

Response to Reviewers – “Boundary-layer height and surface stability at SMEAR II, Hyytiälä, Finland in ERA5 and observations”

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We thank the reviewer for their constructive comments on our submitted manuscript. We have copied the comments of the reviewers in black here and include our response to each individual comment in [blue](#).

Reviewer 2

The paper deals with the estimation of the atmospheric boundary layer height (ABLH) and surface layer stability for the Station for Measuring Ecosystem-Atmosphere Relations (SMEAR) in Hyytiälä, Southern Finland. This station has been in operation for about 25 years with the aim to study land surface atmosphere interaction processes including trace gas and aerosol measurements. The station is part of the European Research Infrastructure Networks ICOS and ACTRIS, its comprehensive characterization with respect to atmospheric mixing potential is thus of high practical relevance. Before 2018, profile measurements of atmospheric variables have been performed at Hyytiälä during short-term field campaigns only using radiosondes, a microwave radiometer has been installed at the site in 2018. To create a long-term ABLH climatology for the site, the authors therefore use ERA-5 data which are validated against radiosonde and microwave radiometer (MWR) observations for two shorter time periods, unfortunately the reference radiosonde and MWR measurements do not cover the same time period.

The paper is well written and well structured. The approach chosen follows a sound methodology, and the results obtained are comprehensively presented in the manuscript. The paper is of limited originality with respect to the methods applied and the relevance for progressing measurement techniques. It relies on the application of methods adopted from literature, not all of them are considered as appropriate. Findings regarding the strengths and weaknesses of the different methods to derive the ABLH from radiosonde and MWR are in line with previous studies reported in the literature. Data Analysis of the ERA-5 data is, to my opinion, the stronger part of the paper. The observations part could be shortened, e.g., I am not sure whether the case studies section is really needed and whether all the experimental methods need to be discussed extensively considering their (known) limitations and methodical differences to the approach used in the ERA-5 analysis. In addition, I see a number of specific deficits and minor issues the authors should address, before the manuscript might be finally accepted. These are specified below.

Specific comments

1. A few more experimental and methodical details should be given, e.g.
 - Which type of radiosondes was used during the 2014 experiment? I am not aware of any radiosonde that measures dry and wet bulb temperature. The sondes were standard Vaisala RS92. The text in line 120 has been clarified to make it clear that the radiosonde does not measure wet bulb temperature directly - we originally meant these variables are available in the data files downloaded from ARM. We have also added details about the vertical resolution of the soundings to this section.
 - Was the 12 UTC radiosonde really released at 12 UTC? (in the operational praxis of Met Services the so-called 12 UTC sounding is often performed an hour earlier or so to ensure that a profile covering the whole troposphere is available at the reference time even in case of a necessary second ascent if the first one fails at low altitudes) – this would affect the discussion in line 625ff. No, the radiosondes were not released at exactly 00, 06, 12 and 18 UTC, they were all released 9 - 45 minutes before these times which varied from day to day. The earliest 12 UTC sonde was released at 1115 UTC and the latest at 1151 UTC. We have added information about this to section 2.2.
 - The authors mention two sonics at 23 m and at 46 m, but it remains unclear which of the sonics was used to derive stability? We used the sonic at 23 m to derive the stability classes. We have revised the text in section 2.4 to make this clear.
 - Why did the authors use a temperature from within the canopy as a reference temperature? We used the 30-minute mean temperature measured at 16.8 m as it was the closest temperature to the surface. Furthermore, the 30-minute mean temperature was only available at 16.8 m and 33.6 m in the smartSMEAR portal. We have now investigate whether the choice of reference temperature strongly influenced the stability parameter by re-computing it using a constant reference temperature and found that it did not.
 - The definition of the stability class ranges in lines 178-180 (and also on p. 10) differs from what is indicated in Figure 6, 7 and in Tables 3, 4. The stability class ranges were correct in Figures 6 and 7 and in Tables 3 and 4. They were incorrect in the text but have now been updated. This error stemmed from originally having the boundary between the weakly and very cases at ± 1 but the distribution made more sense to have it at ± 0.1 and thus we changed this during the research process. There were no errors in the statistics or conclusions presented in the manuscript.
 - The authors work with quite a number of different stability estimates (from the sonic, from the LL10 method, from the MWR output) – all these methods rely on different variables and represent different scales – have the results from these methods ever been compared and critically assessed? This is a valid point and we were not aware of any previous comparisons. We now have compared the stability classes derived from the sonic (eddy covariance data) to the LL10 stability classes (Figure 1 in this response). In general, there is reasonable, but not perfect, agreement. First, the LL10 method mainly (254 times) diagnoses neutral BLs when the EC diagnoses either very unstable or weakly stable BLs. However, there are 28 soundings for which the EC stability class indicates either very unstable or weakly

