**Atmospheric
Chemistry
and Physics**
Discussions

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Interactive comment on "Evaluation of CALIOP 532 nm AOD over opaque water clouds" *by* Z. Liu et al.

Anonymous Referee #1

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The authors examine dust and smoke aerosols in two different regions in the Atlantic. Situations with opaque water clouds are used to provide a directly retrieved value of the aerosol optical depth. The retrieved aerosol properties are presented and compared with other literature estimates. Furthermore, the constrained optical depths are used to evaluate CALIPSO L2 products. Overall the study presents unique observations of dust/smoke over cloud layers and provides a detailed analyses of CALIPSO L2 products; therefore this reviewer finds that this paper may be suitable for publication after the following concerns are addressed.

MAJOR COMMENTS

pg 13-14 section 3.5: I have some concerns that the FC method may be very sensitive

to calibration biases. For example, if the measured backscattered profile was biased high, then even after subtracting the molecular backscatter, the remaining bias in each range bin would be attributed to an aerosol optical depth. Although it is noted that an improved calibration is used (pg 15 lines 18-20), the authors could quantify this possibility by applying the FC method to those profiles which have no aerosol detected in the L2 retrievals (i.e. the subset used in Fig. 3). Those optical depths will be quite noisy, but should be randomly distributed about zero if no calibrations biases exist.

The manuscript would benefit greatly if some effort was made to improve the quality of the figures: many lines are hard to see and distinguish from others and some color scales are poorly designed making it difficult to see patterns. Specifically:

The red boxes in Figs. 6 and 7 I assume correspond to those in Fig 1. Longitude and latitude labels should be added to Fig. 1 to make this clear. Also in-text references to the "red box" confused me at first, I think it would be better to reference these areas as the "spatial domain".

Fig. 2: why does the fit not exist for smaller sizes? The fit appears not to be used anywhere in the manuscript, so I would suggest just removing it entirely.

Fig. 3: the range on the color scale in panels (b) and (c) should be reduced to highlight the patterns in the red boxes.

Fig. 5: it is hard to see the difference between the dark and light green in panel (c), suggest changing one of them to a different color.

Fig. 6 and 7: reduce the range of the color scales so the patterns in the red boxes can be seen. Also, since the focus of the study is on the domains in the red boxes, I don't think it is necessary to show the regions outside those areas on these plots.

Figs. 11-13: the red and blue reference lines are very hard to see on the panels that plot 2D histograms. I would suggest making these lines thicker and picking different colors than red or blue since both of those colors are already used to color the histogram.

Figs. 3a, 6a, 7a: instead of plotting the number samples here, it would be nice to see this expressed as a fraction of the total number of CALIPSO samples. This would help provide the reader with the context of how often the OWC technique can be used.

MINOR COMMENTS

pg 4 line 10: need citation for sentence ending in "year round"

pg 4 line 11: need citation for sentence ending in "during the summer"

pg 6 eq 1: It doesn't appear that this solution is actually used in the study, so I would suggest removing it from the manuscript.

pg 7 section 3.2: much of the information in the section is repeating material already presented in section 2. I would suggest only discussing the geographical regions in section 2 and moving the details of the CALIPSO algorithms to this section.

pg 7 line 20: here the authors state that multiple scattering is usually negligible, but for the focus of this study, dust and smoke, that isn't quite true (for example see Wandinger et al. Geophys. Res. Lett. 2010). The authors themselves contradict this statement on pg 25 lines 1-8 where a discussion of potential dust multiple scattering factors are discussed. It should be made clear here that while the CALIPSO retrievals assume no multiple scattering, physically that isn't always true for the aerosols being examined in this study.

pg 9 line 2: for the re-scaling only a single aerosol type is used so "dominant" should be changed to "only"

pg 10 lines 17-18: The sentence "This ratio is..." is confusing. There is no depolarization term in Eq. (4).

pg 11 line 17: is there a particular justification to using cloud tops lower than 2 km? Seems like the only concern here is to make sure that ice clouds aren't used. But in the tropics the melting layer remains somewhat constant at about 4-5 km, so 2 km seems

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overly restrictive. Also, why not use the temperature to ensure liquid phase instead of an altitude cutoff?

pg 12 lines 8-21: I found this paragraph to be not clear. The way the lead in is to this paragraph gives the impression that some estimate of particle size is used here. After reading it over a few times I think what is actually done is that the measured backscatter (B prime) is integrated over the water cloud layers and Eq. 5 is used to quantify the multiple scattering contribution. Then the variability of this single-scattering integrated backscatter is attributed to differences in the liquid lidar ratio and hence particle sizes. If that is the case, it might be interesting to see what the spatial distribution of Eq. 5 looks like, since it is possible that the spatial variability presented in Fig. 3 is some combination of difference in multiple scattering and particle size.

pg 13-14 section 3.5: It is not clear what lidar equation solution is used, is it equation 2?

pg 15 lines 1-6: how is the solar background, which is a random error, responsible for a bias? Also is it not correct to say that more averaging can reduce a bias error, only a random error can be reduced by averaging. I think what the authors are getting at is that the solar background necessitates more averaging, which then increases the chances of including broken clouds in the sample. So it's not the solar background causing the biases, as stated in these lines, but the inhomogeneity of the clouds.

pg 15 lines 7-13: most of this can be removed, it is already discussed in section 2

pg 15 lines 22-23: why must the water clouds be single layered? As long total column liquid optical depth is opaque it shouldn't matter how many layers there are.

pg 17 lines 19-21: the sentence "Sa is an..." is a little too strong of a statement; there are many HSRL and Raman lidar studies that show plenty of variability for a single aerosol type, even within the same profile (i.e. the vertical variation). The authors themselves say this on pg 25 lines 25-27, noting the natural variability within a single

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aerosol type to be quite large.

pg 21 lines 3-6: citation(s) needed for these statements

pg 21 line 17: quantify "well correlated", i.e. give the correlation coefficient

pg 22 line 20: quantify "well correlated", i.e. give the correlation coefficient

pg 23-25 section 4.5: The discussion of the relationship of lidar ratio error to optical depth error is nice to see. Does this also depend on the type of solution used? For instance I would expect a different relationship between the two for Eqs. 1 and 2 since propagating error through these two equations will give different expressions. If that is the case, it would be nice to point that out in this section.

pg 25 line 16: suggest changing "water clouds," to "water cloud during nighttime,"

TECHNICAL CORRECTIONS

pg 9 line 2: remove the extra period

pg 9 line 13: change "(based" to "based"

pg 10 line 3: remove "recently"

pg 14 line 9: change "ad" to "and"

pg 18 lines 15-16: this sentence can be removed

pg 22 lines 22-23: remove the sentence "The following factors..."

pg 27 line 8-9: remove underlining