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Determination and climatology of the planetary boundary layer height above the Swiss plateau by in-situ and remote sensing measurements as well as by the COSMO-2 model.

ACPD- 2014-314

The authors thank the three referees for their constructive comments that help us to greatly improve the clarity and the quality of the manuscript. In the following, all the comments are answered and the modifications introduced in the revised manuscript are described.

Answer to the first anonymous referee's comments :

General comment:

- I'm missing a combined PBL time series, resulting of all methods, due to the authors knowledge of advantages and limits of the different methods.

The conclusions of the paper and Table 4 try to synthetize a kind of "best use" of all experimental methods. The MWR with the various PBL detection based on the T profiles has the great advantage of producing a complete diurnal cycle of the CBL, NBL and SBL, but missed all information concerning the RL. The lidar (comprising also the ceilometers, even if not used in this study) seems to the best suited instrument to measure the RL. The radio sounding has a too low time resolution but remains the most reliable measures at midday and midnight.
To implement a real operational algorithm that chose in each case (depending on the time of the day, the season, the meteorological conditions) the best measurement for each PBL layer (CBL, NBL, SBL and RL), a further work on the uncertainty and consequently on a kind of quality flag for each method would be necessary. Then an adjustment to the various and probably numerous special cases related to particular meteorological conditions has also to be developed. This stays however beyond the scope of this paper.

Specific comments:

- I think it's a pity that you have a ceilometer, but used is just for cloud detection. Instead of the temporally "bad" resolved ASR, you could use the pure backscatter signal of the ceilometer with very high temporal resolution for the detection of the CBL height (see Lammert and Boesenberg: Determination of the convective boundary-layer height with laser remote sensing. Boundary-Layer Meteorology 119, 159-170, 2006).

The fact is that the ceilometer used for this study (CBME80 from Eliasson) was not set up in 2012 and 2013 to provide the whole backscatter profile but only the cloud bases (see manuscript p. 15426 line 7). Moreover, the resolution of the backscatter profile is far too low to be applied for PBL detection. It would be otherwise clearly used in this inter-comparison! A new Jenoptic ceilometer was installed at Payerne in 2013, allowing a future comparison (on-going activity).

- You have shown the limits of the bR method due to the sensitivity to the correct surface temperature. In COSMO this method is used for PBL height determination – so doesn't it make sense to choose another method? Have you checked other quantities, like humidity or temperature profiles from the model in combination with one of the other methods?

A study was done (Balazs Szintai, Improving the Turbulence Coupling between High Resolution Numerical Weather Prediction Models and Lagrangian Particle Dispersion Models, Phd Thesis, EPFL, Lausanne, : http://infoscience.epfl.ch/record/150277/files/EPFL_TH4827.pdf, 2010) to choose the best suited method. Not only the reliability but also the applicability were tested and bR method was chosen. The focus of the present paper is to compare the various measurements among each other and with the existing COSMO-2 product , not to improve the latter. A study is however presently conducted to improve the COSMO-2 PBL height estimation.

- The climatology of the CBL and cloudy CBL: in Fig. 9 and 10 you showed a very good agreement of Lidar and MWR for the cloudy CBL, but an underestimation for the CBL. That surprises me, due to the advantage of lidar in cases of cloud free, convective conditions. Where does this underestimation comes from?