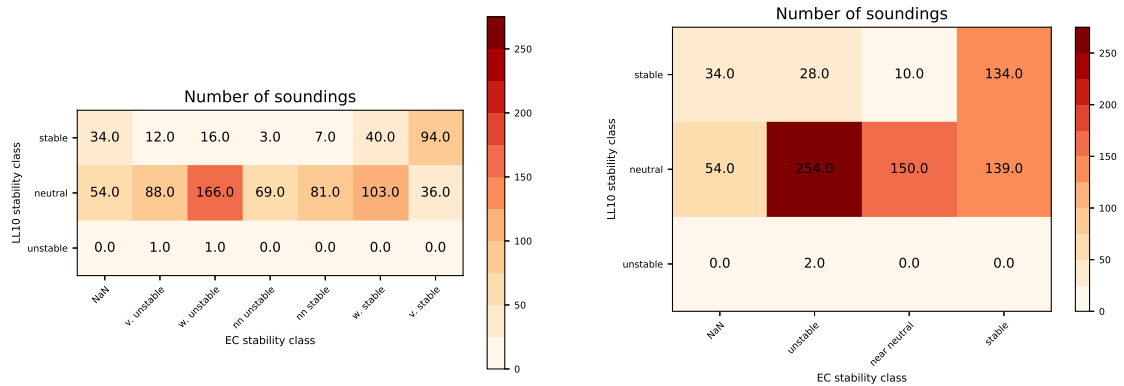


Figure 1: Comparison of the stability classes derived from the LL10 method (y-axis) and from the eddy covariance data (x-axis). Shading and numbers show the number of soundings in each bin. In the left hand panel, the EC stability classes have been group so that unstable includes very and weakly unstable, neutral includes near neutral unstable and near neutral stable, and stable includes weakly and very stable classes. NaNs are almost all caused by missing EC data.

unstable conditions but the LL10 stability is stable. There were 134 cases where the EC stability was weakly or very stable and the LL10 stability agreed, also predicting stable conditions. The biggest discrepancy is the 139 cases where the EC stability class is very or weakly stable but the LL10 stability class is neutral. It appears that the LL10 neutral class is too broad and contains many cases deemed unstable by the EC method and some stable cases. Other studies have also shown the convective BL type as diagnosed by the LL10 scheme to be less common than neutral BLs over land (Zhang *et al.*, 2021). We now include these additional figures in supporting material and have added some discussion to section 3 where we describe the LL10 scheme.

- For the temporal assignment it appears a bit irritating in several places that UTC is translated to local legal time instead of local solar time since ABL dynamics is related to the latter rather than to the former. We now include both local legal time and local solar time in the text when we have translated UTC to local legal time. We retained local legal time as it may be useful to some readers who want to compare other studies to this one (many papers using aerosol data from Hyytiälä use local legal time).
2. A critical discussion on the limitations of the measurements and methods is largely missing – just a few examples:
- According to Figure 1, the sonic is positioned at the edge of an about 1:12 slope with a lake and some larger clearings just a few hundred meters to the west. Does this have any influence on the representativeness of the sonic data? How can it be justified that a local z/L value close to a forest canopy is appropriate to characterize atmospheric stability up to a height of several hundred meters. Did the authors perform any comparison of, e.g., the stability derived from the eddy covariance measurements compared to the one derived from the soundings in the frame of the LL10 method – see above? We agree that the station

site is not homogeneous and therefore does have some limitations when observations from slightly different locations are compared. We have added text to section 2.1 noting the non-homogeneous nature of the site and the potential limitations of the measurements. We also have added text to the methods section to highlight how the non-homogeneous nature of the site, and the different measurement locations, means that the EC measurements may not be completely representative of the surface layer where the radiosondes were released.

