Reply to Referee #2

We thank Reviewer #2 for his/her useful feedbacks, which made us improve the manuscript.
We have significantly revised the manuscript.

Below is an answer point by point to the review.

General Comments

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.
It is true the manuscript was lacking of previous works on those techniques. We sigificantly enriched the introduction.

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 few cases are used as illustrative examples. Those are very helpful to highlight the capacity of the algorithm, and the challenges of building an automatic algorithm.
An analyis of the resulting results is not in the scope of this paper. A climotology was indeed realized in Lothon et al 2023, for the characterizatin of the P2OA-CRA site. But is is not the purpose of the present article to make those analyses.
However, many days are actually considered when optimizing the algorithm, or comparing it with in situ profiles.
In the revised article, we made clearer the time period of the used dataset, according to its role.

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.
Those techniques are indeed very interesting for their simplicity (for many of them) and for their relative robustness. But those would not manage in some complex cases that are pointed here.
All algorithms and approaches face the same issue, in link with the atmospheric vertical structure complexity. And the simplest algorithm can definitely not deal with it.
We have more discussed the existing methods published in the literature, which have common issues and common principle with the algorithm presented here.

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.
We had already simplified the introduction structure at the submitting pre-review process.
Here we have decided to keep the two sub-sections of the introduction.
However, we did remove the part relative to the definition of the CBL and more rapidly converged toward the CBL depth measurement techniques.
Before this, we stressed on the importance of this variable, following a suggestion of Rev #1.
We also added several references to the RWP-based Zi retrieval techniques, following both reviewers suggestion.
We cited Kottaus 2023, which we had not in mind when writing our manuscript. We thank the reviewer for suggesting this very relevant article, which we missed during the writing process. Our background discussion now starts with this review paper.

Specific Comments

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.
We agree that this was a lack of our submitted manuscript. We did enrich the references in the introduction, and also along the manuscript, when discussing results and in the concluding discussion. Kottaus 2023 is part of the references, and used as an exhaustive review. We notably citet and discussed the work of Heo 2003, Compton 2013, Collaud Coen 2014, Molod 2015, Bianco 2002, 2008, Liu 2019.

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.
This citation of Kottaus et al was not the same article as you mention here. The reference of Kottaus et al 2020 (and not 2023) was actually relevantly made here for the discussion about the STRATFINDER algorithm they developped, and the reasons for differences within the various Zi retrieval estimates.

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.*
We strickly speaking agree with you here. But we also state that this approach could not be used on large statistical dataset without large errors. This is actually shown by Grimsdell and Angevine, 2002. And the illustrative examples that we give also demonstrate this. Defining Zi as the absolute reflectivity maximum will lead to erroneous attributions, in case of residual layers above, with inversion more marked than the CBL top.
We revised the way to discuss this point (see page 4, lines 90 to 107).

(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).*
We thank the reviewer for the reference. Actually, the publication of Teixeira et al does not specifically address this subject. However, it cites Molod et al 2015 who did applied a "robust" simple algorithm for the statistical study of the composite diurnal cycles Zi in a large area from RWPs (about 30 stations in USA), and over 5 years.
Note, though, that Molod et al 2015 restricted their study to the months of June-July, and find large differences between thosethe Zi estimates made from the in situ radiosoundings based on the bulk Richardson method, and the RWP-based estimates, even on avearge iver five years. Indeed, the bias is about 250 m in average, but can reach more than 700 m at some places (still in average over 2 months and 5 years) – See their Fig. 6 and 7. Also two locations, judged as "complex" are ignored. This means that the point of view is very different in Molod 2015: the idea is to have a global view, with a climatology of the diurnal cycle, and finally work with quite approximate estimates. And the evaluation is made at this scale.
But a finer comparison would reveal the exact same issues encountered in complex – but very common – situations, as shown in our study.
Note, moreover, that Molod et al used a similar approach for an improved estimate of Zi, with a correct start of the CBL growth: they search for an "emergence time" at the first available gate. This is very similar to what we have done. It also chases a local maximum with criteria on the temporal continuity. The main difference is on the key considered variable (SNR in their study, and NPx in our study, which is one innovative aspect brought by CALOTRITON).
For all those reasons, our study and that of Molod 2015 are very complementary, and mutually benefit from each other.
We have citet this reference at several places in the revised paper.

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.
It is true that the organization of the manuscript was not optimized. We have profoundly revised this structure, moved figures, made them clearer, added a table. Several of those changes also answered to Rev#1 specific suggestions.
Introduction was revised, as mentioned earlier.
The description of the context and data was made clearer.
The description of the method was also made clearer, and we removed the illustrative examples from this section.
We made a specific section on the 3 illustrative examples.

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!*
We made this sentence clearer in the revised version.

Section 2: *A map indicating the locations (lat, lon) of the instruments should accompany Table 1.*
We do not find appropriate to give maps here. But we improved the explanation of the experimental devices and gave references for precise maps.

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!
We have now moved those figures later in the manuscript, in a dedicated section to illustrative examples, and after all variables are defined and the algorithm explained. We also improved the clarity of the figures.

***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.*
We have improved the clarity of those figues: we kept only 4 panels over 6, softened the background colours, chose more visible symbols, lines and colors on the front.

