Referee Review: *CALOTRITON: A convective boundary layer height estimation algorithm from UHF wind profiler data* by Philibert et al., 2023.

General Comments

This manuscript reports on an algorithm to estimate the height of the atmospheric convective boundary layer (CBL) using measurements from a radar wind profiler (RWP). Measurements were taken at two locations for a period of 22 years. The algorithm is validated by comparison with radiosonde observations. The topic addressed by this manuscript falls within the scope of Atmospheric Measurement Techniques since it concerns the use of ground-based observations to estimate the height of the lowest layer of the atmosphere. The manuscript in its present form however has several shortcomings, as summarized below.

The methods used in this work are based on valid physical concepts that have been used extensible to estimate planetary boundary layer heights by many researchers since 1994. Limited results are discussed. The authors state that one of the aims of developing their algorithm is that can be used to obtain a long-term series of daytime estimates of CBL height, yet the manuscript only shows a few days' worth of data, which weakens the argument. The algorithm uses information provided by the RWP measurements and meteorological data to handle most (or as many as possible) conditions that can be encountered in the boundary layer (clouds, precipitation, other interference such as birds, etc.). This approach provides restrictions that may make the automatization of the method rather cumbersome. There are already other algorithms (simpler) that estimate CBL heights from RWP measurements, using backscatter or signal to noise ratio (equivalent to using the air refractive index used here) that have proven to be robust and reliable and have been used with long data records and applied over large geographical areas. In addition, consideration to related work (including appropriate references) are not appropriately given.

The title reflects the contents of the paper, and the abstract provides a concise and complete summary.

This manuscript is not a review paper but a report on a new algorithm to estimate CBL heights from a particular type of radar wind profiler (RWP). With that in mind, I would suggest that the Introduction does not need to be so 'sub-sectioned' as presented. Standard definitions of the height of the planetary boundary layer can be found in textbooks and existing methods to estimate planetary boundary layer height from measurements (of which only a few are mentioned!) can be found in many recent publications, particularly in the most recent review article published in AMT by Kotthaus et al. (2023) and references therein. A few paragraphs in the Introduction, specific to the present article would suffice.

In sum, in terms of scientific significance and quality and in terms of presentation quality, I rate this manuscript as 'fair'.

Specific Comments

The authors do indicate their own contribution. The authors do not give proper credits to related work; therefore, I suggest that references be reviewed to address this shortcoming. For example, see Section 2.2.3 and parts of Section 3 of Kotthaus et al., 2023. I note that the latter article is cited (page 23, line 425, in passing, related to what appears to be future work? it is not clear) but it is my opinion that the article is more pertinent to the present work than expressed by the authors.

Page 4, line 75, the authors state "However, this technique is not robust enough for statistical studies based on long series." This is an inaccurate assertion. (1) If by 'this technique' the authors mean the exact methods/calculations performed by Angevine et al. (1994), then I need to state that I am not aware of any long term (or large geographical extent) study of this kind with RWPs data, which it does not demonstrate that the technique is 'not robust enough' but that such a study has not been done. (2) Using RWPs measurements to retrieve CBL (or PBL) heights with approaches/techniques (or algorithms) that basically follow the same method than that of Angevine et al. (1994) do exits and have shown to be robust and reliable even in the presence of clouds (see for example Teixeira, J., and Coauthors, 2021: Toward a Global Planetary Boundary Layer Observing System: The NASA PBL Incubation Study Team Report. National Aeronautics and Space Administration, 134 pp; https://science.nasa.gov/science-red/s3fs-public/atoms/files/NASAPBLIncubationFinalReport.pdf, and references therein).

The overall presentation of this manuscript is NOT well-structured and clear. Some parts of the paper (text, figures, tables) should be clarified, and others reduced, combined, or even eliminated. See comment about Introduction.

Page 3, bottom: " … existing technique based on Angevine et al. (1994) was used so far for the estimate of Zi with this instrument." What 'existing technique' is being used here? By the authors? applied to the data reported later? Is this the technique reported by Angevine et al. (1994)? Please clarify!

Section 2: A map indicating the locations (lat, lon) of the instruments should accompany Table 1.

Figures 3 and 4 (with text in pages 6 & 7, lines 110-115) show variables that are not defined until 11! Yet they are used to make arguments about comparison. The figures themselves are quite hard to 'read' and follow and not having variables defined make the work of the reader (reviewer) extremely hard!

Figures 2, 4 and 7 are very hard to follow! The dark shading obscures the superimposed line plots and it is hard to follow the lengthy caption in these figures, particularly Fig. 4. The vertical dashed line that corresponds to the time of radiosondes measurements needs to be made clearer in Fig. 2.

Figures 3 and 8: too hard to follow, lines need to be thicker perhaps. In addition, Fig. 3 uses measurements from two different days – one noted as a clear day and another more complex situation with clouds, etc. Do both days need to be in the same figure? May be making larger panels and separating the two days will help.

Then Fig. 8 caption refers to Fig. 3 ('same as Fig.3 …'); Fig. 3 is on page 7 while Fig. 8 is on page 19!! The reader is expected to scroll nearly 10 pages to understand and follow Fig. 8?!

Line 345: "From 14:00 UTC onwards, a low-level marine breeze (< 500 m) can be seen on the Fig. 7a and 7b." What exactly indicates in this figure that we are observing a 'low-level marine breeze'?

Figure 9: What significance is given to comparisons with the CBL height computed from thermodynamical variables as measured by radiosondes? Why not use the Richardson number method (or bulk Richardson number commonly used), which is more appropriate for CBL conditions? The results shown in panels a through d in Figure 9 are to be expected and add no meaningful information.

Section 4.2 could be shortened and more concise (and clear!)

The manuscript would benefit from a more defined 'summary and conclusions' section, which rather than discuss initial objectives (repetitive to some extent) would summarize the main findings, contributions, and innovative aspects in this work. As written, all of these are hard to determine.

Technical Corrections – Minor Comments

Page 2, line 2: "CBL top (Zi) is a key variable in air quality since pollutants, dust, smoke,... emitted" Is these '…' a typo, an error, of it means more elements? May be better to use 'etc.' or to list items specifically. This is seen again on line #45 on page 3.
Some editing, mainly for clarity in English, will help the text. For example, on line 120, page 7 reads "Figure 3 (panels h to n) confronts in situ measurements of thermodynamical …", perhaps the word needed here is 'compares'.

Line 220: Define CBH. I assume it stands for 'cloud base height' but it needs to be stated.