Response to the Comments from Referee #2

We sincerely thank the reviewer for the valuable feedback and the references that we have used to check our newly-developed algorithm and improve the quality of our manuscript as well. The reviewer's comments are laid out below in *italicized font* and specific concerns have been numbered. Our response is given in normal font.

Summary

Chen et al. use a mini Micro Pulse Lidar and ceilometer in order to document diurnal changes in the boundary layer height at a coastal location in Australia which is subjected to maritime and continental airmasses. They document three case studies, which are representative of distinct air mass sources, in order to understand the observed boundary layer structure. Chen et al. supplement the observations with WRF simulations and discusssome of the similarities and differences between observations and simulations. The authors also employ sodar observations in the lowest few hundred metres in order to quantify small-scale wind changes and their role in altering the boundary layer.

Overall, the manuscript presents new results in a poorly-sampled region of the world, where additional observations and insights of the boundary layer may help with constraining in model simulations some of the well-known issues over the Southern Ocean. Yet, there are some major problems with the manuscript in its present form which need to be addressed comprehensively prior to consideration for acceptance. These revolve mainly around incorrect (and incomplete) algorithms to properly extract the correct boundary layer height (BLH), which is central to the whole manuscript, an ill- defined 'manual checking' procedure, and an apparent lack of cloud screening and lidar / ceilometer backscatter profile removal before BLH detection algorithms are implemented. Idetail my comments below.

The full citations of literature (those not already cited in your manuscript) referred to inmy comments below can be found at the end of this review.

Major Comments

1. lines 129 - 135. I disagree strongly with the statements about the performance of the 'IEDA' and 'gradient method' algorithms for detecting BLH. Indeed the results which you presented in

Figure 3 indicate your algorithms as presently developed and implemented are not satisfactory for detecting the BLH on partially cloudy days. Further, I do not agree with your choices or justifications of using the IEDA for the miniMPL and the gradient method for the ceilometer data. I explain why, and offer suggestions, in the paragraphs below.

Crucially, and I feel this point is glossed over in the manuscript, you cannot compute a BLH via either of your methods (IEDA, gradient) in the presence of thick low-level clouds and subsequent loss of lidar signal above. You do not know a priori whether the low-level clouds are above or in the BL. Also, consider the miniMPL signal in the free troposphere (above say 1.5km) during this day shown in Figure 3b. You only see some backscattered signal (light blue colors, well above 0 a.u.) intermittently, indicative of signal return from these altitudes at these times. It's not clear whether the 'hour of day' is UTC or LT, but regardless, the MPL clearly has sufficient power to resolve a background at both daytime and nightime, which makes it very useful.

The miniMPL figure (3b) suggests that all the red colors are likely clouds, given there is no detectable signal above. This would explain why your gradient method (white circles Figure 3b) fails at these times of red-colored 'clouds', yet seems believable otherwise (e.g.10-12 hours, 15-18 hours – note the cloud at 12-14 hours is very likely sitting at the BL top).

As for the ceilometer results, your gradient method seems to be (successfully) detecting cloud base height in the present of cloud, or at least, the maximum backscatter signal gradient inside cloud, but this of course is not BLH. It agrees with the miniMPL gradient algorithm well during clear air (10-12 hours, 15-18 hours).

It is not clear to me how your IEDA algorithm can work where there is zero signal above optically thick clouds. I can follow how it works when there are no clouds (e.g. Figure 2). In fact, close inspection of Figure 3 (both ceilometer and miniMPL) suggests that the gradient method is working for both instruments (note that both agree during cloud-free periods throughout the day), whereas IEDA varies substantially during cloud-free periods and based on Figure 3, seems the less trustworthy method for either instrument. Thus I disagree with your statements on lines 133 – 135. I suggest that the gradient method is the best (only) one to use for both instruments, based on Figure 3, during cloud-free conditions only.

