Comments on the manuscript

Atmospheric boundary layer height from ground-based remote sensing: a review of capabilities and limitations

submitted by

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General Assessment

The paper represents a comprehensive review of the current state-of-the-art of boundary-layer height estimation based on data from measurements with ground-based remote sensing systems. To create this survey can be considered as a very timely initiative, since publication of the last comparable review paper dates back for about ten years, and there has been considerable and significant development both in instrument technology as well as in data analysis and algorithm improvement during the last decade. Some of the authors of the manuscript decisively contributed to this recent progress in the field.

The paper is very well written and clearly structured. The text is the result of an enormous effort to collect and assess the relevant literature published in the field. As typical for a review paper, it does not contain really new results, however, it successfully integrates and evaluates the findings from a large number of original contributions. Besides the thorough discussion of the capabilities and limitations of different ground-based remote sensing techniques to allow for a determination of the boundary layer height, in particular the section on existing remote sensing networks can be considered as a great achievement.

In summary, I fully support publication of the manuscript. However, I also see quite a number of weaknesses which I would like to invite the authors to work on before the material gets finally published. These are specified in detail below.

Specific comments

1. The atmospheric boundary-layer height (ABLH) definition issue
   - In the first sentence of the abstract, the authors start with a definition which does not appear as general as it could be. This goes back behind Seibert et al. (2000) who, after intense discussion, replaced "pollutants" by "trace substances" (which must not necessarily be pollutants) and allowed heat, moisture or trace substances to be released not only at the Earth's surface but also inside the ABL or even from above (e.g., by entrainment).
   - In the abstract, the authors implicitly identify the ABLH with the mixing height, although later in the manuscript they interpret it differently (e.g., when they identify the ABLH as the top of the SBL height + RL height).

- Common ABLH definitions often introduce a time scale of interaction between the earths surface and the atmosphere. The word “rapidly” (L2) is not very specific in this context. And in section 1 (L103) it appears, that the authors do not want to decide whether to focus on processes with a duration of about one hour or, alternatively, one day – which is a significant difference. It becomes obvious later, that they seem to prefer the latter one.
- In the introductory chapter (L62ff) the authors start with introducing the ABL with a poetic description of some of their relevant characteristics. Again here, I would prefer to have a clear definition.

2. Section 2 on Measurement principles.
   - This section gives a comprehensive overview on the different profiling techniques. It is, however, not well-balanced. While microwave radiometers, Doppler lidars, and ceilometers are described extensively, most of the other techniques are treated quite short. This appears understandable considering the fields of scientific expertise of the authors and also the fact that these three techniques are obviously seen as the most promising candidates for more extensive networks. But then the authors should explain and motivate this from the beginning. Also, one gets the impression that the different subsections were contributed by different co-authors but not adequately harmonized in the final editing process.
   - In general, the instruments description could probably be a bit shortened by referencing to the instrumental literature – the recently published “Handbook on Atmospheric Measurements” (Ed. T. Foken, Springer) would be an ideal source of references for this.
   - I wonder about the so-called “automatic lidars”. Does one really need this term? And is it still appropriate today? A Doppler lidar or Vaisala’s DIAL, once properly configured, may run automatic as well over weeks, months … (as the authors also state – L531)
   - The affiliation of the single systems does not appear straightforward in any case. DIAL and Raman lidar are primarily thought to provide humidity and temperature profiles, but they are not discussed under “thermodynamic profiling”.
   - The sequence of presentation does not always appear logical. Section 2.2.1 starts with towers and tethered balloons, but instead of continuing with radiosondes (called the classical standard in-situ profiling technique in the introduction) the authors then switch to aircraft before coming back to the radiosonde. Similarly with the lidar systems: After a quite specific description of the principle of Doppler lidars, section 2.2.4 starts with a very general definition of a lidar system.
   - Concerning the radiosondes time resolution one might remark that the synoptic practice at weather services (to launch sondes typically at 00 UT and 12 UT) implies that the representation of typical ABL regimes depends on longitude. While we usually have a “good” SBL / CBL sounding in Central Europe, the standard ascents just represent the transition periods in certain regions of Asia and America. But to brake a lance for the sondes: They provide synchronous profiles of temperature, humidity and wind from just one sensor system, while with remote sensing one needs two or three (expensive) systems for that … The tracking error (mentioned in L261) is not that a big issue anymore since former radar tracking has been replaced by GPS in most networks today.