- Both the parcel and Ri number methods are quite sensitive to the surface temperature value used in the analysis, and it is always a matter of discussion which data to use here. A near-surface temperature measured independently in a shelter often does not fit to the radiosonde profile, on the other side the first temperature values from the radiosonde profile may suffer from the sonde being in the hands of the observer before being released and not experiencing the ventilation as during the free flight. In some studies, an excess temperature is introduced or a limit for a possibly superadiabatic near-surface lapse rate is set. These issues should be addressed in the methods description. Yes, we agree that these are important aspects and have added text about these issues in section 3
 - In lines 522-523, the authors bring in another ABLH estimate which is based on a completely different variable than all estimates considered in this paper. I suggest to omit this, otherwise it would call for a critical discussion of ceilometer ABLH estimates which is a delicate task itself. We have removed the text about the Jiang et al (2021) study from this section as we agree that we do not want to include discussion of estimating the BL height from ceilometers.
 - Figure 9 indicates quite a substantial part of missing data, even in summer. For winter the authors argument with icing of the sensor, what are the possible reasons in summer? Are these missing data assumed to introduce a bias in the distribution of stability classes? The main reasons for the missing data in summer is due to the thresholds and requirements of the methods used to compute the fluxes. First, the data must be stationary enough over the 30 minute averaging time and second the friction velocity must exceed a threshold otherwise these timesteps are removed. We do not think that the missing data introduce a bias into the distributions as although there is an annual cycle in missing data there a much weaker (or no) diurnal cycle in the amount of missing data (Figures 10b, 11c).
3. The authors in their analysis mix methods that are based on the consideration of different physical processes, they thus compare apples and pears naming all these derivatives ABLH without a critical discussion on that. This necessarily will result in some dis-agreement when comparing the results. It is obvious that Heffter’s method is basically suited to obtain an estimate of the convective ABLH, and previous studies have already demonstrated the tendency of this method to overestimate ABLH. From the MWR, in stable conditions, they derive a kind of inversion or stable layer upper boundary, that is purely based on an analysis of the temperature profile which contains limited information on shear-induced turbulence, if at all. For the ERA-5 analysis, a Ri number method is considered as appropriate, it would thus be consistent to put emphasis on those experimental data which provide a comparative ABLH estimate.

This is a valid point. We fully agree that the different methods are estimating slightly different things and acknowledge that whether shear-driven turbulence is considered or not, will have a

critical impact on the values of BL height derived. We have added text to section 3 to highlight this limitation and attempt to relate some of the detected differences in section 5 and 6 to the differences in the methods. Furthermore, we agree that the fairest comparison with ERA5 is with the Richardson number methods applied to the radiosondes. We now attempt to make this clearer in section 6 and try to put more weight on this comparison.

4. The presentation of results in Sections 5 and 6 is widely descriptive, I would love to see some discussion of the findings already here, even if it is partly given in Section 8, e.g., We have attempted to add more physical explanation into section 5. However, for many of the points we cannot provide solid evidence or show causality. Therefore, much of the discussion is somewhat speculative.

- Why is ERA-5 in better agreement with $Ri(0.5)$ from radiosoundings for the very unstable case, but with $Ri(0.25)$ for the weakly stable case? This is difficult to know for sure. ERA5 uses a critical Richardson number of 0.25 which should give shallower BLs than the sonde with $Ri_c = 0.5$. However, ERA5 has lower vertical resolution which means that in very unstable conditions the capping inversion may be weaker (more smoothed out) in ERA5 than in the sounding (which is the case at 12 and 18 UTC on the 7th of May in the case study example). This would cause a higher BL in ERA5 than in the sounding if the same Ri_c value was used. This may explain why ERA5 has deeper BLs compared to the radiosondes with $Ri_{0.25}$ and hence why ERA5 agrees better with the radiosondes with $Ri_{0.5}$. For weakly stable cases, ERA5 and $Ri_{0.25}$ agree well whereas $Ri_{0.5}$ estimates deeper BLs than ERA5. This may be because the type of vertical profiles found in weakly stable cases are easier for ERA5 to resolve as they likely do not have strong surface-based inversions or strong capping inversion. However, this is somewhat speculative - a thorough analysis of how ERA5 represents vertical profiles of potential temperature and wind under different stability classes is not within the scope of this current study (although it would be very interesting!).

- Why are very shallow BLs less common than moderately shallow BLs in winter? It is hard to know for sure but there are a few possible explanations. To get very shallow BLs, usually very stable conditions are required. This needs calm wind conditions and strong radiative cooling at night so clear skies. Figure 2 in this response shows that the mean, median, 10th and 90th percentiles of wind speed measured at the main SMEAR station at 67.2 m is higher in winter than in summer. In addition, Figure 2 also shows that the percentage of times that calm conditions (wind speed $< 2ms^{-1}$) occurs is less in winter than in summer. Therefore, it is windier in winter which promotes shear driven turbulence which likely prevents very shallow BLs developing.

A recent study using ceilometer and pyranometer data at Hyytiälä (Ylivinkka *et al.*, 2020) shows that winter has more cloud than other seasons. This also helps explain why very shallow BLs are less common in winter as the presence of cloud will prevent strong radiative cooling at night. Ylivinkka *et al.* (2020) also showed that there was no diurnal cycle in cloud cover from October to April. This helps explain our results that there is no diurnal cycle in the depth of the BL from November to March. We have added this discussion about the seasonal variation of windspeed and cloud cover to section 6