As a first step to rectifying/addressing these issues, you must positively identify low-level clouds and then remove these profiles before you implement either BLH algorithm (especially the 2D image analyses IEDA) and before any subsequent analyses and BLH statistics are presented.

Although I hope you are in fact doing this, you have not providedclear evidence in the text that you are indeed removing these cloudy profiles, and I worry that you are still incorporating cloudy profiles in your results. (see also my Major Comment 2 below)

I suggest in revising Figure 3, you show these plots in logarithmic color scales. This will give the reader (and this reviewer) much more confidence in where you do / do not have lidar and ceilometer signal above what I identify as 'clouds' Also you should identify periods of cloud in these plot yourself too (shade on top perhaps? – there are numerous examples in the literature which you could follow), and confirm in the text that you are removing these cloudy profiles prior to BLH calculation and analyses (at the least, removalof cloudy profiles where optically thick cloud is present in the BL, which preclude any BLH determination).

In summary, as presented in Figure 3 and in the text, these results are likely incorrect and I strongly urge you to outline and demonstrate an adequate cloud removal algorithm (and subsequent lidar backscatter profile removal) prior to BLH determination (gradient and IEDA methods), before Figures 4 and 5 in your manuscript can be trusted. At present, only the gradient method seems trustworthy and then only for periods where no low-level clouds exist.

Response:

Thank you for the suggestion. We have improved the IEDA BLH detection algorithm by applying cloud removal and revising the process of the image conversion. We hope it can make the algorithm clearer and more trustworthy.

Cloud removal:

First, we used the gliding method to identify clouds. According to the characteristics of the cloud backscattered signal (Zuev et al.,1987; Cadet et al., 2005), it can be known that the integral of noise is close to 0, while in the area where the cloud exists, the signal integral value will be a considerable positive one. Therefore, the signal integral in the cloud can be very different from that in other cloudless areas, which can help effectively distinguish the cloud signal from noises. As shown in the following figure, a filtering window W is presented. W_h and W_L are the upper and lower edges of the window respectively to integrate the echo signal in the window, i.e

$$C(w) = \int_{W_L}^{W_H} X(z)\,dz \tag{1}$$

$$= \int_{W_L}^{W_H} (P(z) + v(z)) z^2 dz = \int_{W_l}^{W_H} P(z) z^2 dz + \int_{W_l}^{W_H} v(z) z^2 dz \tag{2}$$

$$= \int_{W_L}^{W_H} P(z) z^2 dz \tag{3}$$

Where C(w) is the integral value of window W, and X (z) is the range-corrected signal. C(w) value approaches 0 in a certain interval if noises exist. For clouds, the integral value C(w) will be significantly larger than that of the aerosols. By properly selecting the threshold C(w), the cloud information can be extracted through the window integral value.

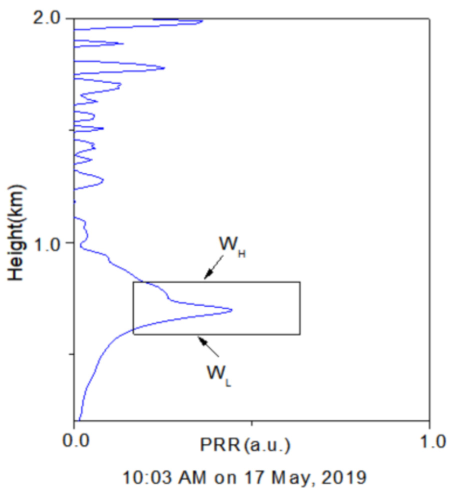


Figure. The cloud identification by sliding window integral algorithm window.

Furthermore, in order to ensure that all clouds within the measurement range are extracted, the window starts to move from the ground to the maximum detected height. Every time the window moves to a new area, the backscattered signal of the area is integrated to determine whether there is a cloud. If a cloud is present, further calculation of the position of the cloud base and cloud top will be implemented. Next the window continues to move upward from the cloud top until the signal ends. Because the inversion of cloud information is estimated by the movement of the window and the integral calculation of the signal in the window, this algorithm is called sliding window integral algorithm. We then identify the BLH after discarding the detected cloud profiles.

