Atmos. Chem. Phys. Discuss., 10, C5586–C5594, 2010
www.atmos-chem-phys-discuss.net/10/C5586/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Updraft and downdraft characterization with Doppler lidar: cloud-free versus cumuli-topped mixed-layer" *by* A. Ansmann et al.

A. Ansmann et al.

albert@tropos.de

Received and published: 22 July 2010

Updraft and downdraft characterization with Doppler lidar: cloud-free versus cumuli-topped mixed-layer

A. Ansmann, J. Fruntke, and R. Engelmann

Authors response to comments:

We thank the two referees for their valuable and fruitful input and recommendations. Many thanks to reviewer #1 who opened the door to recent and very interesting modelling literature! The extended literature research and study than took several weeks before we could start to revise the paper.

Before we answer the comments step by step let us begin with an overview of all essential improvements.

Introduction (section 1): Relevant modelling literature (of the last 10 years) is now included.

All in all we included 16 new references (sections 1-3) : Angevine, Cotton et al. , Dabas et al., Frehlich, Heus et al., Jakob and Siebesma, Patton et al., Soares et al., Spindler et al., van Heewaarden and Vila-Guerau de Arellano, Verzijlbergh et al., Vila-Guerau de Arellano et al., Wendisch et al., Wilde et al., Wulfmeyer et al., Yu et al.

Experiment (section 2): The impact of signal noise on the uncertainty in the determined vertical wind data sets is now discussed.

Results (section 3): a) Chirp effect and potential consequences are discussed. b) We provide surface forcing information, c) expanded the discussion to clearly indicate differences (yes/no) between clear and cloud-topped ABL, d) and expanded the discussion of observations (Figure 12) in comparison with atmospheric modelling (conceptual models, subcloud layer, cloud layer, updrafts as a prerequisite for cumulus formation etc.).

e) We improved the figures as requested, include ABL top heights, show radiosonde profiles, all profiles are now given as a function of height above sea level (asl). We no longer show cloud top heights, only base heights.

Let us continue with a step by step reply:

Reviewer #1 comments:

Three cases are selected to study the structure of the convective boundary layer by means of the Doppler lidar. Two cases are cloudless and one is influenced by the formation and presence of shallow boundary layer clouds. The research stresses the

ACPD

10, C5586–C5594, 2010

differences in intensity and structure between clear and cloudy boundary layers. I found the results very interesting and well supported by the observational quantification of the turbulent structure. Moreover, the observational study adds new information on previous research. A positive point is that the results can be used for future modeling (for instance, large-eddy studies). However, the discussion level is a bit too phenomenological and descriptive. I miss therefore a more thorough physical reasoning of their findings. Below my major and specific comments:

Major comments

1.- Although the characterization and quantification of the coherent structures is very thorough, there is hardly any description on the surface forcing, thermodynamic structure (including the cloud structure) and boundary layer evolution (only and very briefly for the case 5 April 2005). In my opinion, it is necessary to include the magnitude and evolution of the surface forcing; the potential temperature, specific moisture and wind profiles and interrelate them to the thermal structure of the three cases described. The inclusion of the profiles will also allow showing the role of entrainment process on the intensity and form of the updraft and downdraft motions. Moreover, by adding and discussing a more complete and comprehensive information of the three cases, the research would become very useful for future large-eddy simulation studies.

Reply: We include observations of sensible heat flux (Melpitz, 50 km northeast of Leipzig) and radiosonde profiles (temperature, moisture, Meiningen radiosonde, 170 km southwest of Leipzig). But as can be seen, these observations are not taken at the lidar field site. For this reason, we only can provide rough estimates. It is almost not possible to provide any reasonable number for the latent heat flux. Nevertheless we provide some information on the vertical gradient of water vapour pressure measured at Melpitz.

Because the mean boundary layer characteristics were rather similar (disregarding the presence of clouds) we avoid to discuss the potential, detailed role of entrainment, we

ACPD
10, C5586–C5594, 2010

think that would be just speculation.

2.- One of the main findings is related to the differences in the updraft and downdraft characteristics between clear and cloudy boundary layers. The authors do not provide any explanation on the reason of these differences. Do they occur on the sub-cloud layer (normally with very similar characteristics to the cloud free boundary layer? Were these differences related to the thermodynamic structure, role of entrainment in warming and drying the CBL, or to the surface forcing? These points should become clearer and making the necessary connections through all the paper. I also think that the reader will appreciate a conceptual explanation of these differences.

