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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of aerosol and cloud layer structures from the space-borne lidar CALIOP using Seoul National University ground-based lidar" *by* S.-W. Kim et al.

Anonymous Referee #1

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Review of "Validation of aerosol and cloud layer structures from the space-borne lidar CALIOP using Seoul National University ground-based lidar" by S.-W. Kim, S. Berthier, P. Chazette, J.-C. Raut, F. Dulac, and S.-C. Yoon

[general comments] this paper describes a selection of ground-based measurements made using the Seoul National University (SNU) lidar. the SNU measurements are compared to coincident profiles recorded by the space-borne lidar aboard the CALIPSO satellite, with the goal of validating the layer heights and backscatter intensities reported in the publicly available CALIPSO data products.

the goal of the paper is worthy: the authors declare that they will "focus on the magni-

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tude of the lidar backscatter as well as the detectability of the height and thickness of aerosol and cloud layers" (pg. 11209, line 15). since the magnitudes of the attenuated backscatter coefficients reported by CALIPSO are directly related to the accuracy of the calibration constant(s) applied, comparisons of this type represent a critical first step toward validating the CALIPSO level 1 data products. similarly, successful comparisons of the cloud and aerosol layer boundaries detected by the two instruments will help validate some of the most fundamental quantities (i.e., layer base and top altitudes and layer type) reported in the CALIPSO level 2 data products.

while this paper has the potential to make a useful contribution to the laser remote sensing literature, in its present state I believe it is not quite ready for final publication. after reading the article several times, I find that several fairly important questions remain unanswered... and it is my opinion that these should be addressed satisfactorily in the body of the text before the paper is approved for final publication. I've listed my major concerns below:

[specific comments] -a- based on the references provided, this paper appears to be the debut of (perhaps a brand new version of??) the SNU lidar in the peer-reviewed literature. beyond the short recitation of system characteristics provided in the body of this paper, there are no references to additional instrument and/or data analysis descriptions that might be useful in evaluating system performance. the sole reference provided by the authors (Murayama et al., 2001) states that the SNU instrument is ""a micro-pulse lidar (MPL)", operating at a wavelength of "523 nm generated from a Nd:YLF laser". however, in the text of the article, the SNU lidar is described as a two-wavelength, polarization sensitive Nd:YAG system. presumably the SNU system has been rebuilt from the ground up...but when???

for the SNU lidar to be useful in validating the CALIPSO measurements, it should have an established history of high quality measurements reported in the peer-reviewed literature. from the references provided by the authors, it's not clear that such a history actually exists... assuming it does, however, pointers to the relevant literature are re-

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quired. a reference to something along the lines of the material presented in one of the following articles would be ideal:

McGill et al., "The Cloud Physics Lidar: Instrument description and initial measurement results," Applied Optics, 41, pp. 3725-3734 (2002)

Campbell et al., "Full-Time, Eye-Safe Cloud and Aerosol Lidar Observation at Atmospheric Radiation Measurement Program Sites: Instruments and Data Processing", Journal of Atmospheric and Oceanic Technology, 19, pp. 431-442 (2002)

DeCoursey et al., "Recent modifications, enhancements, and measurements with an airborne lidar system", in Laser Radar Technology and Applications, G. Kamerman, Editor, pp.178-188 (1996)

-b- on page 11210, line 16 the authors say that "for the purpose of CALIPSO validation, the SNU lidar has been continuously operated from March, 2006". why then are the comparisons limited to only 6 profiles? certainly these in-depth profile comparisons are useful in their own right...but ideally they would be presented as specific examples from a broader statistical study (e.g., the PDF comparison approach to CALIOP validation mentioned in the Kovacs et al. 2004 article (which they cite later in their paper). with both a daytime pass and a nighttime pass occurring every 16 days (page 11211, line 2), the authors should have been able to accumulate ˜40+ coincidences prior to submitting their paper...so is it really the case that only 15% of these opportunities yielded useful comparisons? if so, a description of why the others were rejected would be in order. if not, some rationale for including only the current examples should be offered.

also, the fortunate geographic location of SNU system would seem to make it ideally suited for comparing and contrasting the performance of the CALIPSO retrieval schemes when operating on (relatively) high SNR nighttime data versus low SNR daytime data. while doing so should not be a prerequisite for final publication, the value of this paper to the CALIPSO data users community would be considerably improved by adding a discussion of day/night performance differences.