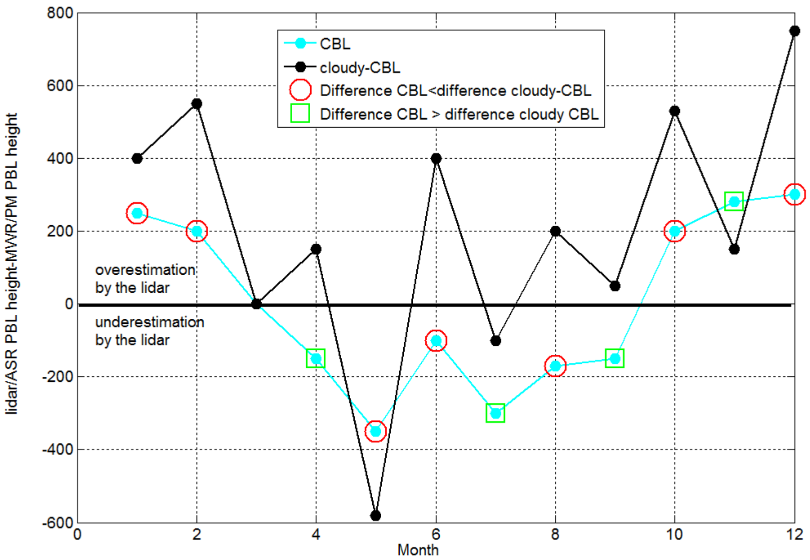
The difference between lidar/ASR and MWR/PM PBL height is not really smaller for cloudy-CBL cases than for CBL cases. These difference are plotted on the next figure. It can be seen that:

- The difference for CBL cases are more regular, showing an overestimation by the lidar in winter and an underestimation in summer.
- The results for the cloudy-CBL cases are much more scattered, reflecting the great difference in the number of cases for the lidar and the MWR. The lidar cannot measure if there is too much or too low clouds. The comparison between the PBL detected by both method has therefore to be taken with great care for the cloudy-CBL climatology.
- The difference between the lidar/ASR and the MWR/PM PBL height is smaller for CBL cases for 7 months and larger for 4 months.
- Taken into account the uncertainties of both methods, we cannot really conclude that the lidar/ASR underestimate the PBL height (or that the PM overestimates it, which can also be possible due to the MWR uncertainties). The absolute differences are most of the time smaller than 200 m, which is similar to the uncertainty range.

The following atmospheric phenomena could also occur:

- The PM method considers an adiabatic rise of the air masses. In the atmosphere, there is probably a deviation of the adiabatic conditions and a mixing of the air masses during the rising up to the PBL top, leading to a lower altitude for the PBL measured by particle concentration than by theoretical convection conditions.
- The PM was applied to θ and not to θ_v. The PBL estimated from θ_v is higher than the one estimated from θ, since the water vapor is lighter than dry air. The consideration of the air moisture will involve higher PBL height and a greater difference between MWR/PM and lidar/ASR in summer, but a smaller one in winter.
- The particles are heavier that air masses and can perhaps stay at a lower altitude than air due to the gravity force, at least for the greatest particles such as mineral dust and pollens. This could explain the lower PBL height measured by the lidar/ASR.

I do not have however a clear estimation of the impact of these atmospheric phenomena and cannot therefore scientifically estimate their end effect on the PBL height determination.



- Fig. 9 and 10: Why haven't you included the time series for MWR/bR? It would be helpful to better rate the results of COSMO.

  The MWR/bR results are now included in the CBL and cloudy-CBL climatology (Fig. 9 and 10) and the text was consequently adapted. Not much modification were added, since MWR/PM and MWR/bR present, as expected, similar results. MWR/bR were already present in the night climatologies (Fig. 11 and 12).

- Fig. 11 and 12: The spread between the lines is very high, so it is hard to know, which lines should be compared together. What's the reason for the low number of cases for MWR compared to the other instruments? MWR PM in CBL cases and MWR/bR for stable ones are both plotted in red, which suggest a bit that both methods are complementary – was what your intention? In the conclusion your suggestion for a good combination is MWR/PM and MWR/SBLpT.

The authors try to adapt the figures describing the climatology in the new version of the manuscript. Now the color code and the symbols are completely similar to Fig. 4, 5 and 8 to allow a better comprehension of all figures. There was no reason for having the MWR/PM and MWR/SBLpT both in red, this was modified.
There is a low number of cases for MWR/bR, but not for MWR/SBI and MWR/SBLpT as can be seen on the lowest panel of Figures 11 and 12. The bR method needs a wind measurement and the first levels of the wind profiler are often invalidated by the automatic quality check of the instrument. All SBL under 300 m could therefore not be calculated due to the absence of wind measurements. This information was added in the description of the bR method:" During night, the Ri_b number is sometimes greater than the threshold already at the ground level due to stable θ profile near ground impeding any PBL detection. Moreover, the invalidation of the first levels of the windprofiler data caused by environmental perturbations also restrict the detection of low PBL height (< 200-400 m) by the bR method.".