Improved IEDA:

Initially the gray conversion was applied to the colormapped image. We found that such conversion depends on which channel (red, green, or blue) of the image that the user has decided to use. However, none of the channel varies smoothly from black (0) through to white (255).

Therefore, we improved the IEDA algorithm by making the conversion directly from lidar backscatter signal to grey value (0-255), in other words, producing a greyscale pseudocolour plot. The IEDA processing steps are shown in Figure 2. We also described IEDA algorithm more precisely in the section 2.1.1 of the revised paper.

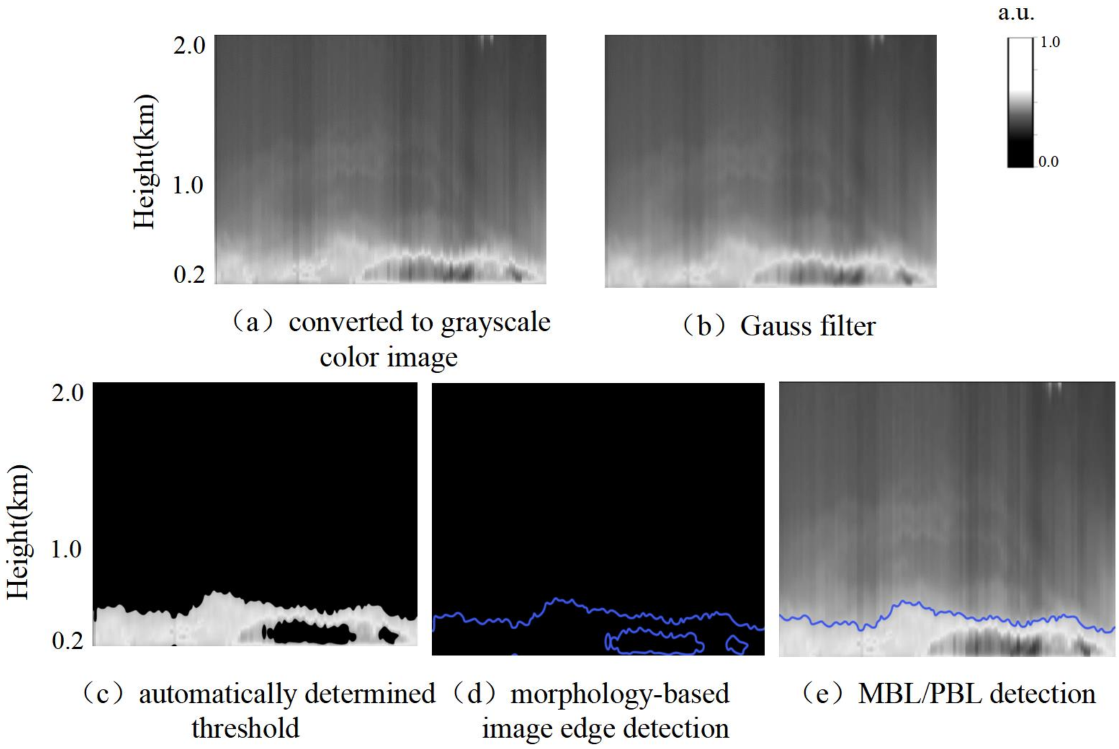


Figure 2. The main steps of IEDA processing for PBL/MBL detection.