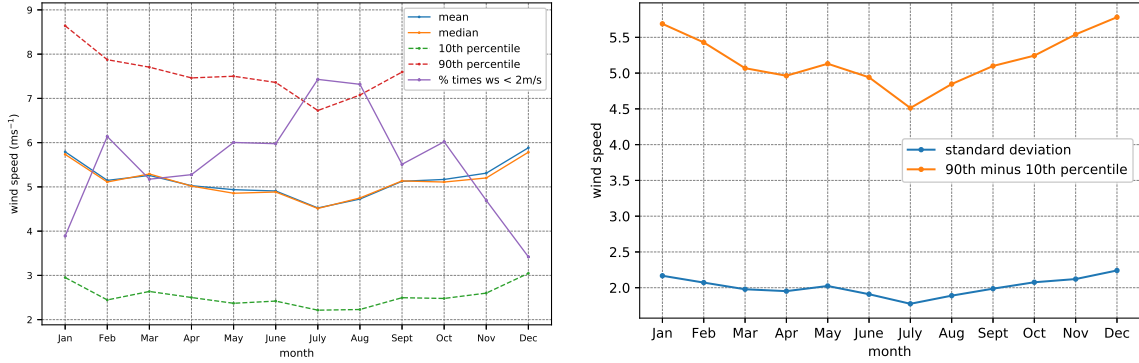


Figure 2: Left: The monthly mean, median, 10th and 90th percentile of windspeed measured at 67.2 m at SMEARII estimated using data from January 1997 to December 2019. Right: The monthly mean value of the standard deviation in the wind speed and the monthly mean value in the difference between the 90th and 10th percentile of windspeed measured at 67.2 m

- Why is the ABL height variability so large in April and May? The BL height variability is largest in May and second largest in June and this should be explained. The variability in April is smaller so we do not focus on this. We analysed the long-term temperature climate and its variability over the 23 year period. The BL height is most likely very variable in May since the temperature is more variable in May than in most other warm season months (Figure 3 in this response) - both the standard deviation and the difference between the 90th and 10th percentiles show a local maximum in May. Large variations in cloud cover could also be a reason for more variability and Ylivinkka *et al.* (2020) show that there is a diurnal cycle in cloud cover at Hyytiälä in May with a peak in late afternoon. We have added more discussion about the high variability in BL height and its likely causes to section 6.
 - How to explain the annual cycle in the occurrence frequency of the weakly stable class (line 620ff)? This is not easy to explain. The weakly stable case has two peaks, one in October and one in March. These peaks are primarily caused by the frequent occurrence of weakly stable cases at night - a feature which is not evident in winter or summer. Unlike in summer, it is too windy in March and October for very stable BLs to occur frequently at night so even in the case of some radiative cooling, weakly stable BLs form instead of very stable BLS.
5. Deficits exist concerning the discussion of the authors work with respect to previous studies on, e.g., ABLH climatology at higher latitudes. How do the authors results behave when compared to the cited studies of Liu and Liang (2010) and Seidel et al. (2010, 2012)? I would also like to bring to the author's attention the work of Lotteraner and Piringer (Boundary-Layer Meteorol. 161 (2016), 265-287 – concerning the deficits of Heffter's method), as well as of Beyrich and Leps (Meteorol. Z. 21 (2012), 337-348 – concerning the analysis of radiosonde data: many of the authors results on methods comparison, annual variability and uncertainties have been reported there as well). Liu and Liang (2010) combine multiple locations in many of their plots so a fair,

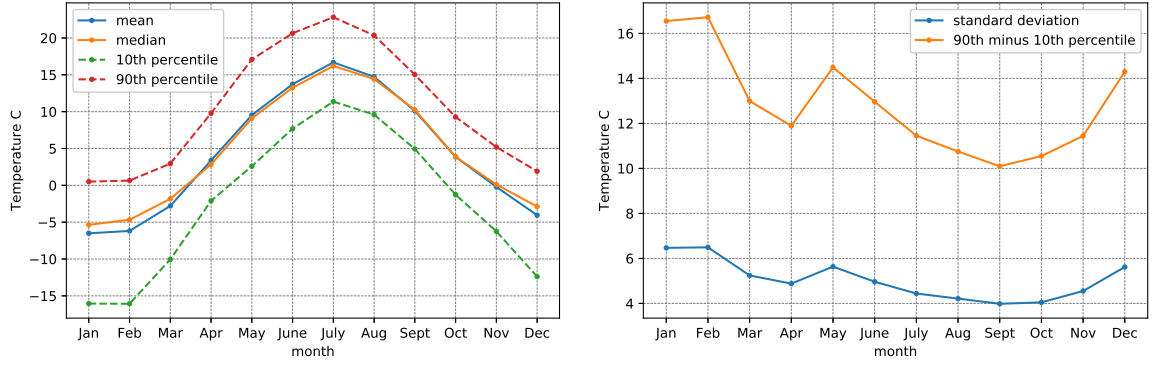


Figure 3: Left: The monthly mean, median, 10th and 90th percentile of temperature measured at 4.2 m at SMEARII estimated using data from January 1997 to December 2019. Right: The monthly mean value of the standard deviation in the temperature and the monthly mean value in the difference between the 90th and 10th percentile of temperature measured at 4.2 m