Technical corrections:
- P9, L33: please compare with values in Table 3 (0.47 vs. 0.49 . . .)
Your comment is right, the values between the text and Table 3 were harmonized.

- P12, L6: Please explain APCADA.
Apcada is explained under § 2.1: "The cloud cover is detected by Automatic Partial Cloud Amount Detection Algorithm (APCADA) that estimates in real-time the sky cloud cover from surface based measurements of long-wave downward radiation, T and humidity (Dürr and Philipona, 2004). APCADA does not take into account the cirrus clouds."

- Fig. 4, 5, and 8: please exclude the legend of the lower plot and plot it to the right hand site.
The legends are now outside the lower plot.

- Fig. 5: What's the reason for "no data" below 400m in the background?
The windprofiler is not able to measure the first 110-130 m due to electronic time delay between the emission and reception of the signal (some ns). A quality check then flag all the data and often leads to an invalidation of the measurements under 200-300 m a.g.l. due to contamination by the direct environment of the instrument (such as cars, leaves movement, reflection of building, trees,…). This automatic QC is based on numerous parameters (time and spatial homogeneity, SNR threshold, width of the peaks,…) and leads to the invalidation of the measurement up to an altitude depending mostly on the meteorological conditions. A sentence was added to the revised version: "The first levels of measurements up to 200-400 m a.g.l. are often automatically invalidated because they suffer from internal and environmental perturbations.".

- Fig. 8: The background is SNR or lidar? Both would make sense, but please decide. ;-)
It is SNR, which is now mentioned in both text and figure caption!

- Fig. 11 and 12: The additional lines in the lower plots are not explained. Why have you separated the number of days in these cases, but not for the CBL cases?
The additional lines give the number of cases used to calculate the monthly medians for each method as explained now in the figure captions. In case of CBL only the MWR/bR and lidar/ASR have been differentiated since they really have a lower number of bases for each months. The monthly medians of all the other methods were calculated on the same number of cases.
Concerning the SBL cases, the restriction to clear and cloudy nights greatly restrict the number of available cases. Taking into account only the cases when all methods provided PBL heights would have restricted the analysis to a too low number of cases (<10 or even <5) and therefore limited the pertinence of this SBL climatology analysis.

- Please notice a uniform notation of the methods (MWR PM or MWR/PM,COSMO vs. COSMO-2...)
The various notations were checked in the text, tables and figures.

Answer to the second anonymous referee's short comment :

- The paper users a COST Action definition of the PBL that highlights the turbulent nature of the PBL, in which constituents "become vertically dispersed by convection or mechanical turbulence within a time scale of about an hour". However, the methods that the paper use, all then ignore the turbulent character of the PBL and focus instead only on the mean profile structure.

The authors do agree that the turbulence inside the PBL is not at all analyzed in this paper and that a larger amount of studies tend to characterize the PBL by the turbulence. The wind turbulence remains however most of the time the greatest at the PBL top, and this phenomena is used to detect the PBL height from the wind profiler measurements. This paper "only" aims to estimate the PBL top with several methods in order to compare and validate them. Even if the paper does not study the turbulences inside the PBL, the PBL definition from the COST action remains valid and illustrates most of our PBL height detection methods.

- In particular, for the wind profiling radar PBL depth estimation, they do not use the most recent techniques that utilize turbulence information (Bianco and Wilczak, 2002; Bianco et al, 2008), but instead rely on an older technique already well known to be inadequate. I would have thought that these newer techniques would have been used in section 2.2.2 "Method based on wind turbulence profiles" but this section makes no mention of the wind turbulence profiles at all, only the Cn2 profiles!

