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Atmospheric Measurement Techniques Discussions

Interactive comment on "Mixing layer height retrieval with ceilometer and Doppler lidar: from case studies to long-term assessment" *by* J. H. Schween

Anonymous Referee #3

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Note: This referee comment was previously submitted as a quick report for this manuscript.

As the authors of this manuscript already mention, lidar has become widely available during the past decades and yields great possibilities of measuring the wind field and related turbulence parameters. In this manuscript the authors derive the height of the atmospheric boundary layer in two ways. One, with ceilometers, by considering the backscatter coefficient, i.e. the aerosol load. This quantity is typically large within the atmospheric boundary layer and the corresponding residual layer. Two, with Doppler lidar, by considering the variance of the vertical wind speed, i.e. buoyancy, which is

typically large within the convective boundary layer.

The authors compare two ceilometers and subsequently analyse one case study and a year of data for which they compare the Doppler lidar and ceilometer boundary-layer height estimates. All in all the manuscript is well written and the methodology is good, but the innovative level of the manuscript is rather limited to my opinion. The latter could to be a reason for rejecting the manuscript and the reasoning for that is explained in the three points below:

First, the author's main conclusion is that by considering the aerosol load the boundary layer height is often overestimated, except for daytime conditions when a well-developed convective boundary layer is present. This is a well-known drawback (or strength depending on viewpoint) of ceilometers and in that regard not so much new; it has been mentioned many times before. Even for lidar many studies that consider boundary layer height already exist, Harvey et al., 2013, state it like this: "Remote-sensing techniques, in particular lidar, are very useful for analysing the structure of the boundary layer due to their ability to sample at many levels throughout the lower atmosphere and to record data over long time periods. As such, numerous previous studies have used ground-based and airborne lidars to diagnose boundary-layer depth (e.g. Steyn et al., 1999; Davis et al., 2000; Mok and Rudowicz, 2004; Davies et al., 2007; Pearson et al., 2010; Barlow et al., 2011), to determine the vertical velocity and its higher-order moments from Doppler lidar measurements (Lenschow et al. 2012; Lothon et al., 2009) and to retrieve profiles of wind and temperature throughout the lower atmosphere (Newsom et al., 2005)."

Second, my hope was that by combining the ceilometer measurements and the lidar measurements in a clever way extra information could be obtained. However, the authors limit their study to a simple comparison, which to my opinion is a missed chance. The lidar also yields a backscatter profile, theoretically it can also perform polarised measurements so that the depolarisation coefficient can be determined, etc. Nothing of this is taken into account in this study. The authors use well-known and simple

threshold algorithms for determining the boundary layer height. There are neither innovative ideas on how algorithms could be improved for determining the boundary-layer height, nor clever ways of combining the available data from their dataset for improving boundary-layer estimates. To my opinion the simple comparison of the boundary-layer height data, based on threshold values, is a bit disappointing when considering what kind of data set the authors possess.

Third, one could argue that the 1-year data set add extra merits to the study, because so far many studies are limited to case studies. However, the authors seem unaware of a study from the UK Met office (Harvey et al., 2013, A method to diagnose boundary-layer type using Doppler lidar, Quarterly Journal of the Royal Meteorological Society 139: 1681–1693). This study presents a thorough analysis of lidar-based boundary-layer heights for all kind of boundary layer obtained over a period of 3-years. When considering the Harvey et al. study, I come to the conclusions that the presentation of the conclusions of this manuscript is limited in quantification. So questions like e.g. “how much do mixed-layers occur (percentage)?” and “for how many of these cases the ceilometers function well?” remain unanswered or hidden in the main text. Also, the authors throw away many data and are obviously not interested in the boundary-layer "climatology". For me, climatology could be a valid reason for introducing an extra dataset to the scientific literature, but that is not the case here.

So when considering these three issues, I conclude that they present a good, but simple comparison study of ceilometer and lidar performance under mixed-layer conditions and as such present not much news.

Technical comments General: check the use of “which” and “that”. “which” should be preceded by a comma, “that” not.

P.3 Line 4-6: "Furthermore, the MLH determines if cumulus clouds (. . .) for numerical weather prediction.“ This content of this sentence is technically incorrect; physically the MLH does not determine anything. It are the turbulent forcings together with the

strength of the capping inversion, humidity content of the ABL etc. that determine MLH and cloud development.

P.3 Line 13-15: "After sunset on days (. . .) a neutral or slightly stable nocturnal ABL forms, called residual layer (RL)“ This definition of the residual layer is confusing, because the nocturnal boundary layer is often addressed when the stable boundary layer is meant. I would leave out the word nocturnal and add that the RL is the layer above the stable boundary layer.

