Comments on "Planetary boundary layer height from CALIOP compared to radiosonde over China"

General Comments

The planetary boundary layer height (PBLH) is an important length scale in weather, climate and air pollution models. The CALIOP-derived PBLHs can construct the PBLH climatology on a global scale. The problem is that the validity of CALIOP-derived PBLH should be examined and the uncertainties of CALIOP-derived PBLH should be known. In this paper, the authors compared the CALIOP-derived PBLH to the radiosonde-derived PBLH in China. The results suggest that they agree very well. The authors also analyzed the difference in the PBLHs derived from the two methods, and showed the spatial distribution of deviations. The results in this paper can help to understand the applicability of CALIOP-derived PBLH in China, and provide the basic information for further investigations. However, some details of the dataset should be further specified, and the English writing should be further improved. Therefore, I recommend the manuscript for publication in ACP, pending minor revisions.

Specific Comments

1. The author declare that the method of Sawyer and Li (2013) was used in this study (in page 6 line 9-10). I suggest that the authors should give a concise introduction of this method, so that the readers can understand how the PBLH is derived from CALIOP in this paper rather than the cited paper. Is this method also applied to the radiosonde data to derive the PBLH? Because the measurement time is almost at noon, the potential temperature profile should exhibit the typical structure of convective BL. Thus the method of maximum potential temperature gradient is suitable for determining the PBLH. Why not use the maximum gradient method? The authors should explain the reason.
2. The derived PBLH should be the height above the ground. However, shown in Fig. 2, the derived PBLH is above the sea level. Is the terrain height derived from CALIPSO or obtained from other data source? The authors should specify this issue. As shown in Fig. 2, the terrain surface is not very clear in some places.
3. In page 9 lines 3-4, the authors state "Note that over regions where BL is not convective the retrieved values are not representative of the PBLH (Liu and Liang, 2010)". Also in this section (Section 2.3), the authors describe the method how to eliminate the effects of clouds on the CALIOP-derived PBLH. In other words, the CALIOP data in clear days are used to derive the PBLH, and the BL should be convective. Moreover, the passing time of CALIPSO is 13:30 BJT. Thus it can be expected that the PBLH at this time is not very low. However, Table 1 shows that the minimum PBLHs in different seasons are 0.2-0.4 km. I think these values are unbelievable. On the other hand, Table 1 show that the maximum PBLHs in different seasons are 4-6 km with the largest value in winter. I think these values

are also unbelievable. It is likely that uncertainties are introduced in the CALIOP-derived PBLH. Then the problem, to what extent the CALIOP-derived PBLH over China is reasonable, arises. I suggest the author discuss this problem and provide additional information about the statistics of the CALIOP-derived PBLH. For example, by setting the reasonable range of PBLH based on the up-to-date knowledge, the percentage of the derived PBLHs that are in this range can be calculated and compared.

4. For the title of Table 1, "seasonal mean" is not accurate. I think, the maximum PBLH, as well as the minimum PBLH, is not the seasonal mean. Maybe "Statistics of the CALIOP-derived PBLH in different seasons" is more accurate. "Standard deviation PBLH" should be "Standard deviation of PBLH". Moreover, the authors should tell the readers how to determine/calculate the values in the table. Is the maximum/minimum PBLH determined as the maximum/minimum value of one grid in the duration or as the average of the maximum/minimum values at every grid in China? Is the standard deviation calculated at every grid and then averaged in China or calculated directly using all the data?
5. Following above question, Fig. 8 shows that the CALIOP-derived PBLH ranges from 1.2 km to 2.4 km. But the statistics in Table 1 show that the CALIOP-derived PBLH varies in a very large range. How many data are not considered in Fig. 8? The authors should specify this issue in the text or in the figure caption.
6. The authors declare in the abstract "The CALIOP observations belonging to Scenario 2 were found to be better for comparison with radiosonde-derived PBLH, owing to smaller difference between them". Similar statements are found in the conclusion section. However, Fig. 7 shows that the mean difference for Scenario 3 is the smallest. What is the solid evidence for this conclusion?

Technical Corrections

1) The grammatical errors should be corrected (Just some are listed here. The author should thoroughly check for simple typos and grammatical errors). For example,
   Page 2 line 1, "for comparison with" should be "in comparison with".
   Page 2 line 2, "at early summer afternoon" should be "in early summer afternoon".
   Page 3 line 20, "the fact the number" should be "the fact that the number".
   Page 4 line 22, "are" should be "is".
   Page 6 line 9, "this methods" should be "this method".
   Page 8 line 7, "in combination with and" should be "in combination with".
2) Fig. 2, at the top of this figure the times "05:33:17" and "05:47:14" should be the local times "13:33:17" and "13:47:14".
3) Fig. 7, the value of mean difference between the CALIOP- and radiosonde-derived PBLHs in each panel (0.17km, 0.22km, 0.17km and 0.15km respectively). But the figure shows that the difference for a single site is either positive or negative (denoted by different colours). How to calculate the mean value, directly or by the absolute values? I guess by absolute values. Therefore the absolute value sign should be added to ΔPBLH.