

We thank the reviewer for the positive response to our manuscript and the valuable feedback. Below, we respond to all comments point-by-point.

The paper “Atmospheric boundary layer height from ground-based remote sensing: a review of capabilities and limitations” by Kotthaus et al., provides a summary of state-of-art boundary layer height estimates using ground based remote sensing systems. Such reviews are important as there has been significant development in novel techniques and instruments, and many papers go un-noticed unless highlighted by such a review. The authors have done a really nice job going into details of PBL definitions (some clarifications needed), type of instruments, new algorithmic developments and current deficiencies.

The reviewer does have some additional clarifications, and comments which would be helpful if addressed in the revisions.

Major Comments:

1. Definition of ABLH is mostly provided from a lens of onshore applications or land-based ABLH, how do these definitions hold up in an offshore marine boundary layer? There needs to be some discussion on that front in the initial sections (Section 1.1).

Extracting information from the climatology section of the previous version of the manuscript, we have included the following paragraph into Section 1.1 (line 119ff)

“The marine ABL exhibits similar dynamics as the boundary layer over land, however, the lower variability in sea surface temperature, marine air temperature, and surface sensible heat fluxes cause structural differences in layer heights. While the daytime maxima in MBLH are smaller compared to over land (McGrath-Spangler and Denning, 2013), the daily average marine MBLH is deeper overall (von Engel and Teixeira, 2013; Davy, 2018) due to its reduced diurnal amplitude (Medeiros et al., 2005; Liu and Liang, 2010) explained by a persistent capping inversion (Medeiros et al., 2005; von Engel and Teixeira, 2013; Chan and Wood, 2013; Luo et al., 2014; Sathyanadh et al., 2017). CBL conditions are more frequent over the oceans with unstable stratification even persisting at night. The neutral RL regime is more rare over the ocean during daytime, with its frequency reduced by 20 %–33 % compared to the average land boundary layer (Liu and Liang, 2010).”

2. In Figure 3, it is indicated that the vertical velocity variance estimates can be used to estimate the stable boundary layer depth, the reviewer is not aware of many papers discussing/showing that methodology (except for Pichiguina and Banta 2010). So not a widely accepted methodology, as the vertical velocity variance is very low during nighttime conditions. Please provide appropriate citation for other references or reconsider this statement.

As you mention correctly, the vertical profile of the vertical velocity variance shown in Figure 3c was incorrect. This was also pointed out by Reviewer 1. We have adapted the figure accordingly.

3. The latest COSMIC-2 satellites have better spatial and temporal resolution and can provide better boundary layer height estimates compared to legacy COSMIC/other RO satellite data. Please mention something about COSMIC-2 to the reader, I don't see that in the article.

We added the following sentences (line 202ff):

“Following the success of COSMIC, the promising COSMIC-2 mission was launched in 2019 to provide radio occultation data at even higher through deeper tropospheric penetration (50% within 200 m of Earth’s surface). These observation enable improved detection of the ABLH and superrefraction at the top of the ABL (Ho et al.,2020; Schreiner et al., 2020).”

4. Table 2, Network operation, the Atmospheric Radiation Measurement (ARM) network is missing in the list.

Table 2 has been updated.

5. What is the importance of knowing ABLH during daytime or nighttime transition periods? That needs to be discussed, as models tend to deviate significantly during those time periods. Do we expect a given instrument to perform well during those transition periods?

Both morning growth and evening decay are discussed in Section 4 on the monitoring of the diurnal cycle of the ABL. We have added the following sentence to the conclusions (line 1323):

“The morning growth and evening decay of the CBL also poses challenges to numerical simulations for a range of applications, including air quality, greenhouse gas assessment and numerical weather prediction. When using observations for model evaluation or comparisons, it is crucial to carefully consider the specific uncertainties of the respective measurement used. Also, it is important to understand which atmospheric variable is used for layer detection, as it can introduce systematic biases if e.g. turbulence-derived layer heights are compared to results exploiting aerosol profiles”

6. For DWL, another issue is the power of a given scanning Doppler lidar to reach the boundary layer. Some low powered scanning DLs fall short of reaching the ABLH during convective conditions, due to attenuation of the signal, increased Cn2 effects, instrument noise, etc.

A more general description of lidar systems and their capabilities is now presented in lines 160-174, which highlights this point more clearly. It is also repeated in the DWL section and in the conclusions.

7. The NY Mesonet network in the US is missing:
http://www.nysmesonet.org/data/profiler#stid=prof_alba. They have DLW and MWR profilers.

Thank you for pointing this out, The NYS Mesonet MWR are now shown on Figure 4 and mentioned in Table 2.

8. Offshore ABLH should be given a separate section here, as there are challenges in measuring them due to trapped aerosol layers, internal boundary layers, coastal effects etc.

Section 5 has been deleted from the manuscript.

9. ABL climatology sections seems out of place here. Once you define these advantages and disadvantages, the climatology will be digested with a “grain of salt” by the reader. Unless you put some uncertainty plots etc. I would encourage the authors to reconsider this section. Perhaps another climatology article would be most appropriate. This would also reduce the length of the article.

We understand that section 5 can be regarded to go beyond the scope of the current publication. Also to shorten the manuscript and make it more accessible to the reader, we are following your suggestion to remove the chapter and all its mentions in the abstract and other sections.

Minor Comment

1. I am not a big fan of Table of contents for a journal paper but will leave it to the editor to decide. Figure 1 encapsulates this nicely.

We agree with your comment and removed the Table of contents for the final publication.