P.3 Line 20-22: “In case of moderate surface winds (. . .) by surface roughness and stored heat” . More emphasis may be put on this sentence, because normally readers will automatically think of the convective (daytime) boundary layer when reading the definition “mixed layer”. Apparently, the authors also refer to nocturnal cases of induced turbulence by strong wind shear when they talk about the mixed layer; this should be made absolutely clear. I in any case got confused when reading the results and saw that also night-time data were presented, the more because the authors use the definition “convective boundary layer” more often when talking about the mixed layer results.

P.3 Line 25-26: “Most of them are based on proxies, such as (. . .), for the mixing process.” Rewrite as “Most of them are based on proxies for the mixing process, such as (. . .).”

P.4 Line 14: add a comma between “surface” and “turbulent”

P.5 Line 5: add “can” between “lidars” and “measure” . Doppler lidars namely only measure the vertical wind speed when pointing upwards.

P.9 Line 24: “All data below 350 m were” instead of “All data below 350 m was”

P.9 Line 25: Add “to” after “up”

P.10 Line 17: “the actual range is limited to areas with sufficient occurence of aerosol.” Exactly the same is true for the ceilometers. I think this should be added on P.7 line 8

and line 19.

P.11 Line 6-7: "\sigma_w is calculated every 5 minutes over an interval of 30 minutes." I do not get what is done here. Do the authors collect six variance estimates per 30 minutes, which are subsequently averaged to one 30-min value?

P.12 Line 5-6: " (. . .) were excluded that mainly concerned night-time data." This sentence does not flow. Add a semicolon or so "(. . .) were excluded; that mainly concerned night-time data."

P.12 Line 15-17 and 19-20: "Before 10 and after 15 UTC (. . .) as error estimate for MLH_wind" and "In the morning and afternoon hours the relative difference goes up to $\pm$30%." These two sentences seem to belong together, but they are interrupted by a line of thought that wants to make a point about the result found in line 13-15. Restructure for clarification.

P.12 Line 19: the sign in front of the 7% appears to be rotated 180°; in latex write: "\pm" instead of "\mp"

P.13 Line 22: "below 300 m" – I thought ceilometer data from below 350 m were not considered?

P.15 line 25: "The MLH from both methods shows (. . .)" reword as "The MLH from both method show (. . .)" (the authors are talking about more than one mixing-layer height.

P.16 line 4 and 5: delete "MAM" and "JJA".

P.16. Line 20: "This could be as expected (. . .)" reword as "This is as expected (. . .)"

P.18 line 3-4: "(. . .) one can see that the qualitative (. . .) results shown in Fig.5." If I look at Figure 7 then I tend to compare it with Figure 6 instead of Figure 5. Is this a typo?

P.18 line 11: change "feature of" into "feature for"

P.18 line 13: "A way of more clearly visualising" reword as "A clearer way of visualising"

P.19 line 15: add a comma after "both seasons"

P.19 line 25: change "feature of" into "feature for"

P.20 line 16: add a comma between "hundred meters" and "which"

P.21 line 3-5: Even though the areas outside the clouds will be stable, a lot of convective mixing happens within the clouds of a cloud-top boundary layer. Ignoring this cloud-layer in the boundary layer parameterisation of a model can result in significant underestimations of the true mixing. So, why do the authors consider it to be a good proxy for mixing-layer height? Is the mixing-layer height something different than the boundary layer height for these cases?

P.21 line 21: reword "can not" as "cannot"

P.22 line 6: add a comma between "night" and "we could"

P.22 line 9: add a comma between "residual layer (RL)" and "but also"

P.23 line 23: "BL" is not defined before probably ABL is meant?

P.24 line 8: "would significantly alter" reword in "significantly alters"

P.25 line 15: change "back scattered" to "backscattered"

P.25 line 20: change "which" to "that"

P.26 line 1: "further" instead of "furhter"

P.28 line 8: "the detected MLH would decrease (increase) by 7%." reword "the estimated MLH decreases (increases) by 7%".

Figure 1 and Figure 2: adapt the x-axes to go from 6:00 UTC to 18:00 UTC, i.e. remove the redundant white space.

Caption Figure 2: "During daytime there are in average 440 out of" reword as "During daytime there are on average 440 out of"

Figure 3 in the text the authors talk about subplots a and b. Add “a” and “b” to these subplots.

Caption Figure 4: “Black lines with diamonds shows” reword as “The black line with diamonds shows”

Figure 5, 6, and 7: The “N” at all the upper panels can easily be misinterpreted as a “Z”, maybe this can be changed to prevent confusion

Figure 9: “before sunrise” and “after sunset” data seem to be absent, why are they referred to in the legend?

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4275, 2014.