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-c- also, the paper could be easily and enormously enhanced by the addition of color coded time-height images of the data acquired by the two different lidars. context is everything, and seeing these would no doubt help the reader better understand (for example) the differences seen between the two profiles shown in the middle and right-hand panels of figure 3.

-d- how is the SNU lidar calibrated? if a molecular normalization is used, what altitude regime is chosen, and how does this choice affect the data acquired BELOW the calibration altitude (e.g., PBL aerosols.) since the authors comment on disparities between the backscatter signal levels measured by the two instruments, the SNU calibration procedures should either be explained in the text, or a suitable reference provided

-e- why was the averaging interval for the CALIPSO level 1 data chosen to be 20 consecutive profiles? above 8.2-km the CALIPSO data is averaged on-board the satellite to a horizontal resolution of 1-km, which is equivalent to three laser pulses. for examining data in the mid-to- upper troposphere (e.g., cirrus clouds) it seems to me that either 18 profiles or 21 profiles would have been a better choice, as then all data from each 1-km on-board average would be equally weighted

-f- referring to figure 2, the authors attribute discrepancies in the attenuated backscatter coefficient profiles to "the combined effects of large signal attenuation and spatial inhomogeneity of PBL aerosols along the CALIPSO track." (pg. 11211, line 24). without the time- height images mentioned above, it's very difficult to know whether spatial inhomogeneity plays any real role...and in the absence of these images, one might very well expect that, contrary to the authors assertions, the aerosol layers would be largely homogeneous over the small spatial (˜6.7-km) and temporal (5 minutes) scales examined in this paper (e.g., as in Anderson et al., "Mesoscale Variations of Tropospheric Aerosols", Journal of the Atmospheric Sciences, 60, pp. 119-136 (2003))

with respect to "large signal attenuation" being the culprit, presumably the authors

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mean the attenuation of the CALIPSO signal from TOA to the top of the PBL? if so, I believe this could be quantified. at the top of the PBL layer shown in the figure, the two-way attenuation of the 532 nm CALIPSO signal due to molecules and ozone is ˜0.8 (a more exact value can be computed from the meteorological data included in the CALIOP level 1 data files). so, is the integrated attenuated backscatter reported in the CALIPSO data products roughly 20% lower than the integrated attenuated backscatter derived from the SNU data?

another question that arises is, could the signal disparities that are seen in the daytime cases be a result of calibration errors in the CALIPSO data? it is now recognized (and has been discussed at recent SPIE and IGARSS meetings) that CALIOP suffers some drift in the value of its 532 nm calibration constant during the course of the daytime portion of each orbit, and this drift is not properly compensated for in the current CALIPSO data processing routines. while I understand that this situation may not have been widely known when the authors first began writing their paper, an evaluation of the effectiveness of the CALIPSO calibration scheme is a proper topic for a validation paper, and is perhaps essential for a paper claiming to "focus on the magnitude of the lidar backscatter" (which can only be ascertained via calibration)

-g- figure 1 does a fine job of showing the tight spatial coincidences between the SNU lidar and CALIPSO. however, the authors are too brief in describing the specifics of the temporal coincidences between their measurements and the CALIPSO data. the only solid info I was able to dig out is in the caption in figure 2, which states that the SNU data was acquired between 4:45 and 4:50 UTC, whereas the CALIPSO data represents a one-second average acquired at 4:50 UTC. was this the standard practice for daytime? (and what about nighttime??) once again, a color-coded time-height image of the SNU data, with the location of the CALIPSO coincident data clearly marked, would greatly improve the readers understanding of how data handling issues affect the characteristics of the profiles being compared. for example, see figures 1 & 3 in McGill et al., "Airborne Validation of Spatial Properties Measured by the CALIPSO Lidar", Journal of