- To get a quick overview on the capabilities of the different systems with respect to range (lower and upper limits) and height resolution, another Table would be very helpful (or additional columns in Table 2)

3. The section on ABL height retrievals contains, to my opinion, a few errors and debatable points:
   - The statement about the Ri method in L664f is wrong: Why should the Ri method not be able to provide a layer height estimate in stable conditions? Ri exceeds its critical value somewhere in the stable range (Ri>0) and marks the level where negative buoyancy finally suppresses mixing by shear. In fact, it is seen as an advantage that the Ri method can be applied consistently to convective and stable boundary layers as it is done in many schemes applied to NWP output fields or radiosonde data. Also, the discussion later in this section (L736ff.) is irritating: How can the parcel method "provide good results under a wide range of meteorological conditions" if its application is limited to the CBL? And how can it be superior to the Ri method if the latter one is a generalization of the first one?
   - In general, I have the feeling that Ri number based ABLH estimation appears a bit underrepresented in the manuscript, as do other turbulence-based methods. In the conclusions section it does not occur at all. This is probably due to the fact that current schemes applied to single ground-based remote sensing systems do not allow application of this approach. However, in the outlook one could discuss that a synergy of different instruments might allow to use this approach in the future.
   - When mentioning the height of the SBI it should be noted that this might be accepted as a stability-based criterion, however when asking for the processes behind the derived values it might suffer from ambiguity since the upper part of a surface inversion is often governed from radiative cooling alone while turbulent mixing is present in the lower part only.
   - "The vanishing gradient in the air temperature profile marks the base of an elevated inversion" – I do not understand this: At the base of an elevated inversion the temperature gradient should become positive. And mixing might already be suppressed in a zone of constant or even slightly decreasing-with-height air temperature, if the potential temperature gradient becomes positive.
   - It is not the refractive index, but the refractive index structure parameter which is the primarily observed variable by sodar and RWP (L774/775). Later (L810ff) follows a redundant paragraph just replacing Cn2 by SNR (which is somehow equivalent), these two pieces of text might be put together.
   - Section 3.3 starts with a very positive assumption – can it be really taken as given that the distribution of aerosol and moisture is in parts a result of previous mixing processes? I would more generally say that the "distribution of … can be a result of either mixing, transport / advection, accumulation or formation processes or any combination of these"
4. The sensor synergy section provides a nice summary and synthesis, a few possible additions might be considered
   - When discussion the potential of sensor synergies (L1036f) it might be mentioned, in addition, that the availability of observations from several sensor systems could be used to assess and quantify the uncertainty of any ABLH retrieval and to add a quality flag to the derived values (as has been done for the different retrievals from radiosonde profiles by Beyrich and Leps, 2012).

- In Section 4.2 on the morning growth I miss the sodar which has been shown to be an ideal instrument to observe the CBL growth (at least in the early stages, up to a few hundreds of meters, depending on the instruments characteristics).
- The statement “radiosonde ascents are rare between sunrise and solar noon” (L1098) should not be made that general, first this depends much on the geographical longitude, and second the timing of the morning growth phase varies with the season. The more severe problem is the fact that radiosoundings just provide a snapshot-like picture of ABL structure which is a serious limitation during phases of strong non-stationarity, as the morning growth.
- Another too strong statement (L1107): “Analysis of RWP data allows for the detection of CBL morning growth” – this at least depends on the type of RWP. UHF profilers at around 500 MHz definitely miss the early stages of CBL growth due to a lowest measurement height at around 500 m.

