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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.

S. Berthier et al.

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Question: (I believe there are typos in the legend for Figure 4 and on line 11 of page 5278. According to the author's references, Winker 1996 is the LITE overview paper, whereas Winker 1998 is "Cloud Distribution Statistics from LITE").

Answer of the Author: The reviewer is right. The LITE CPDF is given in the Winker 1998 article, named "Cloud Distribution Statistics from LITE" and not in the Winker, 1996 reference. We make the correction in the article in the section 4.1.

Question: 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

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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.

Answer of the Author : The explanations for such discrepancies between the curve given by Winker et al., 1998 and the curve we give in the figure #4b of our article is that we didn't take account in this figure of the clear sky ratio. The initial CPDF given by Winker has been thus normalized in consequences to compare with our results.

Remark of the reviewer: 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 authors should:

Question: [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';.)

Answer of the Author : The reviewer is right. The LITE data was obviously acquired and distributed at 10 Hz, and not averaged over 10 seconds. The work has been made in this view. The correction has been made inside the article, by changing "of 10-s 5 averaged atmospheric backscatter data';, to: "with a pulse repetition rate of 10 Hz";

Question: [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.) Answer of the Author: We change the writing of the equation 1 to give a better understanding.

Question: [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).

Answer of the Author: The GLAS and the CALIOP PDF and CPDF given in the figure 4a and 4b has not been retrieved by our 'local method';, but correspond to the L2 operational product given by the GLAS and CALIPSO Science Team. Then, the reviewer

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asks for a comparison between the processing of the GLAS and CALIPSO database by our method and the operational product. However, this information is yet given in the section 3, and in the figure 3, where correlation between the operational product and our product is about 0.95 and 0.93 for GLAS and CALIPSO.

Reviewer: 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) 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?) Author: Following our studies and analysis, the first week of measurement (last week of September 2003) of GLAS cannot be fully trusted, and therefore, could be hardly used in this study. Therefore, the author have decide to start the use of the GLAS measurements at the first week of October 2003. To assert the stability of the PDF and the CPDF during this period, we give at the end of our response to the reviewer #2, a full demonstration, using CALIOP and GLAS data, where you can reefer.

SPECIFIC COMMENTS OF THE REVIEWER

Reviewer: page 5270, line 19: change "lidar signal ratio"; to "lidar signal-to-noise ratio"? Author: Reviewer is right. Change has been done in consequences.

Reviewer: page 5270, line 25: remove the second occurrence of the word "impact" Author: Reviewer is right. Deletion of the repeated word has been done.

Reviewer : page 5272, line 2: please add a reference for "New spaceborne backscatter lidar missions". Author: The author is right. This reference is lacking in the article. So, we have added in the text the names of the next ESA and NASA planned space-borne lidar mission.

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Considering the European ESA space agency, the next mission are the lidar ALADIN/ADM-AEOLUS (2010); the lidar ATLID/EARTHCARE (2013) and the A-SCOPE (Advanced Space Carbon and Climate Observation) mission. See http://www.esa.int/export/esaLP/index.html). We give as a reference the proceeding presented at the last ILRC conference :

Ingmann, P,. STraume-Lindner, A.G. and Werh T., 2008 : ESA space-borne lidars in preparation or planned, in: Proceedings, 24th ILRC, edited by: Hardesty, M. and Mayor, S., NCAR, Boulder, CO, 2008. pp. 1109-1110

Considering the NASA space agency, the next mission will be the mission ACE (Aerosol Cloud Ecosystem), which is expected to be launch in the period extending from 2013 to 2016. The corresponding reference is the presentation made by Stephen Volz, with the title "NASA Earth Science Decadal Survey Implementation", which can be found at the following location http://www.veg3dbiomass.org/VolzVeg3Dworkshop.pdf

Reviewer : page 5273, line 15: the symbol sN is used in the text, vs. sB in equation 1 (N for noise, B for background?) Author: The reviewer is right. However, it's seem that on this point, there is a difference between our version and the version of the article given on the website. The variance sigmaN is the right one, and correspond to the variance of the noise.

Reviewer: 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). Author: The area concerned is between 19 and 20 km. Change has been done in the article, by replacing "below 19 and 20 km height" by "between 19 and 20 km height".

Reviewer: 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. Author: The reviewer is right. Median filter has been only used in the case of the LITE data, due to the high vertical resolution of this last (15m). We indicate this restriction in this sentence.

Reviewer: page 5274, line 6: "Two distributions are thus retrieved" Is this procedure automated? How much data would be required to generate truly representative histograms? Author: This procedure can be automated, but have been here applied to 4 CALISPO orbit section. Nevertheless, the obtained results seem to show a great stability considering the value of the threshold to be applied. Over section has been further tested, and the retrieved value of the threshold that has been used give correct results.