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Geophysical Research, doi:10.1029/2007JD008768 (in press, accepted 16 July 2007; see http://www.agu.org/contents/journals/ViewPapersInPress.do?journalCode=JD)

-h- at the top of page 11212 (lines 1 through 14), I'm confused by the authors' discussion of the "relatively small variations of the CALIOP-derived [beta prime 532] above the boundary aerosol layer". these are attributed, at least in part, to "strong spatial and vertical inhomogeneous distributions of aerosols"...and I wonder: how so? an inspection of the CALIOP browse image from the time period in question (see http://www-calipso.larc.nasa.gov/products/lidar/show_detail.php? browse_date=2006-10-24&orbit_time=04-11-52&page=3&granule_name= CAL_LID_L1_Exp-Launch-V1-08.2006-10-24T04-11-52ZD.hdf&show_wide) shows a fairly bland and uniform PBL aerosol layer, so the solar back- ground should not be changing wildly as a result of spatial variations in the backscatter intensity of the PBL. (and, as demonstrated by the image, this is indeed the case.) and while there are no doubt some stratospheric aerosols in the path of the CALIOP beam (e.g., Thomason et al., "CALIPSO observations of stratospheric aerosols: a preliminary assessment" ACPD, 7, 5595-5615, 2007) I find it highly unlikely that this layer has any significant inhomogeneity on the spatial scales being considered in this comparison. also, with respect to the profile shown in figure 2, multiple scattering seems also to be a highly unlikely explanation for the signal variations in the clear air region above the PBL (unless the authors expect signficant multiple scattering from the molecular atmosphere and/or whatever stratospheric aerosol is present)

[technical corrections] page 11208, line 1: "We present first observationally based..." should be changed to "We present the first observationally based...". also, claiming this article as "the first" could be a bit of a stretch (see for example the McGill validation paper in JGR). these comments apply equally to the text on page 11209, line 9

page 11208, line 7: "case" should be plural (i.e., "cases")

page 11208, line 7: the phrase "multi-aerosol layers" is ambiguous; while I believe

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the authors mean "multiple aerosol layers", it could also be taken to mean repeated instances of individual layers which contain a mixture of aerosol types.

page 11208, line 9: the current text reads "...algorithms of the discrimination of cloud and aerosol as well as of their layer..." the readability of the sentence could be improved with the following change: "...algorithms for the discrimination of cloud and aerosol as well as for the detection of layer..."

page 11209, line 4: "production", not "productions"

page 11209, line 12: eliminate the word "based"

page 11209, line 24: the authors should clarify the fact that while the CALIOP transmitter emits polarized light at both wavelengths, polarization discrimination in the receiver is only done for the 532 nm channel

page 11209, line 26: for a description of the CALIOP lidar specifications, Winker et al. 2004 is a much better reference that the two currently cited (Vaughan et al. 2004 & Winker et al. 2006)

page 11210, line 3: in re "The CALIOP is calibrated..."; a reference to the CALIOP level 1 ATBD would be appropriate here

Hostetler, C. A., Z. Liu, J. Reagan, M. Vaughan, D. Winker, M. Osborn, W. H. Hunt, K. A. Powell, and C. Trepte "CALIOP Algorithm Theoretical Basis Document - Part 1: Lidar Level I ATBD - Calibration and Level 1 Data Products", PC-SCI-201, NASA Langley Research Center, Hampton, VA (2005). (available for download at http://www-calipso.larc.nasa.gov/resources/project_documentation.php)

page 11210, line 15: is there any concern that an altitude of ˜17.5-km is too low to be measuring background light levels? (I'd think this would be especially so for nighttime measurements.)

page 11210, line 26: insert "the", so that the sentence reads "...for the selected 6

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days..."