Reply: Now we clearly state (at different places) that the main body of results belongs to the subcloud layer and that we hardly see differences for the three different days. The only clear feature (difference) is found when the updraft horizontal extent is plotted versus mean vertical velocity.

We do not discuss (because we do not know) whether the found difference is due to thermodynamic structure or entrainment strengths or surface heating.

When discussing the last figure (Figure 12) we mention that our observations (in case of the cloud-topped ABL) are in good agreement with recent, published model approaches, we state what these models assume, and how they work. This should be sufficient regarding the explanation of the observations.

Specific comments 1.- p. 9222 It should be Lothon et al. (2006).

Reply: Done

2.- p. 9224. Does the assumption w=0 hold during all the period? Why is it only checked in the early morning and late afternoon?

Reply: It does not hold during the active ABL phase (always slightly negative, -0.05 to -0.20 m/s). The reasons may be: we see more downdraft areas than updraft areas, surface heterogeneity may also introduce some systematic effects, we state that. An-other reason is that the sampling error is one large additional error source during the time of convection.

3.- P. 9224. It is necessary to include information on the spatial surface conditions during the AVEC experiment. The mesoscale circulation induced by surface heterogeneity can have a strong influence on the boundary layer dynamics and in the development of updrafts and downdrafts (see Patton et al, Journal of Atmospheric Sciences, 62, 2078-2097, 2005) and in cloud formation (see van Heerwaarden et al, Journal of Atmospheric Science 65, 3262-3276, 2008).

Reply: We provide the requested information in section 3.1 (and in section 2, surface heterogeneity).

4.- p. 9225. Please include and discuss equation (6) in figure 2.

Reply: We include the profile after Eq.6 in Figure 2, but we leave out to comment the rather close agreement.

5.- p. 9228. What do you mean by "By keeping the mean wind speed at 4.2 m/s into account"?

Reply: We rearranged the phrase. We need the horizontal wind speed to compute the horizontal extent from the observational time period.

6.- p. 9229. How do they know that shear is a small contribution? Furthermore there are relative recent papers (Pino et al., Journal of Atmospheric Science 60, 1913-1926, 2003; and Conzemius et al. Journal of Atmospheric Sciences 63, 1151-1175, 2006) which point out the relevance of shear in the evolution of the turbulent structure and the boundary layer dynamics.

Reply: We skipped the sentence.

7.- What was the role of wind in the other two cases?

Reply: We have no idea! We leave out to say anything in this direction.

ACPD
10, C5586–C5594, 2010

8.- p. 9229. What is the criterion in estimating zi?

Reply: We leave out to discuss the basic retrieval of zi. This is done in detail in Baars et al. (2008) and the many references in that paper. The Baars paper is now cited several times. The sharp and strong drop of the backscatter signal indicates the ABL top. There may remain an uncertainty of appr. 50-100m, because we do not know how thick the actual entrainment zone is.

9.- p. 9230. The impact of the dust layer on the thermals in the case 18 September is hardly discussed. In addition of the explanation based on the wave activity, Do the updraft and downdraft characteristics decrease in intensity due to the decrease of the surface forcing or the stratification in the upper part of the boundary layer because of the aerosol absorption properties? In my opinion, these two factors can have a larger influence on the structure of the thermals that the wave activity (see for instance Yu et al. Journal of Geophysical Research 107, D124142, 2002)

Reply: We discuss the 18 September in a bit more detail. We discuss the aerosol absorption effect, and provide new references (Yu et al., and Wendisch et al.). But the Saharan dust optical depth was on the order of 0.1. And therefore we clearly state that such thin aerosols do not have a significant impact on surface heating and temperature profile. This we learned from measurements in southern Morocco (Ansmann, 2009). We say that.

10.- p. 9233. As mentioned it, it is necessary to distinguish between the updraft and downdraft characteristics within the sub-cloud layer and the cloud layer.

Reply: The data presented are mostly for the subcloud layer. Therefore, we cannot make solid statements for the cloud layer. And to our opinion, the most interesting layer is the subcloud layer. Here cloud formation is initialized by producing strong updrafts. . .

11.- Figure 11. Do the continuous line show the shallow cumulus? How is it estimated? In the text (p. 9233) is mentioned that the cloud base is 500 m? How is it estimated?