direct comparison is quite difficult. Liu and Liang (2010) find that for land stations, neutral BLs are more common than convective BLs during the day which is in agreement with our results. However, we find notably fewer convective BLs than Liu and Liang (2010). We have added a statement about this to section 3. One difference between our results and those of Liu and Liang (2010) is that while we find that the mean BL height is shallower in late spring and summer compared to winter, no such seasonal cycle is detected by Liu and Liang (2010). However, they only show this result for the Southern Great Plains site (36N) so this is not too surprising given that Hyytiälä has a much greater variation in day length and a very different climate in general. Seidel *et al.* (2012) exclude Arctic areas and only investigate European stations between 35 and 60N so a direct comparison is again slightly difficult. However, our results do agree quite well - summer daytime values of BL height averaged over all of Europe are deeper than what we found (potentially due to the lower latitudes) but the same seasonal cycle is evident. Seidel *et al.* (2012) also find that at night-time ERA-Interim gives consistently higher BL heights than obtained from the radiosonde soundings which is agreement with our results between ERA5 and the $Ri_{0.25}$ method. Furthermore, at night in Europe, both Seidel *et al.* (2012) and Beyrich and Leps (2012) find that the shallowest BLs occur in spring and summer which is in agreement with our results. We also find agreement with the results of Lotteraner and Piringer (2016) in that they also find that the Heffter method diagnoses deeper BL heights at 12 UTC compared to the Richardson number method and shallower BLs at 00 UTC. We have added more comparison to these studies in the discussion section of the revised manuscript in an attempt to better relate our results to these previous studies.

6. For the Figures, I noticed some issues concerning either the interpretation in the text or the methods chosen to display the data:
 - Figure 2 (related to text line 295ff): It appears not really possible to reproduce the ABLH values given in the text from Figure 2. At 00 UTC, I would not see any of the values at 307 m – all the lines appear to be well below 1/4 of the first height axis tick label at 1000

m. The same is with the inversion at the 06 UTC plot which appears to be well below 500 m. And at 12 UTC, the lowest plotted estimate is definitely below 1000 m whereas the text gives the minimum value with 1073 m. Thank you for highlighting this. The values reported in the text were the BL heights above sea level (which is how ARM reports them in the VAP files) whereas the values plotted are the height above ground level. Therefore the values in the text all had a positive bias of 179 m. The values in the text have now been corrected to be height above ground level and thus now agree with the horizontal lines in Figure 2.

- Figures 5-7, 9, 11: I am not sure whether the choice to plot the whiskers at $Q1 - 1.5 \text{ IQR}$ and $Q2 + 1.5 \text{ IQR}$ is a clever one given the skewed distributions of the variables, because this does not consider the real distribution of data points in the lowermost and uppermost quartiles. It may thus happen, that – especially the lower – whiskers cover a range where no data points occur. This becomes obvious by the many whiskers for ABLH ending at $z = 0$ m. I probably would have chosen to represent the 10% / 90% percentiles by the whiskers. This is a valid point and therefore we have re-made figures 5-7, 9 and 11. However, we have plotted the whiskers to represent the 5% and 95% percentiles as this reduced the number of outliers (and made the figures tidier) in Figure 5-7 compared to when we tried 10% to 90%.
- Figure 10: It does probably make not much sense to average the mean diurnal cycle of the stability classes occurrence frequencies over the whole year as the seasons behave very different. Yes this is true and it is one reason we also include figures 11c and 11d. However, to make figure 10 consistent with figure 9 we decided to keep panel 10b. We now note in section 6 / the discussion of Figure 10 the limitations of averaging the mean diurnal cycle of the stability classes occurrence frequencies now.
- Figure 12: I am not sure whether the correlation coefficient is a suitable measure for this type of analysis, at least if the authors chose this as the only parameter considered. From the seasonal distributions we have learned that derived ABLH values vary over a wider range in summer than in winter, this automatically gives higher correlation coefficients if at least the general tendencies are the same in the two data sets considered. Wouldn't it be an idea to consider absolute or relative differences here as well, the more since for practical applications it might be of higher relevance to know the area over which ABLH does not differ by more than a certain absolute (say 100 m or 200 m) or relative value (say 20 %), rather than to know correlation coefficients. Similarly, I doubt whether just from the value of the correlation coefficient one may judge about the spatial variability (lines 680f) Originally, we were more interested in the spatial patterns of the BLH 'evolution' and not as much in the BLH 'value' per se and this is why we used correlations. However, we find the idea of absolute differences to be very interesting as, as the reviewer states, this may be useful information in many practical applications. We have computed the percentage of times during each month and each synoptic time that the BL height at each grid point is within 150m of the BL height at the grid point closest to Hyytiälä. This information is included in a revised versions of Figures 12, S2 - S5 and is discussed in section 7. However, it is important to note that using an absolute value, rather than a relative value, does mean that this diagnostic is impacted by the diurnal and seasonal cycle of the BL height which

we also highlight in the revised text.