5. Section 5 on ABL climatology goes, to my opinion, a bit beyond the scope of the manuscript. It illustrates what could be achieved once we will have available data sets from ground-based remote sensing instruments on ABLH over climatological time scales (30 years +). However, current discussion basically relies on either radiosonde and model data or on measurements over shorter time scales. The text in this section is mainly descriptive, touching many aspects without going into depth, and leaving questions open. Given that the manuscript has already a considerable length I would recommend to consider omitting this chapter and just discussing the climatology perspectives (with mentioning the different aspects covered by the subsections) in the conclusions section.
6. The Figures and Tables
    - Figure 2 appears too simple and partially confusing:
        - What is the relevance of the different colors – this should be explained.
        - The unspecified “exchange” symbols only occur at the top of the growing CBL in the morning – so no exchange between the Earth’s surface and the atmosphere, no exchange between the fully developed CBL and the FT?
        - Solar irradiance only occurs in the morning, but not later in the day, and radiative cooling only occurs around sunset, but not later during the night?
        - Why does wind-shear turbulence occur in the RL only, but not in the SBL (where it is usually the only mechanism to maintain some turbulence) or at the transition between the ABL and the FT?
        - The EZ symbol should be repeated at the top of the fully developed CBL.
        - I consider it for unusual that the development of the CBL starts before sunrise.
    - Also, Figure 3 is not in all aspects a successful illustration of typical ABL variables profiles
        - The vertical dashed lines are not explained, I assume that they should mark the geostrophic wind speed. If so, one may question why it is constant with height in the upper and lower row, but decreases with height in the middle row, this is an unnecessary complication.
        - The upper panel represents an early-morning CBL. Figure 2 suggests a thick EZ at this stage (which shall probably be illustrated by the three different colors along the z-axis representing the CBL <but this is not explained>). However, this does not go together with the well-mixed θv profile across these three sub-regions. If θv appears well-mixed, the sharp drop in tracer concentration at half the CBL height is not straight-forward to explain, nor is the large zone

of supergeostrophic winds across most of the CBL+RL. And the σw profile close to the surface appears to be non-typical for a (even shallow) CBL.
  - In the SBL (lower panel) it is suggested that the SIH and the height of the LLJ axis coincide, but this is not necessarily the case … And since w is zero at the surface, sigmaw cannot have a maximum there.
  - The horizontal yellow dashed line and shading is said to mark the ABLH. I feel this to be a bit irritating – it suggests that the uncertainty (variability?) in ABLH (illustrated by the shading?) is almost independent of the state of the ABL. Moreover, the behavior of the different variables at this mean ABLH appears a bit inconsistent: If θv shows a very sharp increase just at the mean (?) ABLH, why should c then be much more smoothly distributed? And why should c show a smooth transition on the top of the ABLH, but a sharp transition at the top of the CBLH when entrainment is expected to mix the profiles to some extent?
- Figure 6 does not really show results within the scope of the manuscript (ABLH from ground-based remote sensing), panels a) to c) are not marked in the Figure, even if one can easily deduce what is meant here.
- Table 1 could be optimized a bit, to my opinion:
  - I suggest to differentiate optically between atmospheric and so-called measurement variables either by sorting these types in two different columns or by printing the measurement variables in, e.g., Italics.
  - Variables representing basically the same characteristic might be grouped together, e.g., the different temperature and humidity variables, wind speed and the wind vector components etc.
  - I suggest to replace “dynamic processes” by “dynamic and turbulent processes”
  - What about turbulent fluxes or combined indices, like Richardson numbers, which may be used to detect the ABL height as well.
- Table 2 suffers from some omissions
  - DIAL is not primarily used to measure temperature (also not mentioned in the text)
  - Raman lidar may also used to measure temperature, and – of course – all the aerosol parameters listed for an aerosol lidar
  - RWP basic measurement variables are missing: Cn2, vr, also TKE could be derived
  - Sodar can also measure vr, u, v, w, sigmaw,
  - The list of network operations might call for a list of acronyms.

