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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Continuous monitoring of the boundary-layer top with lidar" *by* H. Baars et al.

Anonymous Referee #2

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The paper addresses the provision of the height of the planetary boundary layer (BLH) from lidar signals. It was demonstrated by several authors before that lidar is very well suited to determine this quantity by using aerosols as tracer. So it is not surprising that Baars et al. could demonstrate that this can also be done on a routine basis by running their fully automated system Polly (this is more a technical issue, not a scientific one). As a consequence, in this paper they focus on comparisons of different approaches to derive the BLH in order to find the most reliable and robust algorithm to evaluate the lidar signals automatically. They conclude that a modification of the wavelet covariance technique (WCT) does the best job. Based on this finding, they present statistical analyses (over one year) of the annual cycle of the BLH, mean heights, mean and extreme growth rates etc. Their experimental results are compared to model results

(COSMO) as well. They reveal discrepancies of the order of up to a few hundred meters. This stresses the need for (continuous) measurements.

Overall, the paper is relevant, offers new aspects, is well written and clearly structured. However, I would appreciate a better coverage of the following issues.

Some specific comments:

The main part of the paper deals with the discussion of the potential of different approaches how to find the BLH from the lidar data. This implies that the truth (or "something similar") must be known to be able to select the best solution. It is not very clear how this is done: it seems that the decision is based on visual inspections of the wind lidar data and/or the time-height cross sections of the attenuated backscatter, i.e., more or less on plausibility.

A detailed discussion of the potential of the different lidar approaches would be desirable; the authors present two cases studies and then only state (Sec. 5.2) that in the majority of cases the WCT was the best (which criterion, see above).

The authors should also comment in more detail why they omit nighttime measurements. For the reader this is surprising as one assumes that lidars work better during night than during daytime. An extension to nighttime would considerably improve the value of the data set.

A final point, that remains open to a certain extent is the problem of layered aerosol structures and smooth transitions of the BL to the FT. From my experience (at my site) I expect that in 30-50% of the cases such "difficult" situations occur; so I would expect a much lower number of successful identifications of BLH as indicated in Fig. 12 (including nighttime!)

A few further comments:

The minimum ranges of Polly and WiLi are not consistent throughout the text and the figures (200/250m) or (400/500m).

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Interactive Comment

Polly is stated to be "small and compact"! From Fig. 1 I find a height of 1.8 m; is this correct? What is the mass? What about the required infrastructure? Please comment on this.

Page 10754, line 20 ff: This paragraph sounds like a "commercial" and should be omitted (it is neither relevant in this context nor the statements are proved in the paper).

Page 10756: the forth method is not illustrated in one of the panels but all the other methods are. If possible (with not too much effort), this should be added.

Page 10758: This section starts with the outcome of the following; maybe it is better to shift it to the end of the section.

Page 10763: When discussing the COSMO-retrieval, it would be nice to mention (again), that it is based on the Ri-Number (isn't it?)

Page 10765, line11: Is this comparison not a little bit unfair. If the authors do it on a 2-hour basis, the agreement is better.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10749, 2008.