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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

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1) GENERAL COMMENTS

This paper gives an overview of dust distribution over the Tibetan Plateau (TP) area. Using the first year set of lidar measurements from CALIPSO satellite, the paper presents a unprecedented seasonally and altitude resolved analysis of the dust sources around TP and attempts to identify the most likely transport routes for dust. In addition HYSPLIT trajectory model simulations during spring season were conducted to confirm the source regions and the observed dust transport patterns.

The paper appears appropriate to ACP and the observations are quite interesting. Introducing such new satellite data set the authors reach conclusions substantial on

the dust transport mechanisms in the TP area. The argumentation and the elaboration of the results in the manuscript appears quite well structured.

Anyway in my opinion some more work has to be done to support the publication on ACP. Particularly some lacks arise: on the methods used to obtain the lidar variables (some details and explanations of these in the paper would be useful to the reader); on the statistical analysis for the occurrence distributions of dusts during the year; on the (excluded) comparison with other satellite passive sensors like MODIS and MISR (Multi-angle Imaging Spectro Radiometer).

Thus also the scientific content of the paper appears a bit weak.

Aside from the science, there are some typos and missing references that should be fixed (see Specific and Technical Comments).

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.

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

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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.

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).

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.

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.

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Interactive Comment

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)'.

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

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?

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 HYSPLIT. Moreover the observed differences are not clearly explained.

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.

3) TECHNICAL COMMENTS

Fig. 3. On the left axis, use integer for the altitude ranges. The black contours in the other panels are invisible (on my print), please make them bolder.

Fig. 4. On the left axis, use integer for the altitude ranges. For a better comparison with Fig. 3 please add the higher range (8-10 km) contours also in this plot. Please add on the maps clearly the locations where the simulation start. As Fig3, 'The bold contour depicts...', the contour is unclear on my print, please make it bolder.

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