Revised Figure 3

The revised Figure 3 below shows the BLH retrieval from ceilometer and miniMPL against ERA5 reanalysis data under cloudy conditions. For the miniMPL, the IEDA BLH (green line in Figure 3a) accords better with ERA5 BLH (magenta star) and provides fewer variable results compared to the gradient method (black circle in Figure 3a) as expected. For the ceilometer, the gradient method (black circle in Figure 3b), in the contrary, outperforms the IEDA (green line in Figure 3b), with the latter overestimating the BLH significantly compared to the ERA5 BLH. It is possibly because the IEDA fails to distinguish the target area and background area due to the low SNR of the ceilometer. The gradient method, more sensitive to the signal, may be applicable for ceilometer's BLH detection as it operates with a low-powered laser compared to the miniMPL. The discrepancy and uncertainties between the miniMPL/ceilometer and ERA5 can be mainly attributed to (1) the different definitions of the BLHs applied to each method (i.e., edge/gradient detection from aerosol back-scattered signal for miniMPL/ceilometer, bulk Richardson method for ERA5), (2) the different air masses for the spatial separation of the observing sites, (3) the coarse resolution of ERA5 and (4) the presence of the lofted layer or cloud layers. Among these causes, (1) (2) and (3) would influence our whole period. In the case of cloudy condition, we believe the presence of the clouds contributes the most, though other reasons are not to be ruled out.

Therefore, the BLH detection under cloudy conditions remains challenging for the miniMPL and ceilometer even after the cloud removal. We would recommend the BLH retrieval under cloud-free days and specifically IEDA for the miniMPL and gradient method for the ceilometer. This conclusion is not only applicable for cloudy days, but for cloud-free days. More BLH comparison, including the statistical analysis for the whole observing period and specific case study can be found in the section 3.1 (please also see our response to the major comment 3) and section 3.2 in the revised version.

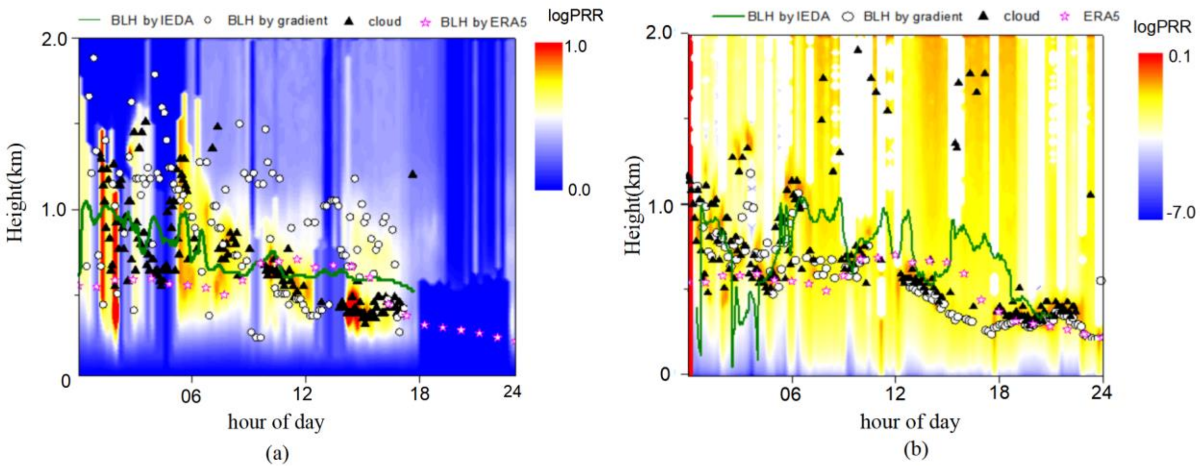


Figure 3. BLH estimations derived from (a) miniMPL and (b) ceilometer on 17 May, 2019 (LST)

The heights in all panels are in km AGL. The vertically aligned color bar (on a linear scale) on the right indicates the intensity of range-corrected signal in arbitrary units (a.u.). Solid green lines and black circles indicate the BLH estimations using IEDA and gradient method, respectively. The magenta star indicates the 1-h averaged BLH results from the ERA5 reanalysis data. The clouds are marked with black triangles.