  The authors are well aware of the results of Bianco et al. (2002, 2008 and 2011). Bianco and Wilczak (2002) apply first a fuzzy logic algorithm to estimate the SNR from the radar Doppler Spectrum and second to estimate the mixing layer depth. Bianco et al. (2008) first prove that the multipeak picking (MPP) procedure to identify the atmospheric signal in radar spectra is the most reliable one. In this study, we also use a MPP procedure (this information was added to the WP description in the new version of the manuscript). Second, Bianco et al. (2008) used a sophisticated method to select the cases corresponding to a conceptual model of convective PBL, where the mixing layer height can be estimated with a very high reliability. The results show a very good agreement with human determination of the PBL height, leading however to a drastic restriction of PBL height availability. The purpose of this paper is to produce an operational estimation of the PBL height and to test the reliability of each instrument. Our study aims to compare and validate the WP results with other instrumental methods in the same number of cases. Furthermore PBL detection in case of all meteorological conditions are used to obtain a PBL height climatology. Therefore, no best cases selection algorithms such as reliability tests and post-measurement temporal continuity algorithms were applied to none of the methods. The authors agree that such improvements would increase the reliability of the data, but this should be done for each instrument and algorithm. Then a combination of PBL height best detection from all method should be developed for all PBL cases (CBL, NBL, SBL,…). This work remains however beyond the scope of this paper. The fact that more reliable results could be obtain by the development of the method and the selection of clear CBL cases is mentioned in the new version of the manuscript with a reference to Bianco et al., 2008.

- Separately, for the radiosonde parcel technique, I wonder what is special about using measurements at 2m, instead of say at 0.2m or 10m? the variation of temperature between 0.2 and 10m on a sunny afternoon can easily be 2-3 C of more, yet the authors only vary the surface temperature by +/- 0.5 C in their sensitivity analysis.

  As recommended by the WMO, the ground temperature is always measured at 2m. We apply this WMO definition in our study as it is now specified in the manuscript in § 2.1. The authors agree that the T variation in the first 10 m can be large. In some studies, an excess temperature is added in convective cases to the measured ground T (at 2m) to compensate the T difference between 0 and 2m elevation. This algorithm was not used for this study but is presently tested in a further study, but we cannot yet estimate if this excess temperature improve the PBL height detection.

- For the radiometer data processing, it would be useful to state how the various angle scans are combined into a single temperature profile.

  This information have been added to the paper: "Statistical regressions models are used to convert the radiation measurements from the elevation scan and from the zenith observations in two temperature profile covering 0-2 km agl and 0-5 km agl, respectively. The two temperature profiles are merged into one single T profile using the profile derived from the elevation scan and the upper part (2-5 km) of the profile derived from the zenith observations.".

- Finally, the manuscript has many grammatical errors. Although acceptable for discussion, the manuscript would require significant modifications for publication in a journal.

  The corrected version of the manuscript was reviewed by a native English speaker.

Answer to the third anonymous referee's comments :

General comments:
- including indepth analyses and discussions on the source of errors in model PBL height predictions;

The authors do agree that an in depth analysis of the uncertainties would be valuable. As explained now in the manuscript, the uncertainties can be estimated either statistically or by analyzing the complete instrumental process. Several studies are presently performed at MeteoSwiss to develop uncertainties analysis for each instrument (see for example Philipona et al. (2014) for the radio sounding and Haefele et al. (2014) for the wind profiler). Since these procedures are not yet finalized for all instruments involved in this study, a complete uncertainties analysis is presently not possible. The uncertainties presented in this study relate therefore to statistical comparison of the results compared to a reference method (validation presented in Figs. 6-7) or to statistical dispersion of the results for cases considered as similar (climatology analysis presented in Figs. 9-12). The manuscript was consequently modified:

