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Interactive Comment

Interactive comment on "Airborne dust distributions over the Tibetan Plateau and surrounding areas derived from the first year of CALIPSO lidar observations" *by* Zhaoyan Liu et al.

Zhaoyan Liu et al.

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We appreciate the reviewer 1's insightful comments. We will revise the manuscript based on the reviewer 1's comments. Here we respond to some of the reviewer 1's comments to clarify several things. Our responses are provided below, embedded after each comment.

Anonymous Referee 1 Received and published: 4 April 2008

2) SPECIFIC COMMENTS 1. The CALIOP profiles: for the 7 considered locations, the lidar profiles should be showed to better address the claimed differences among pollution and dust signatures. Actually it is not clear why the volume depolarization ratio (VDR) has been considered, instead of the particulate depolarization ratio (PDR),

being the last one the intensive physical quantity of dust (and pollution) particles. As an example, in Fig. 1 left-middle plot, the 'd+p' region could be an artefact, becoming only 'd' if particulate depolarization is used? The authors should very well argument the choice. Moreover should be very useful for the reader to see what MODIS saw on the two CALYPSO trajectories showed in Fig. 2. I would suggest to add the MODIS data.

Author Responses: First, as to why the VDR rather than PDR has been used in our analysis to identify dust aerosol, there are two reasons. One is that, as this manuscript was being written, PDR that has been promised for the level 2 data processing was not available. The other one, and most important one, is that the dust aerosol can be identified very well by VDR using a threshold of 0.06 which corresponds to a visibility in access of 60 km as demonstrated in the manuscript. The threshold of 0.06 was determined from the histogram distributions of VDR for different typical aerosol types (dust, smoke, continental and marine) measured by CALIOP at different locations where these aerosol types normally present. For this reason, the occurrence frequency of dust aerosols was derived from level 1 data; no lidar signal inversion is required, which is a merit for the use of the VDR. A new plot of the histogram distributions of VDR will be added in our revision to help readers to better understand our data processing.

Second, polluted aerosol can be identified for optically dense cases, based on the large wavelength dependence of extinction due to black carbon or fine mode particles. For smoke the attenuated backscatter color ratio, which is a ratio of attenuated backscatters at 1064 nm and 532 nm, is exponentially proportional to the difference of extinction coefficients at 532 nm and 1064 nm (we will add an equation describing the relation between the color ratio and extinction coefficients). Because the extinction coefficient at 532 nm is much larger than that at 1064 nm (3 5 times) for smoke, i.e., the reduction of lidar signal due to attenuation is much faster at 532 nm than 1064 nm, the attenuated backscatter color ratio will grow rapidly as a function of increasing extinction coefficient or path length. By checking the gradient of the color ratio we may identify pollution. In practice, however, the gradient is sensitive to signal noise and therefore it is hard to use the gradient of color ratio to identify pollution based on single profile. But, it is possible to look at the layer averaged color ratio or look at browse images to identify pollution. This only works for dense smoke cases. In general, when VDR is high, the layer is likely dust dominant; when VDR is low and the color ratio is high, the layer is likely pollution dominant; and when VDR is medium and the color raio is high, the layer could be a mixture. However, if the pollution layer is optically thin, it is hard to identify it based solely on the color ratio. The histogram distributions of color ratio for different type aerosols measured by CALIOP will also be added in our revision.

We note that, however, in our analysis to derive the dust distributions, only the layer averaged VDR is used. In this paper, the color ratio is used only in a qualitative way for the particular cases shown in Figure 1 for a case study.

2. The Volume Depolarization Ratio threshold of 0.06: considering the Appendix A, where did the (A3) formula come from? I could suggest 'Cairo et al., Comparison of various linear depolarization parameters measured by lidar, Applied Optics, 38, 4425-4432, 1999.' Moreover in such a formula, R appears dramatically depending from PDR, that reasonably ranges between 0.25 and 0.40 for desert dusts: this means that if you try to calculate R with a VDR of 0.2, the resulting R ranges between 2.4 and 6.0 according to the PDR value assumed, that is a very wide uncertainty. Why the authors don't use the classical method of calibration on an aerosol free atmosphere correcting for the extinction? I could guess a better estimate of R would be obtained. The authors should argument also this choice.

Author Responses: As pointed out by the reviewer and presented in the Appendix A of this manuscript, computation of R using A3 is dependent dramatically on PDR. Large uncertainties could be produced, if one would use (A3) to derive R or extinction coefficient from dust measurement data where the dust depolarization ratio vary rapidly over a large range.

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We note that, however, we do not use (A3) in our data processing to derive the dust distributions, therefore this is not an issue in the current study. As we presented in the Appendix A, we only use (A3) to estimate the possible range of horizontal visibility corresponding to a VDR threshold of 0.06 (note, as mentioned earlier, this value was determined from CALIOP measurements), for the range of reported dust depolarization ratios. We want to give readers an idea about what is the range of dust events that we have selected in deriving the dust distributions. For this purpose, (A3) is the right formula to use.

Cairo et al.'s paper is a good one to cite.