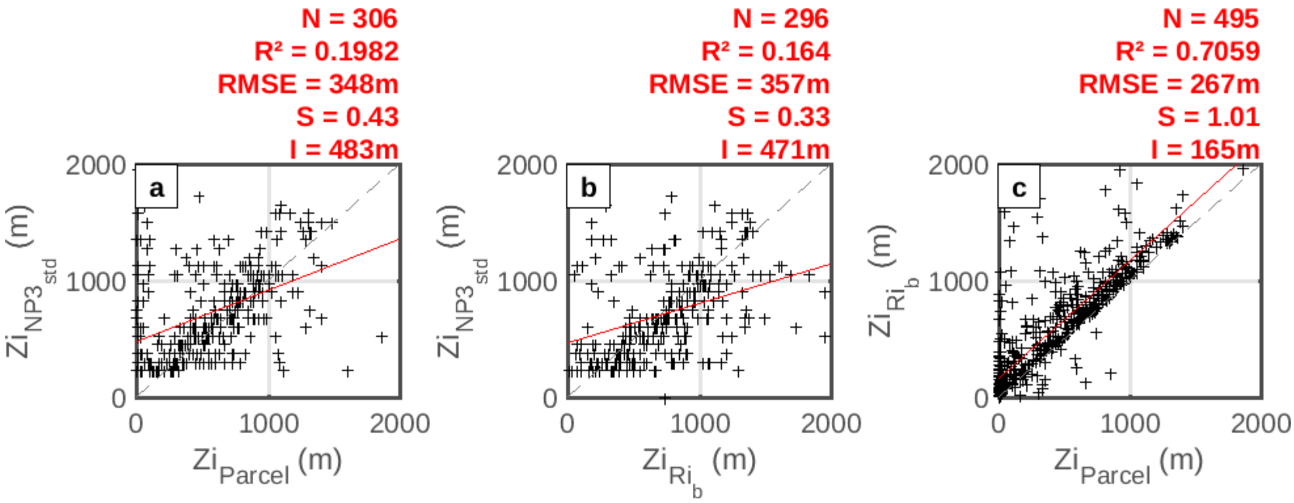
***Figures 3 and 8:** too hard to follow, lines need to be thicker perhaps. In addition, Fig. 3 uses measurements from two different days – one noted as a clear day and another more complex situation with clouds, etc. Do both days need to be in the same figure? May be making larger panels and separating the two days will help.*
In the revised manuscript, we did separate the two days, so that they are displayed at the same location in the manuscript as their comments.
We also improved the clarity of those figures, by thicker lines and symbols, as suggested by REV #2, and also lighter log grids.

***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?!*
In the revised manuscript, we moved the figures later in the text, after the methodology has been explained. Initially, we thought that those illustrating figures were necessary for the understanding of the algorithm. Now, following Rev#1 and Rev#2 suggestions, we moved them after the methodology. They are now in a section which illustrates the capabibily and limitations of the algorithm, with the three cases examples (clear at P2OA, cloudy at P2OA, clear but complex in LIAISE). This way, the figures are well associated to the associated discussion.

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'?
We can see this marine air setting at 14:00 UTC (not shown). It is well seen from the RWP data starting 15:00 UTC, and its associated lower temperature and higher moisture are also well seen on the 18:00 UTC sounding.
This feature is very typical of the area in summer, and is actually called "La Marinada" by the forecasters and local people. We have added a recent reference on this typical local feature (Jimenez 2023).

***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.*
For the CBL, the parcel method is one of the most common ones, which fits to the approach we have with CALOTRITON. The gradient approaches also are interesting to consider, since the radar echoe will be very sensitive to the inversions. So it remains interesting to check how those are manifested in the radar signal.

The bulk Richardson method is interesting because it combines the wind gradient and the temperature gradient. But we did not initially consider it as a reference, due to previous experiences where it revealed not to be the most appropriate. It is actually usually used when one also wishes to detect the top of the noctural boundary layer, which is not the topic here.
However, we have tested it more closely following your remark.
Below is a comparison between (a) Zi_NP3_std and Zi_parcel, (b) Zi_NP3_std and Zi_Ri, and (c) between Zi_parcel and Zi_Ri.



This figure shows that there is no big difference between the results of (a) and (b). In other words, using the parcel method or the bulk Richardon method as a reference for the validation of Zi_NP3 leads to the same message. Also panel (c) shows that both methods agree a lot, except that Zi_Ri more often over-estimates Zi (catching upper inversions).
Thus, there is no significant change of our results if we consider the bulk Richardson method rather than the parcel method. We prefered to keep the parcel method as a common reference.
We added a comment on this aspect in the revised version (page 23 lines 476 to 479).

Also our true reference here is the subjective way, because, for now, there is no better method. And if we can manage with it (if the set of data is not too large), it is worth using it.
Our work actually demonstrates that there is no perfect objective/automatic technique from the in situ thermodynamic profiles. And the bulk Richardson method will not make this message different. This is also why for example Bianco et a 2008 also used what they called the "expert" estimates, which are down by eye, from the RWP data, to validate their "fuzzy logic" method.
We have made this point clearer in our discussion of the results

Section 4.2 *could be shortened and more concise (and clear!)*
We have revised this section by removing sub-sections, by simplifying the discussed figure (Fig. 11 in the revised manuscript), and by being more concise. This part is now a section by itself, since we moved the illustrative examples in one separated section (previous section 4.1 becomes section 4, and section 4.2 becomes section 5).

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.
We have significantly revised the conclusion, and called it "Summary and discussion". We followed Rev#2 suggestion to insist more on the main findings and innovative aspects of CALOTRITON.

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.*
This sentence has been removed.

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’.*
We have corrected those words, and revised the editing with a Native English research scientist.

Line 220: *Define CBH. I assume it stands for ‘cloud base height’ but it needs to be stated.*
We have defined this acronym earlier in the text (page 12, line 265)