Atmos. Meas. Tech. Discuss., 7, C2008–C2024, 2014
www.atmos-meas-tech-discuss.net/7/C2008/2014/

Atmospheric Measurement Techniques Discussions
Open Access

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

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Received and published: 1 August 2014

[draft, final]copernicus

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Interactive Comment

Reply to reviewers

1 August 2014

1 general

We thank both reviewers for reading and commenting the manuscript.

Both reviewers state that the limitations of mixing layer height (MLH) retrieval based on aerosol backscatter are well known. We believe that this is not really the case especially as there are meanwhile some studies on the diurnal evolution of the mixing layer (ML) (e.g. Baars et al., 2008; Korhonen et al., 2014) which do not investigate the uncertainty for long data sets and all, not just ideal cases. We therefore think that a quantification of the average uncertainty of an aerosol based MLH as we present it is needed for the community.

We emphasize this by adding the following text to the last paragraph of the introduction:

There exist studies (e.g. Eresmaa et al., 2012; Träumner et al., 2011) which show that MLH detection based on aerosol backscatter is only fully reliable during noon hours when the convective boundary layer is fully developed and topped by the clean, free troposphere. Recently some studies investigated the climatology of e.g. the maximum MLH or the ML growth rate (e.g. Baars et al., 2008; Korhonen et al., 2014). Especially

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Interactive Discussion

Discussion Paper

evaluating the growth rate between some hours after sunrise and maximum MLH assumes that there are no limitations of the MLH retrieval during this time. Therefore a quantification of the errors of aerosol based MLH retrievals is necessary. MLH retrieval based on doppler wind lidar gives the opportunity to evaluate this on high temporal resolution and over a long time period, if an automated system is used.

Reviewer 1 as well reviewer 3 suggest to investigate statistics of the derived mixed laxer heights. We added a section in which we investigate maximum MLH, hour of its occurrence as well as morning growth rate based on MLH derived from both methods. To make room for this we reduce the discussion about cases under broken clouds (section 3.2.2) and remove especially figures 7 and 9. Main findings of this new analysis are: maximum MLH from the aerosol based method is 300 m higher, the maximum occurs 1-2 hours later and the growth rate is by 30 m/h lower.

2 Reviewer 3 main arguments

(reviewer statments in **bold**)

In this manuscript the authors derive the height of the atmospheric boundary layer in two ways. One, with ceilometers, ... 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.

Sorry for correcting here - but it may lead to misinterpretations: a large vertical wind speed variance may be due to buoyancy, but also due to wind shear. In the second case the vertical movement may work against stable stratification and reduce it to some degree. We will add a comparison with a radiosonde data set to the manuscript. In this data set are several days with high wind speeds and shear when, even during night, vertical mixing is reported by the radiosonde as well as by the wind lidar.

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 welldeveloped 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. ...

As reported in the general section above we add some sentences to the introduction.

... Even for lidar many studies that consider boundary layer height already exist, Harvey et al., 2013, state it like this: "Remotesensing techniques, in particular lidar, are very useful ... 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)."

Most of the studies mentioned by the reviewer are already cited in our text, except for Mok and Rudowicz (2004) which present two case studies for the determination of the entrainment zone during the morning and afternoon transition. They mention shortcomings of the Steyn et al. (1999) method when applied to their cases, but do not generalize it. The other study not mentioned by us is Davies et al. (2007) who make use of MLH derived from aerosol backscatter profiles, but only to evaluate the performance of two models. They do not discuss the quality of the MLH observations.

From the two doppler lidar studies mentioned we added Lenschow et al. (2012) in section 2.2 because they reinvestigate the σ_w profile proposed in Lenschow et al. (1980):

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This profile is originally based on a handful of airplane measurements but hase been recently reconfirmed with doppler lidar measurements and LES simulations (Lenschow et al., 2012).

This is also important as we motivate the choice of our threshold with the this σ_w-profile. As already discussed in the section about the threshold sensitivity we could show that the uncertainty of our wind based MLH estimate can be at least partly explained by this profile.

We also added Luo et al. (2014) in the introduction as it motivates our use of the gradient method for the ceilometer data:

Recently Luo et al. (2014) found that the gradient method works best over a continental site.

And we use at several places Eresmaa et al. (2012) and Träumner et al. (2011) as reference that there exist limitations to MLH from aerosol backscatter.

Finally we add some references to Harvey et al. (2013) (see below).

We also want to emphasize here that we derive for the first time uncertainty estimates for both MLH estimates. And to our knowledge no data set of comparable size and temporal resolution exists which evaluates aerosol based MLH. Most of the existing sets compare to radiosondes which are available a few times per day.

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. ...

Of course we regret to disappoint the reviewers expectations but our aim was to investigate and quantify the performance of stand-alone ceilometers for MLH detecion. To emphasize this we added the word *stand-alone* in the last pragraph of the introduction.

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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. ...

We apologize, but our doppler lidar is not equipped with the polarization option.

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.

That was not the focus of our study. As stated in the introduction we wanted to evaluate MLH retrieval based on aerosol backscatter, because there are more than 100 ceilometers in Europe and only ca. 10-20 doppler lidars. It is as essential to have a reliable method for both of these type of sensors with quantified uncertainty as to have one THE 'multi-sensor method'. In future, developing such a synergy retrieval would be ideal/beneficial.