2. Cloud screening. On line 138 – 139, you state you use 'gliding' method following Plattet al (1994). I could find no reference to this 'gliding method' within the Platt paper. Rather, they discuss at length how to determine the CBH, and settle on a still commonlyused signal gradient method to determine CBH (see their Figure 2). I suspect this wouldbe adequate for your work too (especially since your BL clouds are all likely liquid, or at least, liquid-containing, so very bright), so if you detect a CBH in a vertical profile, you discard that profile from subsequent *analyses before determining BLH. Alternately, you need to explain what is a gliding method and provide citations or full details if it is your own novel technique.*

Quality assurance of data. On line 140, you state that you perform a 'manual quality assurance' step to identify whether the BLH was identified correctly. No algorithm is going to perfectly resolve clouds, or BLH for that matter, so I concur that it is necessary to check the output of your algorithm. However, if your algorithm is 'good enough', the incorrect data points (false positives) should be sufficiently low to be ignored (lost in the statistics). What are you actually doing at this step of manual quality assurance? You need to provide full details. How do you decide yourself whether the BLH was correctly implemented? This is very important to understand as the rest of the manuscript hinges on an adequate BLH algorithm (both IEDA and gradient) but it is unclear what is happening here.

Response: We have removed the cloud before BLH identification. Please see the "cloud removal" part of our response to the comment 1.

As for the quality assurance, the edge point for IEDA or the largest negative gradient for gradient method do not always correspond to the boundary layer. Therefore, when retrieving the BLH it is important to include in the algorithm more than one source of information (e.g. edge/gradient, surface information, synoptic conditions, a priori information) in order to minimize the uncertainty. We developed a graphical user interface to visualize the detected BLH. Some guidelines were adopted to perform the manual check (Poltera et al., 2017), i.e.:

(1) BLH detection must lie on or be very close (within ± 2 range bins) to an edge point or aerosol gradient.

(2) BLH detection must always exist during clear-sky periods. However, if the cloud or fog or precipitation starts to dissipate, the detection is still allowed.

(3) BL starts developing from the ground after sunrise, i.e. elevated layers shall not be selected during early morning stage.

All the information available are taken into account (station measurements, BLH estimations from remote-sensing instruments, synoptic conditions, etc.) and compared.

3. As written in your abstract (line 22), you note that 'this paper evaluates two algorithms' for BLH detection, but there is no statistical analysis conducted. I discuss in the major comment #1 above my concerns about the 'by-eye' evaluation for Figure 3. I support the concept of evaluating your algorithms, this is worthwhile and necessary to do. But you should perform this for the whole campaign, once you have successfully removed cloud (major comment #2). I also *suggest you evaluate ERA5, and radiosondes (if available) against your observations.*

Response: Thank you for the suggestion. In our revised version, we included the ERA5 reanalysis product, as an additional and straightforward check on our observations and WRF. Unfortunately, no radiosonde was launched for this campaign. We have added Figure 4, Figure 5 for statistical analysis of the whole observing period and re-plotted Figure 9, 10 and 11(now presented as Figure 10,11 and 12 in the revised version).

BLH comparison against ERA5 reanalysis data

The inter-comparison 1-h averaged BLHs for the miniMPL and ceilometer against ERA5 during the whole observing period are presented in Figure 4. For miniMPL, an excellent concordance is found between IEDA- and ERA5- derived BLHs, with a correlation coefficient of 0.78 (Figure 4a). The gradient method underestimates the BLH with largest negative bias of 0.83 km, though its coefficient value is slightly lower (0.71 in Figure 4b). It is probably because the gradient estimates appear to detect the largest negative gradient from the bottom-up. Similar to the miniMPL, the ceilometer generally provides lower BLHs compared to the ERA5, though some unidentified elevated aerosol layers result in a few points with much higher BLH than ERA5-BLH. However, the gradient method (Figure 4d) outperforms (Pearson's r = 0.50) the IEDA (Figure 4c, Pearson's r = 0.41) against ERA5 BLHs. Therefore, no single approach can cover all situations over this campaign.