"Each of the considered method and instrument have their own uncertainties in the PBL height detection. The uncertainty minimum is usually obtained for fully developed CBL reported in Figs. 6-7. Several type of uncertainties can be however be estimated. First, a statistical uncertainty (see for example the climatology analyses Figs. 9-12) estimates the fluctuations of measurements for cases that are considered as similar; these fluctuations reflect the measurement uncertainties but also the variation of the atmosphere for "similar conditions", but are unable to detect systematic bias. A measuring uncertainty can also be derived for each instrument providing an estimation of systematic bias and fluctuations; such analysis have been up to now only partially made for some instruments, but not all, impeding our ability to propagate these errors on the various PBL height detection methods. Finally, the comparison to a "reference" (Figs 6-7) allows to statistically estimate the reliability of the other methods. The uncertainties bounded to the method and the instruments (see Sect 2.2) provide however a similar picture as the inter-comparison, with the greatest precision for methods based on T profile and the lowest for WP/SNR.
Finally, in addition to considering the differences in statistical and intrinsic uncertainties found between the various instruments and methods, one has to consider that the measured parameter (PBL height) is in reality not a fixed point but rather a transition layer between two atmospheric states. Both Stull [Stull, 1988] and Garratt [Garratt, 1992] estimate the thickness of the entrainment zone as large as half the mixed layer depth. This transition layer reaches therefore between some tens to some hundreds of meters. Moreover the remote sensing instruments measure an air volume with a thickness corresponding to the instrument level (see Sect 2.2) and not a precise point. The obtained differences between the experimental methods and their uncertainties remain in the same order of magnitude of this transition layer thickness."

- providing more quantitative evidence for supporting some statements

See the answer to the specific comments below.

- providing more clear and precise wording throughout the manuscript.

The corrected version of the manuscript was reviewed by a native English speaker.

Specific comments :
- Page 15422, the paragraph starting from Line 14: It is important to understand the strength/limitation of various methods for PBL heights reported in literature. Descriptions of this paragraph could be more concise and more quantitative.

I do understand that some more quantitative analysis could help the reader. However the cited papers include most of the time comparison between 2-3 setups. These setups comprise various instruments (RS, MWR, WP, lidar, ceilometer, sodar, Doppler wind lidar, measurements on mast, aircrafts measurements ,…), methods (parcel method, Bulk-Richardson (various equations and threshold), gradient analysis, wavelet method, fuzzy logic analysis, near surface thermal gradient), models (COSMO Germany, COSMO Switzerland, ERA), sites (several continents, urban, marine, continental and alpine environment,…), and length of the measurements (from 2 days to 10 years), so that it is impossible to obtain a clear view of the strength and limitation of the methods from all these papers. Having a lot of various instruments at Payerne, this study tentatively try to clarify this situation.

- Page 15422, Line 24: The sentence is not clear to me.

The sentence was reworded: "Non-convective weather situations corresponding in most of the cases to cloudy and rainy situations lead to much greater discrepancy in the PBL height estimations. In these cases, the difference becomes even greater if the methods/instruments are designed to detect various types of PBL such as CBL, NBL or RL."

- Page 15424, Line 21: What does "the low mode" mean? Please provide sufficient information for readers to understand.

The wind profiler description was completed in the revised version: "The WPs are Degreane PCL1300 (Degreane Horizon, 2006) with five antennas operating at 1290 MHz (λ=23.3 cm) alternatingly in low and high mode covering altitudes from 0.1 to 3 km and 0.3 to 8 km, respectively. For this study only the low mode has been used with a vertical resolution of 150 m and the first level at 105 m.".

- The temporal resolutions reported in this manuscript (e.g., 30 min, 40 min) are quite coarse. Is the best resolution that can be achieved? They seem really long!

  The wind profiler has an operational time resolution of 40 min (gliding average of 10 min measurements). A temporal resolution of 30 min could be achieved, that correspond to the usual temporal resolution of wind profiler around the world and that is necessary to ensure a sufficient quality for all weather conditions. This sentence was added to the final version: "this time resolution is necessary to obtain a sufficient quality of the measurements under all weather conditions".

  The lidar/ASR needs also an integration time of 30 min to obtain measurements of a sufficient quality.

  Both instruments were used in their operational mode.

- Page 15427: Why can't cloud fraction be determined by ceilometer measurements?

The ceilometer is only measuring in one direction (at 90° from ground) and is only able to measure the presence of cloud above the instrument. The cloud fraction refers to the total sky fraction covered by clouds, what cannot be estimated by a single point measurement.

- Page 15428, the paragraph starting from Line 17: Please define when is "if needed" in line 20. Additionally, the sentence in Line 24 doesn't read well and I cannot understand it. Also, RH and rho are not defined.