Minor Issues

1. The abbreviation SMEAR is introduced in the title already and it occurs in the abstract again before it is finally explained in line 73. We have removed the abbreviation SMEAR from the title and abstract.
2. The abstract could be shortened a bit. E.g. the first two sentences might be deleted, they would well fit in an introduction section, but are unnecessary in an abstract. I also consider it for unusual in an abstract to write “for example ...” – either the result is so relevant that it should be stated or it might be omitted. We have revised the abstract based on these suggestions and have also attempted to shorten it.
3. Line 36-37: Here the authors state that vertical profiles of temperature and wind speed are required to determine the ABLH, then they write that “these profiles can be obtained from ... ceilometers.” I do not know any ceilometer that would provide wind and temperature profiles. Correct, ceilometers only provide backscatter. We have deleted ceilometer here and also added text to be clear that the MWR only provides profiles of temperature.
4. Line 45-46: Another limitation of remote sensing instruments has to be seen in the fact that often more than one instrument is needed to measure the relevant variables, while tower measurements or sondes may provide the full set of essential thermodynamic variables. Yes, this is very good point. We have added text about this to the introduction.
5. The authors do not seem to be friends of using commas to structure a sentence, this makes it a bit difficult to read in some places (e.g., line 74, 632, 709, 737, and others) We have revised these specific cases and have re-read and edited the manuscript paying attention to the punctuation.
6. Line 119 / line 129: If M is the “main SMEAR station location” (line 110) in Figure 1, I would see the location R to the Southwest of M, not to the Southeast. This was a mistake and it has now been revised to southwest.
7. Line 207: “Stable boundary layer are not characterized by inversions.” – This is a statement I simply do not understand. We meant that, unlike convective, well-mixed BLs, stable boundary layers do not have a capping inversion above a well mixed layer. However, we agree this was a confusing sentence, as stable BLs are usually characterised by surface-based inversions. The start of section 3 has now been completely revised and this sentence is no longer included.
8. Line 246: Isn’t it a bit dangerous to consider just the immediate neighboring levels below AND above a given height to diagnose a low-level-jet. Often the jet nose may be not that sharp that there is a significant change of wind speed both below AND above the height of the maximum? Yes, we agree that taking just one level below and above could be problematic and would only capture very sharp jets. In the ARM VAP documentation (Sivaraman *et al.*, 2013) the word immediately is used. However, in Liu and Liang (2010) they state that “the LLJ nose is identified at the level where wind speed reaches a maximum that is at least 2ms^{-1} stronger than the layers above and below”. This suggests that it is not just the immediate levels above and below.

Hence we are not 100% sure which levels the ARM VAP considered. We have deleted the word immediately here as we think it is unlikely that only one level above and one below is considered.

9. Line 269: The use of z_0 is a bit misleading here, z_0 is normally taken as the aerodynamic roughness length. Yes, we agree that this is confusing and revised this. We have replaced z_0 with z_1 .
10. Line 278-280: The Vogelezang and Holtslag method additionally considers a correction term to account for surface friction / shear. Yes, we were aware of this and it is discussed in the IFS documentation. The method used in the IFS (and thus ERA5) to compute the Richardson number does not include the friction velocity term as *"since the friction velocity is not known from radiosonde data, the surface frictional effects are ignored in the computation of the bulk shear"* (IFS documentation). Therefore, the reference to Vogelezang and Holtslag (1996) is potentially misleading and we have removed it from this specific sentence. Furthermore, the Richardson number is computed using virtual dry static energy in the IFS whereas the Richardson number computed from the soundings uses virtual potential temperature. We have added some more details to the manuscript in section 3.3. about how the BL height is computed in the IFS / ERA5 and note more clearly how it differs from the Richardson number methods applied to the radiosonde.
11. Line 312: It is not clear to me why – “due to the inclusion of the wind profile ... the Ri number methods diagnose shallower SBLs”. Additional consideration of the wind profile would add shear-induced turbulence. Parcel methods will give ABLH where $\theta_h > \theta_0$ while Ri methods will lift this up before Ri exceeds a value of $Ri = 0.25$ or $Ri = 0.5$. In fact, parcel methods are equivalent to Ri methods with $Ri_{crit} = 0$, and with critical Ri values above zero they should give higher ABLH values. We agree that include wind speed and hence accounting for shear-driven turbulence should results in a deeper BL. We have deleted statement.
12. Line 327-328: I cannot follow this argumentation. The wind speed differences with respect to the near surface values are larger in the measurements when compared to the ERA-5 profiles up to a height of about 2.5 km, consequently the denominator in the Ri definition gets larger, and hence Ri gets smaller. It thus appears not logical that the threshold of $Ri = 0.25$ should be exceeded at lower heights. We have done further investigation of the case study shown in Figure 2 of the manuscript. It proved difficult to fully understand as we took the ARM provided values of BL height from the Richardson number methods and did not compute them ourselves. However, we now believe that the underestimation by the Ri methods at 12 UTC maybe related to the choice of surface values in this case or is affected by the vertical potential temperature profile which has some weakly stable layers present. Consequently we have revised the text in section 4.1 and removed this sentence.
13. Line 370: I would prefer to call this RMSD instead of RMSE. To use the word “error” would imply that the truth is known based on a well-calibrated method. We agree with this suggestion as in this study we do not have a measurement or model values that are obviously the “truth”. We are unsure if “D” stands for difference or deviation here (both appear in the literature) but we now now replace RMSE with RMSD (Root Mean Square Deviation).