page 11210, line 27: "The SNU lidar deployed in this study..." How many lidars does SNU have? the way this is phrased makes it sound like there are several (see comment -a- in the "specific comments" section)

page 11210, line 28: change "local-emitted" to "locally emitted"

page 11211, line 3: the Winker et al., 2004 article is not an appropriate reference here, as it has nothing to say about validation strategies (in fact when I do a search for the word "validation" in that article, Adobe Acrobat returns exactly zero occurrences)

page 11211, lines 10 & 11: suggested rewrite of the final sentence in the paragraph: "5-min averaged SNU lidar profiles acquired during the satellite overpass are used for comparison."

page 11211, line 21: some mention should be made of the fact that, for edge detection purposes (i.e., locating layer base and top altitudes), 600 meters of smoothing can introduce significant distortions in the apparent base and top heights of cirrus clouds.

page 11211, line 26: insert the word "structure" (or something similar), so that the sentence reads "...showed almost identical structure (not shown)."

page 11212, line 5: change "vertical" to "vertically"

page 11212, lines 13 & 14: the authors state that a "somewhat little enhanced signal between 8 and 12 km a.m.s.l. is found." while they could well be correct, I'd like to know what kinds of comparisons they base this statement on.

page 11212, line 18: change "is well corresponding" to "corresponds well"

page 11212, line 19: can a reference be provided for the "maximum gradient" method?

page 11212, line 26: insert "the", so that the text reads "Contrary to the aerosol layer products..."

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page 11212, line 26: I'm afraid I don't understand exactly what the authors mean by "the 6 profiles of the top and bottom heights of clouds products (CALIOP level 2 data 1km horizontal grid data) were used"

page 11213, line 18: I'd think that spatial/temporal mismatching of the averaged profiles was a more likely cause for the disparity in the cirrus signal for the February 21 case. have the authors looked at this as a possible explanation?

page 11213, lines 19-23: this example is not strong enough to make me buy into the idea that ground-based and space-based systems are complimentary. the later examples (i.e., the totally attenuating clouds shown in figure 4) are thoroughly convincing...but at this stage, the assertion seems a bit premature (the fact that the signals attenuate differently due to their different pointing positions is not an argument for complimentarity (is that even a word???) if both systems successfully sound the full atmospheric column)

page 11214, line 6: rewrite: "...can be explained by a mismatching of the sampled airmasses..."

page 11214, line 10: change "proof" to either "prove" or "provide proof"; also, comment made earlier for page 11208, line 9 applies here as well

page 11214, line 14: change "were" to "was"

page 11214, line 21: insert "the", so that the text is "strength of the upper cirrus"

page 11214, line 22: insert "the", so that the text is "heights of the cloud layer"

page 11214, line 23: change "is" to "are"

page 11214, line 24: change "with" to "to"

page 11214, line 25: insert "light"; "Laser light emitted..."

page 11214, line 28: change "vertical" to "vertically"

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page 11215, line 1: change "of discrepancy" to (for example) "for the causes of discrepancies"

page 11215, line 1: I suspect that the linear scale used in figure 5 somewhat obscures the magnitude of the variations in the lower part of the figure. a log scale for the attenuated backscatter coefficients would probably be more revealing, and do a better job of making the authors' point.

page 11215, line 7: the authors say that "The gap of cloud base heights between two lidar measurements was also consistently apparent in Fig. 5." what averaging resolution was used to determine the SNU lidar layer boundaries shown in this image? does the SNR of the SNU data permit it to be processed at single shot resolution? if so, showing single shot cloud base retrievals would add a significant amount of interesting information to this figure.

page 11216, line 1: see previous comment for page 11208, line 9

page 11216, line 2: because more than one comparison was made, the text here should read "In cases of aerosol layers..."

in closing, I find it interesting that (based on the listing published at http://www-calipso.larc.nasa.gov/contacts/) no member of the CALIPSO science team is involved in this publication. on the one hand, the fact that no science team members participated means that this is a genuinely independent assessment of the quality of the CALIPSO measurements. (and in that regard, the authors' conclusions – e.g., pg. 11216, line 1 – should be very gratifying to the CALIPSO team.) on the other hand, having a science team member on board may have been very beneficial in analyzing the signal level discrepancies discussed in point -f- of the "specific comments" above.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11207, 2007.

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