Minor Issues

- L74: The poor time resolution of (operational) radiosondes is not the only disadvantage of this technique.
- L78: It would be good to give some rough numbers on the “entire ABL vertical extent” already here for orientation.
- L123: It is not only the height of a surface inversion which is meaningful for vertical dilution processes but the internal structure of that inversion as well.
- L133-134: What about the role of wind shear for the exchange between the ABL and FT?
- L138: Shouldn’t that be “RL or the CBL” instead of “RL or the SBL”?

- L142: “may be located” – this sounds rather vague. If Cu clouds form in the ABL, isn’t the ABLH than normally above the CBH?
- L191: The link to the Rn measurements either calls for some additional explanation or it should be omitted here.
- L238: Wind speed and wind direction from radiosondes haven’t been measured with sensors lifted by the balloon for decades, they are derived from the replacement of the balloon tracked by radar or GPS.
- L266ff: This description of a RASS is a bit too simplified and not fully correct. First there are two types of RASS – a radar wind profiler + an acoustic source and, alternatively, a sodar + an electromagnetic source. Second, with the RWP as wind measurement system, the acoustic component is normally not operated as a sodar but just provides the sound signal detected by the electromagnetic component. Furthermore, with RWP, the sound source is normally not operated in pulse mode. Also the given range limitation for RASS temperature measurements (L273) are not true in a general sense, but depend on the system characteristics: While this limit applies to 1-GHz RWP with RASS capability and to sodar-based RASS, a RASS using an RWP at around 500 MHz allows temperature measurements well above 1 km
- L270: Görsdorf and Lehmann (2000) achieve this accuracy by applying a number of careful corrections, the authors might wish to mention this.
- L288f (and others): I would recommend to be cautious with the term “root mean square error” (RMSE) which implies that the truth is known – a difficult issue for measurements in the real atmosphere with different instruments relying on different measurement principles, it could be better to use the term “root mean square difference” (RMSD) instead.
- L340ff: The discussion of rmsd and bias values between different instruments is just half of the truth since the determination of the ABLH height in many situations is based on gradients rather than on absolute values.
- L359: For ABLH determination from sodar measurements see also Beyrich (1997).
- L357ff (sodars): The possibility to measure wind and turbulence (at least σw) with sodars is completely missing in the discussion here (see also Table 2). It might be confusing for the reader to read here that a sodar primarily measures CT2, while in Table 2 Cn2 is listed. Concerning the limitations of sodars, besides the emission of sound, at least equally relevant is the sensitivity to environmental noise.
- L451-455: This reads if DWL would have no operating limitations (“can operate under all weather conditions”). First a certain aerosol load is necessary to create backscatter. Second, rain typically distorts the measurements and makes vr derivation difficult or impossible. Third, DWL does not provide profile information much above the base of low clouds – a severe limitation in winter in many regions.
- L521: Isn’t that a bit low? – ECMWF data coverage survey typically gives 500-700 temp observations at 00 UT.
- L546: None of the 128 RWPs in China is visible in Figure 4c.
- L618ff: I would not say, that “radiosondes may observe spatial variations in the ABL dynamics” this would require measurements at the same height at different places (as with a scanning lidar). I would instead say that “Vertical profiles derived from radiosondes may be influenced by spatial

variations in the ABL dynamics and do not necessarily represent the ABL structure just above the launch site.". In addition to surface heterogeneities and the synoptic flow, also orography may play a role here.

