Atmos. Chem. Phys. Discuss., 8, S5849–S5857, 2008
www.atmos-chem-phys-discuss.net/8/S5849/2008/

Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

H. Baars et al.

Received and published: 12 August 2008

Please find below our reply (in italic) to the comments of the reviewer (in bold):

The Referee: A problem with the paper is the lack of comprehensive comparisons among the various techniques of finding PBL heights from the lidar data, as well as comprehensive comparisons of any of these techniques with the other methods of determining PBL height from the other datasets presented. These other datasets include data from a vertically pointing Doppler wind lidar, and radiosonde profiles of temperature and relative humidity.

Our reply: The goal of the paper is NOT to repeat all the efforts (comparisons with other techniques) that are already done in the papers cited in the introduction. The goal is to go a step forward, and to find out which of the available lidar techniques is the best *(accurate and most robust) from the point of view of routine, automated monitoring of the BL height with lidar.*

There are three case studies that show comparisons among the various lidar techniques as well as with these other datasets, but this appears to have been the extent of such comparisons. There are no results presented or discussed that show how these techniques would have compared using data from an entire year.

The cases are not the only three we have. We conducted 8 combined 24-48 hour Doppler lidar, Polly lidar (plus EARLINET Raman lidar) observations, and we launched a lot of own radiosondes during these sessions at the lidar site.

To keep the paper short (and this must be always a rather important constraint), we selected three (or better four) cases to explain our key findings. Thus the cases shown must be regarded as DEMONSTRATION CASES. They are selected to show the potential(advantages and limits) of the different lidar techniques at different weather and aerosol conditions.

However, the reviewers are right, we obviously did not show clearly enough the advantage of the WCT method in these cases. We improved this. Now we show in Figures (9,10) also the time series of the BL top obtained with the 5-min variance method (Figure 9) and the gradient method (Figure 11). Now it should be obvious to everyone, why we selected the WCT technique. Be sure, we did all the error and sensitivity studies recommended by the reviewers, and applied all techniques available to the entire data set, but the main result can be summarized in these three cases. We would overload the paper if we would show more(and certainly more complicated) plots indicating how the different methods may fail. We want to attract the non-lidar community so we want to show easy-to-follow results only. And because the amount of information in the paper is already very large (three different topics: methods, case

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studies, statistical findings, seasonal cycle, growth rates, COSMO comparison) we feel unable to further blow up the contents.

The paper does indicate that the Wavelet Covariance Technique (WCT) does appear to provide the best results, but it apparently does so using only the three examples. If in fact only these three cases were used to evaluate the various techniques, then the conclusion could likely be premature. However, if more cases were used for this evaluation, then this should be clearly discussed in the paper and the results presented in tables, graphs, etc.

As mentioned above, we improved the figures to show more clearly why the WCT technique is the best. We disagree a bit that we need more cases to show our key findings. Things become not better, when they are already bad in just one example.

Again: We applied the different methods to the entire one-year data set, and the main message of this study is then discussed based on the three demonstration cases. This is the usual way we always present our results. Should we show height time displays for 50-100 days? Three cases for three different meteorological conditions must be enough, to convince the reader that lidar WCT is useful (and at the same time to avoid overloading of the reader).

Similarly, were the other datasets (Doppler lidar and radiosonde profiles) used to evaluate the various lidar techniques only for these three cases?

More or less, yes! To be clear, we did not run the Doppler lidar or launched radiosondes to check the reliability of the lidar BL methods. As mentioned it was not the aim of the paper to repeat work (comparisons with other techniques). We did these measurements to obtain complementary information in the description of the meteorological state of the BL. By the way, in our paper, Engelmann et al. (JAOTech, August 2008 issue, is cited in the paper) we discuss the context of vertical winds in the PBL up to the top of the PBL and the vertical aerosol fluxes. So, aim of the present ACP contribu-

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tion is not to show in which way a Doppler lidar can be used to determine BL top. The vertical wind data are only presented to indicate the period of significant convection. Radiosonde-lidar intercomparisons have also extensively be done (see references in the in the introduction). We will not repeat this. But again, we did comparisons – BL top heights from routine radiosondes launched by the German weather service at Oppin (30 km northwest and thus usually upwind of Leipzig, but unfortunately not longer launched) versus lidar-derived BL tops – and one plot showing the good agreement is given in Mattis et al., JGR, 2008, press.

If (hopefully) more cases were examined, then the results from these cases should be presented (perhaps as regression comparisons) and discussed in the paper. Therefore, the paper apparently relies apparently only on these three cases to: 1) indicate that the WCT technique provides the best method to derive PBL among the lidar techniques, and 2) that this technique provides PBL estimates that are as accurate, if not more accurate, than other datasets. This is insufficient. It would have been particularly instructive and convincing had the paper shown comprehensive comparison among the lidar techniques (or at least the WCT technique) and PBL heights retrieved from radiosondes.

See our argumentation above. However, as a consequence of the criticism we improved the plots showing the three case studies. More comparisons among the lidar techniques are presented in this way.

Details:

1. (page 10751, line 20) In order to measure low PBL tops, a lidar system must also have very high vertical resolution, much better than the 37.5 m resolution of the POLLY system. As explained later in the paper, the WCT would seem to require several data bins below (and above?) the PBL height.

For BL tops > 500 m height, 37.5m resolution is sufficient, to our opinion. Sure, WCT

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needs several data points, but to identify the BL top with 37.5m resolution.

