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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of cloud statistics from spaceborne lidar systems" *by* S. Berthier et al.

Anonymous Referee #1

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GENERAL REMARKS

This paper introduces a new method for detecting layer boundaries in lidar backscatter data. The authors describe their method, then use it to derive the distribution of maximum cloud tops detected in the measurements made during the 1994 flight of LITE. They then compare the cumulative probability distribution function (CPDF) derived from this data to similar distributions derived from the lidars aboard GLAS and CALIPSO, and the composite passive sensor retrievals produced by the ISCCP. The CPDFs from the lidars show that the ISCCP data set substantially underestimates the occurrence of high cloud. The authors further find "significant differences" between the cloud top height distribution they derive from LITE, and the corresponding distributions obtained from GLAS and CALIPSO. They attribute these differences to the superior

SNR of LITE.

The differences between the various data sets are illustrated in Figure 4, which shows CPDFs for ISCCP, GLAS, and CALIPSO, along with two CPDFs from LITE: the one derived by the authors using their new method, and a second, older version originally presented in Winker 1998 (I believe there are typos in the legend for Figure 4 and on line 11 of page 5278. According to the authors' references, Winker 1996 is the LITE overview paper, whereas Winker 1998 is "Cloud Distribution Statistics from LITE"). The authors further remark that their results are quite similar to Winker's (page 5278, line 11). However, above 12-km, the correspondence between the author's CPDF and the LITE distribution attributed to Winker 1996 is worrisome. Winker 1998 used a very simple threshold method: any layer that saturated the 532 nm digitizer was considered to be a cloud. The Winker paper rightly notes that 'extensive' regions of subvisible cirrus around the ITCZ went undetected by this simple detection scheme, and thus one would expect that a more rigorous analysis would indicate substantially more high cloud than is shown in the Winker 1998 CPDF. Instead, the authors' results are a fairly close match for the Winker results above 12-km. The explanation for this apparent anomaly could be simple though, as the CPDF attributed to Winker in Figure 4 appears to be at odds with (what I believe is) the original figure (#3) in Winker 1998. Eyeballing the plots in Winker 1998, the cumulative probabilities appear to be ˜0.85 and ˜0.90 at altitudes of 12 km and 14-km, respectively. Adjusting these two points would make the Winker LITE distribution look much more similar to the GLAS and CALIPSO CPDFs, and much less similar to the author's newly derived CPDF.

This is an interesting paper, in that it highlights important differences between the cloud top height distributions derived from traditional passive sensor measurements (ISCCP) and those obtained from the new generation of space-based lidars. However, after several readings I remain puzzled by the large discrepancies between the authors' results from LITE and the results reported by GLAS and CALIOP. For readers to correctly evaluate the differences, and the effectiveness of the proposed new detection scheme, the

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authors should…

[o] Be much more clear about the signal processing applied to the LITE data prior to the launching their detection scheme. I'm afraid the manuscript is rather murky in this regard. At one point (page 5275, line 5) the authors imply that they use LITE data averaged over 10 seconds. If true, this could indeed account for the differences; e.g., see the differences in the CPDFs for the GLAS data sets detected at 0.25 Hz and 1 Hz. (However, if this is not what was done, then the manuscript is in error at this point. LITE data was acquired and is distributed at 10 Hz, and is not '10-s averaged…data'.)

[o] Provide a more straightforward description of the detection method would be useful; e.g., layers are identified whenever Sf[k] $>$ F for consecutive data points spanning an altitude range of 100 meters or more (if indeed that is what is done). Along the same lines, it would be useful to know the signal regime in which F is applied (e.g., the GLAS normalized lidar signal, the attenuated backscatter data reported in the CALIOP Level 1 data, etc.)

[o] Perhaps most important, the authors should provide a clear and unambiguous comparison of the CPDFs (or, better, the PDFs) obtained using their method to those reported by either GLAS or CALIOP. Given that both data sets appear to have been processed using the authors' new technique (page 5272, 17), I'm mystified by the omission of this kind of comparison plot (e.g., a plot comparing GLAS results to those obtained by application of the 'local method' to the same GLAS data).

The subjects addressed in the paper are well suited for publication in ACP. The scientific methods used and assumptions invoked are valid and well substantiated by citations to existing literature. However, until the issues above are addressed, I cannot fully endorse the authors' conclusion that their new detection scheme has "proved to be quite powerful" (page 5287, line 17). Furthermore, with respect to Figures 4, 5, and 6, it should be noted that, unlike GLAS and CALIOP, LITE data acquisition was intermittent, and, for the most part, each data acquisition period was carefully scripted to (try to)

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observe specific targets. These differences in sampling strategies, together with the much more limited observation period of LITE, will influence the shape of the CPDFs shown in Figures 4 and 5. (Rather than use LITE for their primary example, perhaps the authors might consider using the GLAS data acquired from September through November of 2003?)