  The cases "if needed" was rephrased: "If the T profile was measured by the MWR and the PBL height is found between two measured levels, a linear interpolation between the two measured θ is applied to determine the PBL height.". The sentence Line 24 was also specified: "Far larger PBL height uncertainties up to 500 m were found just before the sunset, when the vertical heat flux becomes negative. ". The pressure p and relative humidity were defined.

- Page 15430, Line 4: shouldn't it be rho_0?

  Yes of course, I missed this mistake in the proofs.

- Page 15430, Line 1–4: Please elaborate on this and provide sufficient information for readers to replicate results.

  Further information concerning the time continuity algorithm were added: "the k SNR peaks ($s_{k,i}$) at time i with local maximum greater than 75% of the absolute maximum was weighted by a Gaussian function $g(s_{max,i-1}, \sigma)$ with mean equals to the PBL height of the former time step i-1 and standard deviation σ depending on the time of the day. At sunrise, the Gaussian mean (PBL height) is set to zero m a.g.l. (ground level) and σ to 3000 m, that is three time higher than during the afternoon. The PBL height is then attributed to the maximum of the weighted SNR peak $S_{max,i}=max(s_{k,i}*g(s_{max,i-1},\sigma))$. The uncertainty of this method is considered equal to the full width at half maximum (FWHM) of the selected SNR peak after subtraction of the noise floor and is on the order of 100 to 500 m.".

- Page 15430, Line 8–10: Please describe clearly what "SNR slope and curvature" mean here.

We tested several algorithm to improve the PBL height detection taking into account either the SNR maximum, the minima of its first derivative (slope) and the maximum of the second derivative (curvature). The use of only the SNR maximum was found to be the most reliable method. The text was changed accordingly:" PBL detection algorithms involving first and second derivatives of the SNR peak were tested but have shown a lower consistency with respect to the other PBL height detection methods in addition to a higher rate of false detections. ".

- Page 15431, Line 17: Please double check if this should be WP or WR.
We checked and WP is correct.

- Page 15432, Line 17: Please elaborate on this sentence a bit more. The current explanation doesn't really help to explain the difference between two methods.
  I'm really sorry, but I do not understand this question. At line 17, a very good agreement between all methods is described and not a difference. I think that the referee wanted to have more explanation on another sentence, but I don't know which one.

- Page 15433, Line 4: Really? Can WP/SNR detect cloud top no matter how thick clouds are?
The detection of the cloud top depends clearly of the cloud elevation. Cloud tops higher than the detection range of the wind profiler (some 8 km) cannot be detected. The presence of clouds does not however restrict the WP detection range. In cases of multi-layers of clouds, the region with the greatest SNR ratio will depend on each particular cases and no general statement on which atmospheric layer (highest or lowest cloud top, shear wind layers,…) would be detected can be made with our present knowledge.

- Page 15434, Line 10: Isn't this section talking about clear-sky conditions? How come outliers in PBL heights can be attributed to "elevated cloud layers'?
  As it is now mentioned in the text, the selected "clear-sky convective cases" report to days with a clearly recognizable CBL pattern and with at least half of the sun radiation from sunrise to 13:00; the presence of some clouds is therefore not excluded and can generate outliers in the CBL heights detected by WP.

- Page 15435, section 3.4: Could the author please clarify if this comparison was conducted for all cases, or just for clear-sky cases? This should be clearly mentioned in the manuscript.
  This comparison is done with the same cases selection as in § 3.3. This is now mentioned in the manuscript.

- Page 15435, Line 23: What does it mean by 'physically meaningful systematic positive bias"? Also, please elaborate on the explanation for this systematic bias.
 The effect of taking into account the air moisture is described in § 3.1. We elaborate this paragraph to explain this phenomenon and refer to it in page 15435 Line 23:
"This positive difference can be clearly attributed to the effect of moisture that lightens the air and allows it to convectively rise to a higher altitude."

