is still unknown". The Reviewer asks if this absence of conclusion could be attributed to a negligible effect (i.e. the dominant radiative effect would be close to zero).

While this interpretation is certainly reasonable, the issue of the radiative impact of cirrus clouds has not yet been settled conclusively in litterature, and is well outside the

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scope of the present study. Since this particular statement is not directly relevant to the present results, the discussed section of the text has been rewritten to avoid the issue (Sect. 1).

2) p. 10650, l. 26. The Reviewer notes a repetition in the manuscript. Following this comment, the repetition has been corrected.

3) p. 10651, l. 24. The Reviewer highlights a section of the manuscript that refers to the launch of the CALIPSO mission as a future event, and correctly notes the event has already taken place. Following this comment (and similar ones from other Reviewers), the text has been corrected.

4) p. 10653, l.11. The S(T) parameterization was provided by D. L. Hlavka (Goddard Space Flight Center) through a personal communication. This is now mentioned in the text. It is currently used in the production of CPL extinction products. This equation is based on a polynomial regression on retrievals of backscatter-to-extinction coefficients and observed temperatures, in transmissive cloud cases where the use of transmission-loss algorithm was possible.

5) p. 10654, l. 20. The Reviewer refers to Eq. 1, which gives the expression of the backscatter-to-extinction coefficient as a function of the cloud-integrated attenuated backscatter, and asks if it is physically sound to consider a mean lidar ratio for the entire cloud column, since microphysical properties are extremely variable with altitude (especially in a highly convective system).

The Reviewer is right to remark that using a constant lidar ratio through a cloud column might introduce unknown bias in the result, skewing extinction retrievals one way or the other. However, doing so is common pratice in lidar retrievals of extinction coefficients, as making this assumption allows for an analytically stable resolution of the lidar equation $\alpha = S\beta$. Moreover, in the present case, the lidar penetration distance in the convective systems is limited due to the high optical depth, and is constrained to relatively high altitudes (>13 km) hence the cloud microphysical composition should only undergo limited change. In this regard, a study of ice crystal shape classification from the CPL depolarization ratio suggests that only limited microphysical change happens above 12 km in the convective systems observed in July 28 and 29 during CRYSTAL-FACE (Noel et al. 2004). Nonetheless, this is a significant limitation of the lidar extinction retrieval algorithm and it is now mentioned in the text (Sect. 3.1) and discussed in the conclusion (Sect. 5).

6) p. 10659, l.5. 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 2's specific comment. The authors would like to thank both Reviewers for their constructive suggestion.

7) The Reviewer wonders if there is a way to correlate the differences found in extinction coefficients from both techniques to the spatial and temporal dislocations of both aircraft. This is similar to Reviewer 1's comment 4. This seems like a reasonable hypothesis to explain the observed differences. However, comparing time difference and horizontal distance between aircraft with differences in extinction did not reveal any significant correlation. This is now mentioned in the text (Sect. 4).

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