SPECIFIC COMMENTS

page 5270, line 19: change 'lidar signal ratio' to 'lidar signal-to-noise ratio'?

page 5270, line 25: remove the second occurrence of the word 'impact'

page 5272, line 2: please add a reference for 'New spaceborne backscatter lidar missions'

page 5273, line 15: the symbol sN is used in the text, vs. sB in equation 1 (N for noise, B for background?)

page 5273, line 18: I assume the authors mean 'e.g., between 19 and 20 km', rather than 'i.e., below 19 and 20 km'? If so, then why choose that range as 'an altitude range where only noise is expected'? Wouldn't something higher be more appropriate? During the LITE mission, the remnants of the Pinatubo eruption were still fairly prominent in that region. And even for CALIPSO, there is evidence of aerosol contamination there (e.g., Thomason et al., http://www.atmos-chem-phys.net/7/5283/2007/acp-7-5283-2007.html).

page 5274, 3: I'm curious to know how a median filter could be applied to the CALIOP data, as the vertical resolution of that data varies.

page 5274, line 6: "Two distributions are thus retrieved…" Is this procedure automated? How much data would be required to generate truly representative histograms?

page 5274, line 13: change 'has been' to 'have been'

page 5274, line 14: after quickly rereading Palm & Spinhirne 1998, I find

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no mention of the GLAS cloud-aerosol discrimination technique. A better reference would be the GLAS algorithm theoretical basis document (ATBD; http://www.csr.utexas.edu/glas/atbd.html).

page 5274, lines 14 & 15: I'm having problems with the phrase 'the LITE and GLAS prototype algorithm'. I know of a GLAS prototype algorithm (e.g., see Liu et al., 2004 and/or the GLAS ATBD), but I've seen nothing for LITE.

page 5275, line 10: change 'corrected and navigated' to 'calibrated and geolocated'?

page 5275, line 26: change 'elaborated' to 'derived'? also, 'optical' is misspelled.

page 5277, line 3: what is actually being compared? Is it 'the cloud classifications'? Or is it the layer detections, prior to classification?

page 5277, lines 7 & 8: to repeat an earlier remark, what kind of signal processing was done prior to processing the GLAS and CALIOP data using the local method? And how does this processing compare to what was done for the operational algorithms. This information must be provided so that readers can properly assess the correlation coefficients that are reported in Figure 4.

page 5277, line 18: change 'values has' to 'values have'

page 5278, line 18: what does it mean to be 'better distributed'?

page 5279, line 9: change 'calculate of' to 'calculated for'

page 5280, line 5: should 'David' be given either a first name or a last name?

page 5280, line 15: there's a slight discrepancy between the definition for high, middle, and low provided here, and the one give earlier (page 5274, line 25)

page 5281, line 8: perhaps the consistency between the three sets of results has something to do with the fact that, despite the differences in SNR, the cloud/aerosol optical depths that can be reliably penetrated by the three systems are pretty much the same

page 5281, line 13: with respect to the detection of mid-level clouds, the gradient method for cloud aerosol discrimination has difficulties in multi-layer situations, and tends to misidentify attenuated clouds as aerosols (see Liu et al., 2004). Perhaps this has some bearing on the authors' observations in the section?

page 5281, line 18: one would naively think that the multiple scattering in LITE signal would increase the detection of lower level clouds. What is it about this new algorithm that causes the opposite effect?

page 5281, line 22: a latitude band from 20° S to 60° S hardly qualifies as "the southern polar latitudes"

page 5282, line 13: while I think the comparison of results derived from both active and passive sensors adds huge value to this paper, due to the spotty spatial coverage of LITE, I believe the comparisons would be more informative if they were carried out with either GLAS or CALIOP.

page 5282, line 25: ISCCP looks more like 50%, whereas the lidars appear to converge on a value around 40%.

page 5283, line 13: what is meant by 'the interest of the active instrument'?

page 5286, line 7: change 'could seems' to 'could seem'?

Figure 1: The color contrast between the lines for section 1 and section 2 should be sharpened (or perhaps a different line style could be used?) The contrast of the line for section 3 (in yellow) and the background (white) should also be enhanced.

Figure 2: I do not understand how the gradient method described in section 2.1 can identify the faint layer in the upper right hand corner as cloud, while still identifying the layer at ˜4-km vertically and just south of 4.6° of latitude as an aerosol. The data in that aerosol layer is saturated, hence one would expect that the maximum gradient to be

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fairly large…and certainly larger than the gradient in the cloud in the upper right corner. Qualitatively, the signals from that region of the aerosol layer strongly resemble the cirrus cloud signals in the upper right hand corner of the plot. In any case, the gradient method would appear to be a sub-optimal choice for the LITE data, as LITE is so often saturated…

Figure 2: Once again, the contrast between yellow and white is low, hence it's difficult to identify isolated patches of aerosol in the lower image.

Figure 3: What data segments were used to construct these plots?

Figure 4: The information content of this diagram might be more readily accessible to a broader audience if PDFs were plotted, instead of CPDFs. Also, shouldn't the legend read 'Winker, 1998', and not 'Winker et al., 1996'?

Figure 6: The correspondence between the three lidars is seems much better than I would have expected given the disparities shown in Figure 4…though perhaps my judgment is clouded by color contrast problems again, as it's especially difficult to distinguish between the GLAS line and the CALIOP line.

Figure 7: Why are all three data sets plotted using a different Dz? Comparing the results would be easier if the same Dz value was used uniformly, and if the color bar was scaled according to percentages, not absolute numbers. Also, a line showing mean tropopause height as a function of latitude might make a nice addition to these plots.

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