ACPD

10, C5586–C5594, 2010

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Reply: Clouds are easily identified by the huge increase of the signal-to-noise ratio of the Doppler lidar at cloud base. This is used in the estimation of cloud base. We do no longer show cloud tops. They are often not detectable.

Please provide all the necessary information to understand the figure.

Reply: Yes, we hopefully did.

Reviewer #2:

Overview The authors present a study of boundary layer dynamics for clear and cloud topped conditions. The observations where made using a Doppler lidar and aerosol lidar to determine mixed layer depth and turbulence profiles. The updraft and downdraft statistics presented show good agreement with theoretical projections.

General

1) It would be good to see an analysis of the random component of the velocity time series as a function of signal strength (SNR). This would provide an estimate of the measurement uncertainty that could be compared to and combined with the various uncertainties discussed in the paper and could be included in the analysis (using the SNR to apportion the error) as opposed to using the same uncertainty for all of the observations regardless of signal strength (fig2).

Reply: An analysis of the noise error depending on the SNR was made several times during the development of the lidar system. The random error thereby was found from the auto-covariance noise peak and from the high frequency levels in the vertical-velocity wind speed spectrum (e.g. Frehlich, 2001, JTECH). A range with wind errors <10cm/s was found for reasonable SNR. Wind values which are calculated from lower SNR were considered non-valid from thereon. This is now mentioned in section 2. We leave out to present a figure (as suggested by the reviewer). A respective plot could be presented, but we think it would be more distracting to the topic of the paper, than helpful. In this case the somewhat technical wind- and SNR calculation procedure would have to be presented as well in order to interpret the random wind error to SNR relation. But the main point here is that the sampling error is the major error source and probably at least a factor of 10 larger than the instrumental noise.

2) The data shown in Figs 1 & 11 use contours to show the spatial extent of the clouds – how are the tops of the clouds determined and what technique is used to ensure it is not due to attenuation? The lidar data show some evidence of pulse chirp contamination associated with the strong gradients at the cloud's edge in these same regions (Fig 1, panel 2, around 18:00 and panel 3 above the top line of the contour). Have the authors determined that there is no systematic shift in the lidar velocities associated with strong gradients in the backscatter? If it exists and is uncompensated – this effect could contaminate the analysis.

Reply: That's right. Of course the tops of the boundary-layer cumuli cannot be accurately detected with lidar. We changed the graphs accordingly.

The impact of the chirp of the laser pulse on the wind velocity is indeed visible in the vicinity of strong gradients (clouds) in the backscatter coefficient. It appears as a tail with negative velocities behind a cloud and stems from the tail of the laser pulse which shows signs of a chirp of about 0.9 MHz/μs. Nevertheless, the overall effect was found to be small. And because we performed the analysis of vertical velocities characteristics mostly below clouds (no strong gradients, no chirp contamination), we do not see any reason to change any plot or text. But we removed some values in Figure 3 (5 April) which were measured close to cloud top.

Detailed comments: Line # / Comment

Pg 9220 line 13 : "1.3-1.5 larger" Clarify – spatial scale vs strength

Reply: Now we state: Downdraft extents are a factor of 1.3-1.5 larger.

Pg 9223 line 15 : see comments above on error analysis

Reply: As mentioned in section 2, an analysis of the random noise error depending

ACPD
10, C5586–C5594, 2010

on the lidar signal was made several times during the development of the Doppler lidar. The noise error was obtained from constant floor of the vertical-velocity wind speed spectra at high frequency levels (e.g. Frehlich, R. Estimation of velocity error for Doppler lidar measurements J. Atmos. Oceanic Technol., 2001, 18, 1628-1639). A moreover constant range with wind errors from 5-10cm/s was found for a wide signal range. Wind values which are calculated from lower lidar signals were considered non-valid.

Pg 9226 line 25 / Fig2 : break out (or show independently) instrument/measurement noise contribution of error as a function of SNR

Reply: Because the respective uncertainties are on the order of 0.01 m2/s2 and thus rather small we avoid to show this.

Pg 9230 line 17 : updrat

Reply: improved

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C5586/2010/acpd-10-C5586-2010-supplement.pdf

---

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9219, 2010.

ACPD
10, C5586–C5594, 2010
Interactive Comment