Reviewer: page 5274, line 13: change "has been" to "have been"; Author: Reviewer is right. Change has been done in consequences.

Reviewer: page 5274, line 13: page 5274, line 14: after quickly rereading Palm & Spinhirne 1998, I find 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). Author: The reviewer is right. We make the modification and have changed the reference to one of the GLAS ATBD.

Reviewer: 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. Author: The Reviewer is right. We make a mistake. We should mention only about the GLAS prototype algorithm. With our knowledge, and after research for our part, no prototype algorithm has been proposed about LITE data to differentiate between the cloud and the aerosols layers. Indeed, we delete inside the text of the article the mention about the LITE prototype algorithm.

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Reviewer: page 5275, line 10: change ’corrected and navigated’; to ’calibrated and geolocated’ Author: The reviewer is right. Change has been done in consequences.

Reviewer: page 5275, line 26: change ’elaborated’ to ’derived’;? also, ’optical’ is misspelled. Author: The reviewer is right. Change has been done in consequences.

Reviewer: page 5277, line 3: what is actually being compared? Is it ’the cloud classifications’? Or is it the layer detections, prior to classification? Author: We explain in this section how we manage the LITE structure distribution after the layer detection, to differentiate between the cloud and the aerosols structures. After differentiate between this two type of scattering layer, we could then compare the distribution with one obtained with GLAS and CALIOP data.

Reviewer: 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 3. Author: No process is done on the GLAS and CALIOP processing. Contrary to the LITE raw datas, wich are uncalibrated, data are yet calibrated, and doesn’t need other correction. Then, we use exactly the same base to apply the local algorithm, i.e. the database as given by both Science Team of GLAS and CALIPSO.

Reviewer: page 5277, line 18: change ’values has’to ’values have’; Author: Reviewer is right. Change has been done in consequences

Reviewer: page 5278, line 18: what does it mean to be ’better distributed’? Author: The authors want to explain by ’better distributed’that more low level clouds are detected under semi-transparent clouds, with the LITE instrument, than in the case of GLAS and CALIOP instruments. As explained in the article, this effect is directly linked with the SNR of the different instruments. The author is right to say that this sentence is not very clear in the meaning. We have modified this last in consequences: ’where the cloud structures at low and mid levels are better distributed'; has been changed to 'where more low cloud structures are detected'; at the line 18 of the page 5278.

Reviewer: page 5279, line 9: change 'calculate of' to 'calculated for'; Author: Reviewer is right. Change has been done in consequences

Reviewer: page 5280, line 5: should 'David'; be given either a first name or a last name? Author: The Author is right. In this case, David is the family name, and therefore, we now give in the text of the article the full name of this person (Dr. Christine David).

Reviewer: 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) Author: The reviewer is right. The same definition of the altitude is used inside this work. The good description of the three different levels is one corresponding to the (Rossow et al., 1991) reference at the page 5274, line 25, and section 2.1. We have thus rewrite this sentence, in the section 4.2.2, at the page 5280, line 15 as following: 'In the following discussion, low-level clouds are defined as having cloud-top pressures ranging from 1000-680 hPa; middle-level and high-level clouds ranging respectively from 440-680 hPa and 440-50 hPa';

Reviewer: 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 Author: The consistency between the three sets of results, concerning the global cloud fraction, is verified on a global scale, but is not verified on each latitude band (see table 1), with a maximum difference between each of the dataset of 10%. Thus, we couldn't conclude in the text of the article, that, as said by the reviewer, 'the cloud/aerosol optical depths that can be reliably penetrated by the three systems are pretty much the same'.

Reviewer: page 5281, line 13: with respect to the detection of mid-level clouds, the

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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? Author: We have taken in consideration the Liu et al., 2004 publication in our work, and the conclusion of potential misidentification of the cloud/aerosols layer. To assert the quality of our product, the process of the LITE dataset by our algorithm has been visually, carefully and totally checked.

Reviewer: 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? Author: Taking in consideration the table 1, we don't understand this remark, and ask to the reviewer further explanation. Talking in consideration the table 1, this remark is not verified at a global scale (38.6%, 34.7% and 41.1% for respectively LITE, GLAS and CALIOP). This remark is also not verified between 60°S to 20°S (52.2% for LITE, while 33.2% and 51.2% for GLAS and CALIOP) and between 20°S to 30°N (36.0.2% for LITE, while 33.4% and 35.8% for GLAS and CALIOP).

Reviewer: page 5281, line 22: a latitude band from 20°S to 60°S hardly qualifies as "the southern polar latitudes" Author: The Reviewer is right. Two main ideas have been mixed in this paragraph, explaining the misunderstanding of the reviewer. We separate this paragraph in two parts, and rewrite this last to have a better understanding this part.