- L658: Can Ri number methods really be grouped as temperature-based approaches to determine the ABL height? The wind profile significantly affects the Ri based retrievals.
- L732: One may add that these retrievals are typically most uncertain during the evening transition period.
- Section 3.1.2: The uncertainty analysis and quality flags provided by the scheme suggested in Beyrich and Leps (2012) might be mentioned in this subsection as a possibility to characterize the limitations of these (thermodynamic) methods.
- L785: What are "low atmospheric waves"? Do they occur at low altitudes or do they have low frequencies?
- L816: For the sodar – RWP sensor synergy the paper by Beyrich and Görsdorf (Boundary-Layer Meteorol. 76, 1995, 387-394) would be a nice reference.
- L836: It could be made a bit clearer that the effect of rain is twofold here: First the measurements of w or radial winds can be corrupted by contributions from the fall speed of rain drops. On the other side, rain on the antenna (e.g., of a sodar) or telescope (e.g., of a DL) might generally prevent a useful retrieval.
- L995: "… passage of synoptic fronts." – One may remark that the ABL is generally poorly defined under these conditions (the "gust weather" or "dissolving" ABL type according to Schneider-Carius, 1953) which are dominated by larger scale processes such that the surface – atmosphere interaction is of minor relevance.
- L1127: "… agree within a few hundred metres …" → add "… or even less" – A few hundred metres would still be a considerable uncertainty, but values of 50 m – 150 m are possible for a well-developed CBL with a clear capping inversion (see also line 1514f).
- L1172f: Following the definition of the EZ, the determination of its thickness from temporal or spatial variations (spatial variations underly the original water tank studies analysed be Deardorff in the 1970ies) of CBLH would be the preferable way – gradients of mean quantities are a secondary approximation.
- L1262-1263: I am not sure about this generalization – First: What is "high latitudes", second the cited paper refers to the marine ABL concerning the large seasonality over high latitudes: "**Marine PBL depth** has a somewhat similar seasonal march to that over continents but is weaker in amplitude and is shifted poleward. The **maximum seasonal amplitude over oceans** occurs over the Arctic."
- L1290f: Cloud dynamics certainly affect CBL growth rates, however, other variables are of relevance as well, such as moisture content of the ABL, lapse rate in the FT above the ABL, (downward) synoptic-scale vertical motions …
- L1348ff: many of these studies cannot really called "long-term climatological observations".
- L1463f: Lower stability would imply an increasing vertical temperature gradient, wouldn't it?
- Concerning the cited literature I may additionally direct the authors attention to a paper by Zhang et al. (2020): Research progress on estimation of the atmospheric boundary layer height. J. Meteorol. Res. 34, 482-498, doi: 10.1007/s13351-020-9910-3

Some language and technical issues

- L138: Probably better replace “gradient” by “difference” here.
- L168: insights on → insights into
- L170: “tracer variables may portray” (they do not necessarily …)
- L190: record → recorded
- L191: eventually replace “atmosphere” by “atmospheric volume”
- L251: add “worldwide” to the “177 sites”.
- L267: “The frequency … is varied”
- L491: “wide range of models”
- L502: “ABL layers” is doubling the “layers”
- L507: delete “of” after operators
- L536-37: The official name of DWD in English is: German Meteorological Service.
- L547: Close the brackets after JMA
- L547: covering → utililizing (or: working at)
- L555: “and they are the major …”
- L593: “at higher altitudes”
- L594: “… first range gates”
- L658: Vogelezang and Holtslag were definitely not the first that introduced the Ri methods, thus write “e. g., “ or introduce primary reference.
- L662f: better write “… parcel methods that additionally considers the shear contribution to turbulence generation.”
- L695: Who is “L.”?
- L774: “… can be diagnosed from the refractive index structure parameter”
- L796: “scanning-strategy”
- L822: What is “random forest machine learning”?
- L877: Add a comma after “backscatter” to make this better readable.
- L918: Tennekes is only once the author of that paper.
- L947: Cohn and Angevine were definitely not the first to estimate EZ thickness, thus write “e. g., “ or introduce primary reference.
- L1169: add “… (accurately) by ALC”
- L1177: thickness
- L1283: add “h” after the times given (or write 1200 LT etc. instead)
- L1445: daily → daytime?
- L1500: turbulence-based
- L1536: add “to” after “expected”

Recommendation: Major Revision

Frank Beyrich (Lindenberg), 24.5.2022