14. L406-L408: How to explain the outliers in Figure 4 where $Ri(0.5) < Ri(0.25)$? We investigated this and found that there are only 7 cases out of the 827 soundings where the BL height diagnosed from the $Ri_{0.5}$ method is lower than from the $Ri_{0.25}$ method. The cases occur in the sondes released at the following times: 18 May at 0520 UTC (358.0 m), 17 June at 2316 UTC (13.5 m), 21 June at 1118 UTC (17.4 m), 26 June at 1117 UTC (36.2 m), 13 August at 0517 UTC (207.0 m), 15 August at 0519 UTC (13.5 m) and 22 August at 2332 UTC (124.3 m) where the numbers in brackets show the difference in the two diagnosed BL heights. These cases have $Ri > 0.25$ from the surface upwards to a low-level maximum in Ri above which Ri then decreases. Hence the profile crosses $Ri = 0.5$ for the first time at a lower level than it crosses $Ri = 0.25$. This situation arises when there is a very shallow stable layer at the surface with a well-mixed layer above it but below another stable layer. These cases may be affected by instrument error e.g. the surface initial values may be inaccurate (although they were not flagged by the ARM quality control flags.)
15. L458: I would not a-priori expect that the $Ri(0.25)$ methods radiosonde data must be in better agreement than the $Ri(0.5)$ method with the ERA-5 ABLH estimate, even if the latter uses the same threshold: Note that the profiles have different vertical resolution and the reanalysis profiles are usually much smoother than any measured profile which will strongly influence the level of exceedance of fixed threshold values. Yes, this is a good point and in section 3 we have added details to highlight the likely differences in the diagnosed BL height from ERA5 and from the Richardson number method applied to ERA5. We have also revised this sentence in section 5.2.
16. Line 466-467: 23% is a quite accurate number, I would not call that “approximately”. We have deleted ”approximately” here.
17. Line 609-610: This seems to be a bit too general, the very stable cases do occur more frequently in summer. Yes, this was too much of a generalisation here. This sentence has now been revised.
18. L643: “In winter cold conditions are required for shallow boundary layers to develop” – This statement calls for some further explanation. Sorry this was too generic and too vague a statement. We have now revised it to state that shallow BLs tend to develop under stable conditions, which usually occur under calm and clear conditions.
19. L669: It appears to be a subjective view whether Figure 12 really represents a “considerable part of Northern Europe”. We have now deleted the word ”considerable” here.
20. L724f: This conclusion does a bit contradict to the discussion 457-463 (“high vertical resolution ... sufficient“ vs. “limited vertical resolution” explaining disagreement) This is a small language issue and potentially we were not explicit enough. In Line 724 we did add ”in most situations” at the end of the sentence which to us suggests that there are some specific situations (i.e the shallow cases mentioned in lines 457-463 of the original manuscript) that the resolution is not adequate. We have revised this to be more explicit.
21. L735: This is not a suggestion but a rather logical consequence of the stability classification with the LL10 method as described in Section 3.1. We have replaced ”This suggests” with ”This means”.