The previous version:

"However, we find that the southern polar latitudes have a higher proportion of low clouds that may exceed 50% except in the case of GLAS (˜33%). Importance of the high cloud occurrence to the polar latitudes perhaps related to the tightening of the orbit footprints for these latitudes. Some caution must be exercised with the interpretation of high clouds in the polar latitudes. There can be one over-representation of the high

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cloud structures of greater horizontal expansion";

Has been rewritten in the text, in the section 4.2.2. as:

"However, we find that the latitude interval between [60°S; 20°S] have a higher proportion of low clouds that may exceed 50% except in the case of GLAS (˜33%). The importance of the high cloud occurrence to the latitudes greater than 60°S, for GLAS and CALIOP instrument perhaps related to the tightening of the orbit footprints for these latitudes. Some caution must be exercised with the interpretation of high clouds in the polar latitudes. There can be one over-representation of the high cloud structures of greater horizontal expansion."

Reviewer: 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. Author: The author is right. We would have preferred to present such type of comparison between active and passive remote sensors measurement. This work would have comfort our conclusion. However, one of the strong interest of this study is to present a direct comparison (co-localised, and same time), between the two type of measurement. When this article was written, the time-coverage corresponding to the ISCCP cloud product, as given on the ISCCP website, allow only the comparison with the LITE measurements.

This work will make the object of more detailed work, and will be presented in a further article, where the ISCCP and the MODIS cloud product will be used.

Reviewer: page 5282, line 25: ISCCP looks more like 50%, whereas the lidars appear to converge on a value around 40%. Author: The reviewer is right, and we must to recognize this error. We invert the statistic corresponding to LITE and one corresponding to ISCCP. The value corresponding to ISCCP is 47% for clouds layer below the altitude of 3km. The correction has been made in the text of the article in the section 5.1, page 5282, at the line 25.

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Reviewer: page 5283, line 13: what is meant by "the interest of the active instrument" Author: By this sentence, we would like to underline the interest of using data registered onboard active remote sensors, to retrieve the information about the way that overlapping between each cloud layer occur. To clarify this sentence, we re-write this sentence to obtain a better understanding as: "By this way, the interest of using data from active instrument to retrieve the cloud overlap is better highlighted";

Reviewer: page 5286, line 7: change "could seems" to "could seem" Author: Reviewer is right. Change has been done in consequences.

Reviewer: 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. Author: In consideration of the comment of the reviewer, we have modified the colors and the width of each plot of this figure.

Reviewer: 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 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; Author: We have omit to mention in the text that this algorithm of discrimination between cloud and dust layer has been only apply between the ground altitude and the altitude of 8 km. This explain why the faint cirrus cloud in the right corner is correctly detected. The reviewer is right. This remark lack in the text. We add it in the explanation in the section 2.1.

Reviewer: Figure 2: Once again, the contrast between yellow and white is low, hence

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it's difficult to identify isolated patches of aerosol in the lower image. Author: The author is right. We have change the color of the dust from yellow to orange in the figure 2 to increase the contrast. Moreover, we have change the corresponding caption of the figure 2.

Reviewer: Figure 3: What data segments were used to construct these plots? Author: Considering the GLAS data, the segment that were been used to construct this curves are the same that ones used to construct the figure 1. Considering the CALIPSO data, the used data come from the day of the 26 June of 2006, at 20:52 GMT. This information lacking in the caption of the figure, and have indeed been added.

Reviewer: Figure 4: The information content of this diagram might be more readily accessible to a broader audience if PDFs were plotted, instead of CPDFs. Author: The reviewer is right. Authors give now in the corresponding PDF in the figure 4b and 5b, and describe this last in the text of the article.

Reviewer: Also, shouldn't the legend read "Winker, 1998";, and not "Winker et al., 1996";? Author: The author is right. The change has been done in the figure and in the corresponding caption.

Reviewer: 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. Author: The line of the figure 6 has been thickened to answer this difficulty of the plot readability.

Reviewer: 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. Author: The remark of the author is right. However, the resolution of each of the dataset, the quantity of observation, the latitude interval, is different. This explains

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that the number of observation will not be the same. Like proposed by the Reviewer, and as previously done by the author, the colorbar could be scaled in percentage. But we prefer to show the absolute number of cloud layer, as the normalization is not obvious. A normalization on the vertical or the horizontal scale would have destruct the homogeneity on respectively the horizontal and the vertical scale of the distribution, leading to a lost of information on this scale. A pixel normalization has been previously made, to the pixel showing the maximal number of structure, but this normalization is a nonsense, as the distribution is entirely drive by one pixel. Thus, we decide to show the absolute number of the detected layer, and don't make any normalization.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5269, 2008.

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