2. (page 10753, last line) Retrieval of nighttime PBL heights is not necessarily easy even if the scattering ratio is used instead of the elastically backscattered 532 nm signal. Nighttime PBL heights would be considerably lower than the daytime heights; consequently, the 37.5 m vertical resolution would likely be too large to represent well the fine scale structure often present at nighttime. Furthermore, the WCT technique would appear to require several data bins below (and above?) the PBL height. Also, elevated aerosol layers just above the nocturnal PBL are fairly common and would increase the difficulty in retrieving PBL height at night.

We agree (and changed the text accordingly). The laser receiver overlap configuration must be changed (complete overlap must be reached at, e.g, 100m), and the vertical resolution must be about 7.5m. We state that now clearly.

We do not think that the residual layer (or other lofted layers) above the nighttime PBL will disturb the WCT analysis. Nighttime BL tops are very sharp, as we know from our nighttime EARLINET lidar observations with the near-field telescope (10cm telescope), the signal-to noise ratio is much higher at nighttime, and the BL tops are at much lower altitudes.

3. (page 10754, line 4) The statement about large data gaps caused by potential laser damage is not clear. Does this mean the laser only operated between minutes 8-13 of each hour for most days? If so, how many days did the lidar operate more frequently than this? If not, please clarify this statement.

On 41 days, we conducted many-hour EARLINET and CALIPSO observations and the eight case studies.. However, in the statictics we only took the data for minute 8-13 of each hour, even in cases with long measurement periods.

4. (page 10756, line 6) derivation should be derivative.

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Is changed!

5. (page 10757, section 3.1) Why was the gradient-Richardson-number scheme used to determine PBL height rather than examining gradients in virtual temperature, virtual potential temperature, and/or water vapor mixing ratio? It would seem that this would have made it easier to compare COSMO PBL heights with PBL heights derived from radiosondes.

BL top is influenced by thermodynamics and dynamics (wind shear). The Richardson number scheme considers wind shear. The wind field is modelled so that COSMO has this information.

6. (page 10759, last line) How was it determined that the optimum value for a is equal to the depth of the transition zone? Can some results be presented and discussed to show how this was found?

To be short and to avoid discussion in the text, we add the reference Brooks 2003 here, he discusses this issue.

7. (page 10761, line 7) How was it determined that a threshold value of 0.05 was found to be sufficient to identify the BL depth? Can some results be presented and discussed to show how this was found?

We expand the discussion a bit in the text. We now state that we used threshold values from 0.02 to 0.15, and found that values of 0.05-0.06 are most appropriate. Values of < 0.04 often lead to unrealistic fluctuations in the Bl top height, for values > 0.08, many BL tops are missing (not detected). This is now mentioned in the text.

8. (page 10763, line 22) Earlier in the paper, and in figures (7,9,10, etc.), the minimum measurement height was reported to be 200 meters. However, in this paragraph, the minimum measurement height appears to be reported as 500 m. Which is correct? What is the minimum PBL height that can be found using the WCT technique? The minimum PBL height that could be found from the WCT

method would seem to be a few (or several?) altitude bins above the minimum measurement height. What is this relationship?

We have clarified this in the text. In the instrument section we now state that the laser-beam receiver-field-of-view overlap is completed at 500-700m height. The minimum height for layer detection from the range-corrected signal profiles is 200m. The WCT method provides reliable data at heights above 500m. There is no contradiction.

9. (page 10764, line 4) This statement seems to indicate that radiosondes from German Meteorological Service are made all around Leipzig, but not in Leipzig. However, the discussion regarding the radiosonde data shown in Figure 8 seems to imply that the radiosonde was launched at the Leipzig site. Was the radiosonde corresponding to the data shown in Figure 8 launched in Leipzig? If not, where?

We improved Figure 8 (including figure caption) , accordingly. The three case studies show profiles observed with our own radiosondes that were launched at Leipzig (at the lidar site).

10. (page 10767, line 11) observation should be observations

is changed.

11. (page 10768, section 6.1) How would the comparison between the PBL heights found from the WCT technique and the COSMO model vary as a function of time after model initialization? This would help address the question of whether the COSMO low PBL height bias is due simply to the infrequent initialization or whether there are other issues (e.g. spatial sampling, model physics, etc.) associated with the model that lead to this bias.

We compared the PBL-depth of COSMO and Polly for 5 certain times (10,11,12,13,14 UTC). COSMO is initialized at 00 and 12 UTC. Surprisingly, we could not find strong evidence that the lidar-model comparison is correlated to initialization. An increase

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of the slope at 12 UTC was accompanied by a decrease in the quadratic correlation coefficient R2. No clear trend was found comparing the linear fits at the 5 different times. Infrequent model initializations therefore seem not to be a major source for the bias.

12. (Figures) Figures such as Figure 7, 9, 10 and others like them are small and difficult to read. It is difficult to distinguish the PBL heights from the color images shown in these Figures. It would be helpful if the color scale range was expanded so that the PBL height could be more easily identified in these color images. Also, it is very difficult to identify the PBL height from the color WiLi images.

We did already our best in this respect. We are convinced that the plots will be fine when they are enlarged and fit to the one-page size

13. (Figures) Reading through the text, it would appear that PBL height is determined from the WiLi data when the wind speed goes to zero. Is this correct? If so, it is not easy to see these heights in the color WiLi images. The images should be expanded and/or the color scale should be changed to more easily identify the PBL height.

WiLi is not used to determine PBL height. It is correct that we use the vertical wind data to identify the period during which the BL is convectively active, and when the wind speed is almost zero in the late afternoon, we know that we no longer determine the BL top height, then we detect the residual layer height.

Again, we are sure that the figures will be sufficiently large enough after enlargement..

14. (Figure 8) This figure caption should indicate the date and location of these observations.

Yes, we improved the figure and caption accordingly.

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