To our knowledge there exist only a handful of studies which use vertical velocity for MLH determination. None of them investigates the sensitivity to the σ_w threshold as we did in section 2.2. And none of them discusses shortcomings of the doppler lidar method as we do in the conclusions. And as stated by reviewer 1: *The really new and interesting issue of this paper is the use of a continuous measuring wind lidar for the ML detection* . So we believe that our study is valuable.

And to satisfy the reviewers interest in new methods we added, following a comment of reviewer 1, a suggestion for method based on a combination of cloud base height, aerosol backscatter, and surface flux at the end of section 3.4 (connection to clouds).

Third, one could argue that the 1-year data set add extra merits to the study,

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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). This study presents a thorough analysis of lidar-based boundarylayer heights for all kind of boundary layer obtained over a period of 3-years. ...

We were aware of Harvey et al. (2013) but did not cite it because its boundary layer detection is based on a very specific method. Nevertheless we added a citation in the introduction when listing the methods based on aerosol backscatter:

A somewhat different method is used by Harvey et al. (2013): They define the height of the ABL as the lowest height where within one hour more than 80% of the profiles show now detectable backscatter. This rather gives the height of the aerosol layer but not necessary the MLH.

A second citation is given in the introduction when listing methods for MLH detection based on vertical wind:

In their multi sensor approach for a boundary layer classification Harvey et al. (2013) use a rather complex method to decide whether the boundary layer is coupled to the surface i.e. whether it is a convectively mixed layer: They assume coupling to the surface when the profile of the vertical velocity variance is convex in the lower half of the boundary layer. Interestingly they do not use the vertical velocity variance for the BLH determination but instead the aerosol backscatter.

... 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.

As mentioned in the general part above, we will add a section investigating daily maximum MLH, time of maximum and growth speed. We will add a section in the manuscript where we compare results from both retrievals and the points listed here by the reviewer.

3 Reviewer 3 Technical comments

General: check the use of "which" and "that". "which" should be preceded by a comma, "that" not.

Thanks for the hint, we tried to do our best to correct this.

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.

We wrote it as conditional - "cumulus can develop" - but maybe that was not clear enough. So we reformulated the sentence to:

Furthermore, in a convective boundary layer, cumulus clouds can only develop if the MLH reaches the convective condensation level, making it thus a relevant quantity for numerical weather prediction.

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

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RL is the layer above the stable boundary layer.

Agreed. The passage reads now:

After sunset on days with strong local and weak synoptical forcing, convection decays and a neutral or slightly stable layer forms, called residual layer (RL). At night the emission of long-wave radiation by the surface leads to strong cooling and the formation of a stable nocturnal boundary layer (NBL) close to the surface and below the RL.

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". ...

We slightly adapted the sentence:

In case of moderate surface winds during night a shallow nocturnal mixing layer may be formed induced by surface roughness and stored heat especially in urban areas (Souch and Grimmond, 2006).

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.

Actually both retrievals on their own can not decide whether the mixing is convective or shear induced, as pollutants are mixed independently of the forcing which causes the mixing. But we agree: we will critically review the use of the word mixing in the text, and point to the multi sensor approach of Harvey et al. (2013) wich may distinguish between the forcings.

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 (...)."

done

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

done

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

done

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

done

P.9 Line 25: Add "to" after "up"

done

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

we added the following sentence to the CT25K description:

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Nevertheless the actual range is limited by sufficient backscatter from aerosol.

and for the CHM15k:

As for the CT25K, true achieved range is limited by sufficient backscatter from aerosol. But as the instrument has more power, this range is typically larger than that of the CT25K (Heese et al., 2010).

P.11 Line 6-7: "σ_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?

No, we calculate the standard deviation from data of a 30 minutes interval, but we do it every five minutes and do not average these, i.e. we do oversampling. We changed the sentence to clarify:

From the filtered time series of the vertical velocity, the standard deviation σ_w is calculated every 5 min from the data of the surrounding 30 min interval.

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."

done.

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.

We restructured the whole sub section.

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

This is intended: a change of +7 % in height is related to a change of -25% in σ_w and vice versa.

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

The height limit of 350 m was only used in the comparison with the CHM15K ceilometer (section 2.1.4) for which sufficient overlap is reached only above 350 m. For the Viasala CT25K there is no restriction in height due to overlap (see section 2.1.1).

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.)

done

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

done

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

done

P.18 line 3-4: "(...) one can see that the qualitative (...) results shown in Fig.5." If

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I look at Figure 7 then I tend to compare it with Figure 6 instead of Figure 5. Is this a typo?

No it is not. As we will drop the figures and much of the discussion of the broken cloud cases this paragraph will change entirely.

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

done

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

done

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

done (before which).

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

done

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

done

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

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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?

We add some explaining words at the end of the first paragraph of that section:

In the following we define the top of the MLH to be identical to cloud base although in case of cumulus clouds mixing continues into the cloud. We do this for two reasons: first with the onset of condensation buoyancy increases again and physics changes. And second lidars and especially ceilometers can not view very far into a cloud.

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

done

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

done

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

done

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

yes it is meant. Checked throughout the document.

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

done

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

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done

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

done

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

done

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

done

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.

we do not agree: the figures have the same scaling from 0h to 24h as the diurnal courses in figures 5,6,7 and 10. They visualize that during night in general no MLH detection and no error estimate is possible.

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

done

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

to be done

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

done

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

we think that this is not a severe problem - we will leave the plots as they are.

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

Because they are present in figure 8. All of the 8 subplots of figure 8 and 9 are made with the same program. As we will drop Figure 9 this is not relevant.

We thank reviewer 3 for looking through many of our English phrasing errors.

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