- Page 15436, Line 4: Please provide evidence to support this statement.
This statement can be directly deduced from equation (1) in convective situation: if $\theta(z_0)$ is overestimated, $(\theta(z)-\theta(z_0))$ and Ri_b will be smaller, so that Ri_b will becomes higher than the chosen threshold for higher z. This leads to an overestimation of the PBL height. Since this statement comes straightforward from the definition of the method, I did not described it extensively in the paper but just added the reference to the equation (1).

- Page 15436, Line 12: It is quite disappointing that this manuscript does not provide more analyses to identify the main sources of the model errors.
The current paper focuses on the various measurements. We are now studying the sources of errors in the COSMO-2 model and will present the results in a forthcoming paper. The model estimation includes a lot of parameters including the model estimation of the T at 2 m, the T, RH and wind profiles, the difference is using θ or θ_v, the use of PM or bR methods, the possible interpolation between the grid points, the possible use of excess temperature (see answer to referee #2), and the various atmospheric states during the year. Such a complex study is really beyond the scope of this paper.

- Page 15437, Line 20: The paragraph needs to be re-written in a more scientific way.
This paragraph was corrected in order to suppress all qualitative adverbs.

- Page 15438, Line 1: Please provide quantitative evidence to support the statement.
  An analysis of the cloudy-CBL detected by the wind profiler and the cloud base measured by the ceilometer shows that the cloudy-CBL is found at a higher altitude than the cloud base in 45% of the cases. This percentage increases to more than 50% if the cirrus as excluded by taking only cloud bases < 4000 m. This is now mentioned in the manuscript:" The WP/SNR cloudy-CBL heights are most of the time more than 500 m higher than MWR/PM ones; 45% of these cloudy-CBL heights detected by WP/SNR are however higher than

the cloud-base detected by the ceilometer. The WP/SNR measures in that cases the cloud top (see for example Fig. 5).".

- Page 15438, Line 12: Could the author please comment why these two ground sites have almost the same monthly variations of the sample size? Is it expected?

In case of precipitation, the lidar is shut down, the radiometer does not provide reliable results and the windprofiler measures the precipitation velocity so that the PBL height can also not be estimated. All the experimental methods are therefore similarly limited by precipitation. Both stations are situated in the Swiss plateau at about 160 km. A correlation between raining days at both stations is therefore highly expected.

- Page 15438, Line 1–15: Not all data points in 0–5 hours on the same day would be selected in the analyses. Therefore, it would be more scientifically/statistically correct and useful to count the sample size in terms of each data point, and then convert them to an equivalent length in days.

The clear or cloudy nights were selected first as nights without precipitation. This mean that night with precipitation between 0h-5h are simply not taken into account at all for this analysis. This was reworded to avoid confusion: "Clear-sky (186 at PAY and 163 at SHA) and cloudy nights (126 at PAY and 151 at SHA) were selected with the criteria of no precipitation between 00:00 and 5:00 in addition to 0-2 and 7-8 octa of the sky covered by clouds estimated by APCADA, respectively.".

- Page 15438, Line 22: How was this statement made? Evidence or reference?

The reasons why the WP is more frequently subjected to false attribution than the other method is briefly described under § 2.2.2 (this was added at line 23) and the reasons are really inherent to the measurement methodology. The lower reliability of WP/SNR PBL height determination can also be noted by the size of the percentiles and whiskers on Figs 6 and 7 as well as on all plots describing the climatologies. We think however that a reference to the figures 6-7 and 9-12 would be redundant at this point and would make the description of the results more opaque.

References:

R. Philipona, A. Kräuchi, G. Romanens, G. Levrat, P. Ruppert, D. Ruffieux, B. Calpini : Upper-air radiosonde intercomparisons and uncertainty estimation, WMO technical conference on mtetorological and environmental instruments andmethods of observation, Saint Petersburg, Russian Federation, 07-09 July 2014, (http://www.wmo.int/pages/prog/www/IMOP/publications/IOM-116_TECO-2014/Programme_TECO-2014.html).

Haefele A., and Ruffieux, D.: Validation of a 1290 MHz windprofiler using radiosonde GPS wind measurements, Meteorological Applications, submitted, 2014.