22. L742-743: Again, a minor inconsistency: Here the authors, to my opinion, correctly point at the difficulties to determine the ABLH during the evening transition both from the measurements and in ERA-5, in Section 5.2 (line 496) they solely “blame” ERA-5 for the deficits. We think that radiosondes would more accurately represent the evening transition than ERA5 but it is likely that one radiosonde may not be completely representative of the atmospheric state during the evening transition when the BL structure changes rapidly. We have modified the text in the discussion to be clearer that it may not just be ERA5 at fault here.
23. L767f: I also see the widely missing diurnal cycle (no pronounced daytime heating) as an additional reason for the reduced occurrence of very stable situations in winter, except of the more frequent occurrence of clouds. We have added text about the lack of radiative heating during the day and its likely impact on the frequency of very stable BLs to the discussion.
24. Figure 2: Relative humidity is not a good choice to plot vertical humidity profiles for illustrating the ABL structure, absolute or specific humidity would be better here. In essence, the humidity profiles are not even needed to be displayed here, at least they are not discussed in the text. We have removed the relative humidity profiles from Figure 2 as we did not discuss these profiles in the text. In addition, we have also changed the scale on the y-axes to better show the structure of the BL at 00 UTC.
25. Figure 4: The diagrams give the impression that some lower boundary values exist for the LL10 method (no stable values below 40 m, no unstable values below ca. 150 m) – nothing has been mentioned about that when describing the method. For neutral or convective cases, the two vertical upward scans (to check for the required thresholds in potential temperature difference and potential temperature gradient) start from the data level right above 150 m a.g.l. to avoid noisy readings near the surface. Therefore, in the LL10 method, convective or neutral BLs cannot be shallower than 150m. However, for stable cases there is no minimum boundary value. To confirm this, we plotted the lowest BL heights which are shown in Figure 4 in this response. This zoomed in figure illustrates that there is no sharp cut-off at 40 m. We have modified the text in section 3.1 where we discuss the LL10 method and also remind a reader of the minimum in convective and neutral cases when we discuss figure 4 in the manuscript.
26. Figure 5: The black points are black crosses. The caption has been revised accordingly.
27. Figure 8: It appears unnecessary to explain the legend again in the text of the Figure caption. We have kept this information in the caption as it contains more information (e.g. that hourly median values are plotted) than is possible to fit into the legend.
28. Figure 9, caption: Just to make sure: panel (b) is stability from the sonic or from ERA-5? The data period represented by the graphs should probably be mentioned in the Figure caption, the more if it is not the same (ERA-5 40 years vs. sonic 19 years, as in Figure 10?) The stability data shown in Figure 9b is from the sonic. We have revised the caption to state this and to now include the time periods of the two data sets.
29. Table 3, caption: NMSE must be nRMSE, in the Table NRMSE should be nRMSE according to the definition given in the text (but see my remark on RMSE vs. RMSD) This has now been revised.

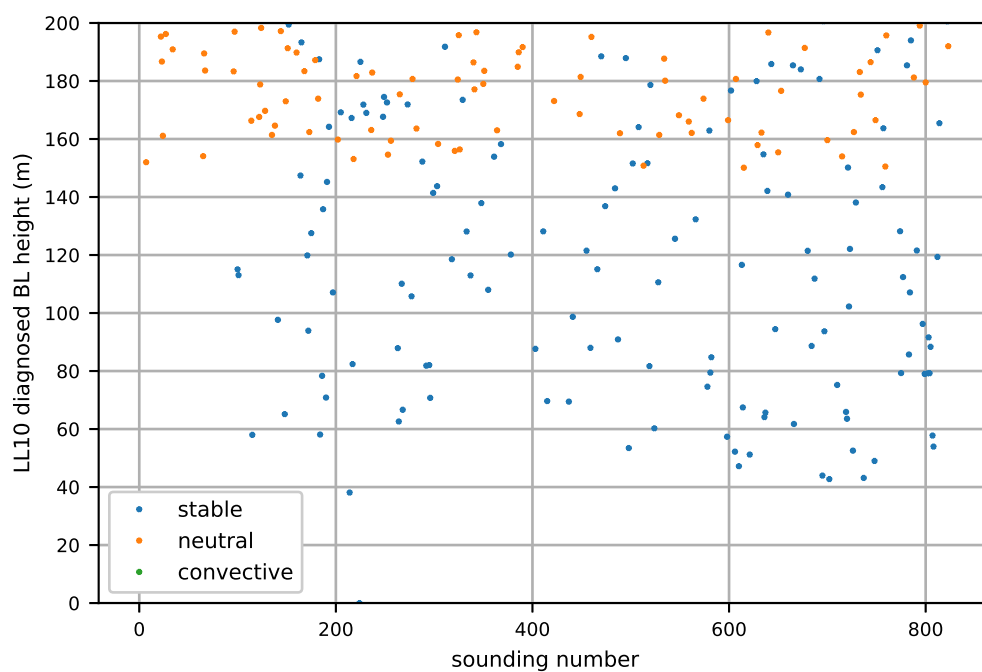


Figure 4: Boundary layer heights less than 200 m diagnosed by the LL10 scheme. Blue points show BLs diagnosed as stable by the LL10 scheme, orange points neutral BLs and green points convective BLs.

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