3. Statistical analysis of lidar data: in paragraph 3.2 the method used to obtain the frequency distributions should be better outlined: i.e. the number of total/used profiles, explaining if the cloud free constrain could introduce a seasonal behaviour in the statistics, if the daytime profiles were used in the statistics and if their S/N ratios allow the detection of pollution events with low VDR. It could be interesting (even if, I understand, it might be slightly out of the main target of the paper) to show also the total aerosol distribution and the pollution distribution alone over the TP area: my feeling is that it could really help the reader to understand the great improvement in the studies given by a double polarization/double wavelength lidar data set from space, allowing as well a comparison with the aerosol optical depth as measured by other satellite sensors (see next point 4).

Author Responses: Agree that we should discuss more about the statistical analysis. A cloud-free /total profiles ratio map will be added in the revision. However, it is hard to quantify the possible effect of cloud screening. If the cloud occurrence is not largely correlated to dust occurrence, the cloud screening effect should be small. The exclusion of daytime measurement is considered to be small. Although the emission of dusts tends to be larger during daytime than nighttime, this should have only a small effect on dust occurrence frequency, particularly when floating dusts dominate; for the dust distributions derived in this paper, the number of floating dust events is dominant,

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particularly in non-source regions. Nevertheless, the dust distributions appear to be reasonable.

As to the inclusion of pollution distribution in the revision suggested by the reviewer, we agree that it would be interesting to derive pollution distribution. However, we are afraid that we are unable to follow the comment, for two reasons: 1) deriving pollution distribution is out of the scope of this paper, as the reviewer has realized; 2) the methods used in this paper do not allow us to do so, (as mentioned in our response to 1, the use of attenuated backscatter color ratio can only allow us to identify relatively dense pollution layers). More quantitative method needs to be developed to derive pollution distribution. The methods developed by Shimizu et al. [2004, JGR], Sugimoto et al. [2006, Appl. Opt.] and Nishizawa et al. [2007, JGR] may be modified and applied to the CALIOP measurement to partition dust and pollution aerosols. This can be one topic of our research in future.

4. Exclusion/Use of MODIS and MISR data: both data set are available for the period. Actually on the MISR web page, the seasonally averaged data (years 2006-2007) are already present for a quick comparison with the CALIOP averages. A difference between the MODIS/MISR aerosol optical thickness and the CALIOP one is foreseeable, again allowing to explain the reasons of such differences and to show the great improvement given by CALIOP data set.

Author Responses: It is a good idea to compare the CALIOP dust occurrence distributions with other measurements. In fact, we have compared our derived CALIOP dust occurrence distributions with seasonal aerosol index (AI) maps from the OMI measurement and MODIS AOD, though we did not present the OMI results in the manuscript. As expected, the comparison showed some similarity while some differences in the distribution patterns. We will add one comparison to the revision.

Very recently, the beta version of extinction retrieval (from which aerosol optical depth is derivable) has become available. Comparison studies of CALIOP aerosol optical

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depth with MODIS are being conducted by the CALIPSO Lidar Science Working Group to validate the CALIOP extinction retrieval.

Page 3, 8th line up. ’Dust is a major component of....’ Please reference the statement.

Page 5, 11th line. ’The depolarization ratio of dust is high due to the nonsphericity ..’: ’and the large size’ should be added. I appreciated the work of Murayama et al., but on this specific argument more ’robust’ references could be easily found, please add or substitute.

Page 5, 21st line. In Qian et al., the limit of 10 km for the floating dust visibility seems to me more a lower limit than an upper limit, so I would change ’(<10 km)’ in ’(about 10 km or above)’.

Author Responses: A limit of <10 km appears to be defined by Chinese Central Meteorological Bureau, also based on other articles by Chinese Scientists.

Page 6, 14th line up. The desert dust hygroscopicity should be cited.

Author Responses: Very good suggestion and we will do accordingly in the revision.

Page 6, 23rd line up. ’the ACR will not be vertically uniform’. Please reference the statement. And are the CALIOP measurements fine enough to get such behaviour?

Author Responses: This statement was based primarily on the experience gained from the CALIOP measurements. We will try to find a reference but we doubt that we will find one. However, we will describe more about this in the revision. As mentioned in our responses to 1, a single profile is pretty noisy.

Page 8, 1st line up. Please refer to Point 3 above.

Page 8, 2nd line. The decimal in the altitude ranges is not useful. Please use integers

Page 11, 19th line. Use ’at altitudes above 2 km’ instead of ’at higher altitudes’

Page 12, 3rd line up, It is unclear why you expect differences between CALIOP and

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HYSPLIT. Moreover the observed differences are not clearly explained.

Author Responses: The simulation using HYSPLIT in this paper is a simulation for air mass transport from three selected points in the source regions. We expected this HYSPIT simulation would (and actually did) provide useful information about the dust transport pathways from these sources. However, we would not expect to see a distribution simulated by HYSPLIT the same as that observed by CALIOP, primarily for two reasons. 1) The sources in our HYSPLIT simulation are only three 'points' selected in the sources. 2) The dust emission at these three points was assumed implicitly to be continuous and invariant over the simulation period. We will revise the paper by adding more explanation.

Fig. 4. On the plots, I don't understand how it is possible to have dust distributed over the TP at altitudes of 0-2 km and 2-4 km.

Author Responses: The altitudes in Fig.4 are related to the ground level. We will convert the plot to altitudes above the mean sea level to make consistency with Fig.3.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5957, 2008.