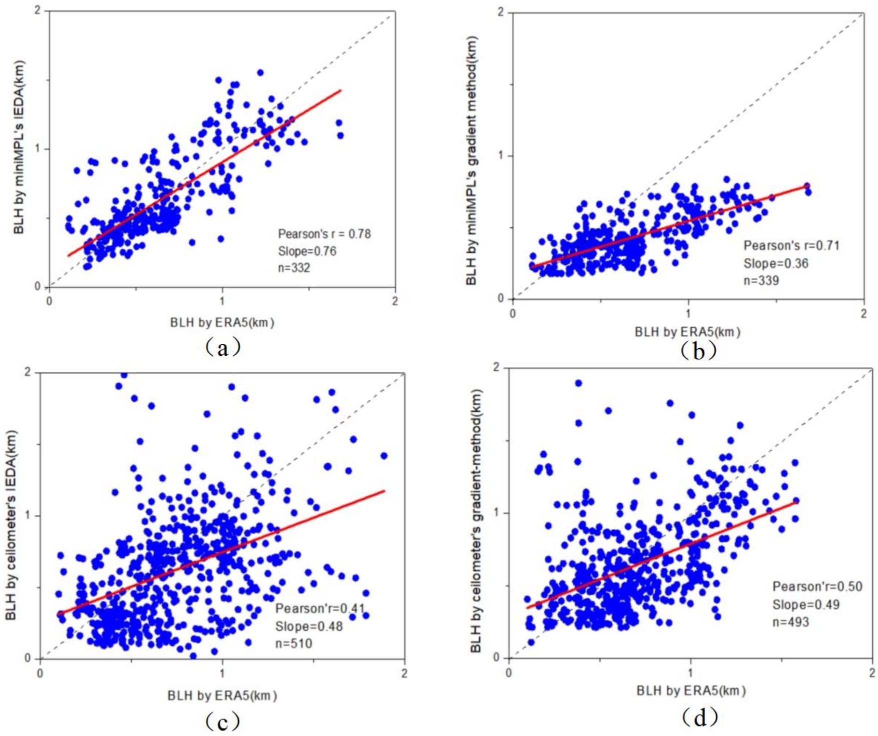


Figure 4. Comparison of 1-h averaged BLH estimations based on different instruments and methods against ERA5 results: (a) IEDA from miniMPL (b) the gradient method from miniMPL

(3) IEDA from the ceilometer and (4) the gradient method from the ceilometer. In each case, a linear regression through the origin is performed (red line) and statistics are shown: slope, Pearson's linear correlation coefficient (Pearson's r), slope, and number of samples (n). The black dashed line is a 1: 1 line.

Diurnal cycle of BLH

Considering the comparison in Figure 4 above, we chose IEDA for miniMPL and gradient method for ceilometer respectively to investigate the diurnal cycles of BLH (Figure 5). Generally, the ERA5 presents smoother and higher averaged BLHs (0.71±0.08 km) than those from miniMPL (0.64±0.06 km) and ceilometer (0.65±0.07 km). However, the diurnal cycles of BLH from 1 h-averaged miniMPL (IEDA) observations show good agreement with those from ERA5, especially from the early morning to 11:00 LST, with a negative bias (- 0.02 to -0.10 km). The miniMPL (IEDA) BLHs reached the maximum of 0.76 km at 14:00 LST while the ERA5 BLHs peaked at 0.86 km at 15:00 LST. For comparison, the ceilometer (gradient) shows more variable BLHs, as expected due to multiple sharp gradients corresponding to multi-layer or lofted aerosol layers, and presents less diurnal characteristic of MBL (marine boundary layer). Its BLH fluctuated from 0.52 to 0.72 km in the morning and appeared to collapse immediately afterward before growth at 19:00 LST again. The largest difference between miniMPL/ceilometer and ERA5 occurs during the MBL developing period (from 12:00 to 20:00 LST) and the mean nocturnal boundary layer are higher than 0.5 km.

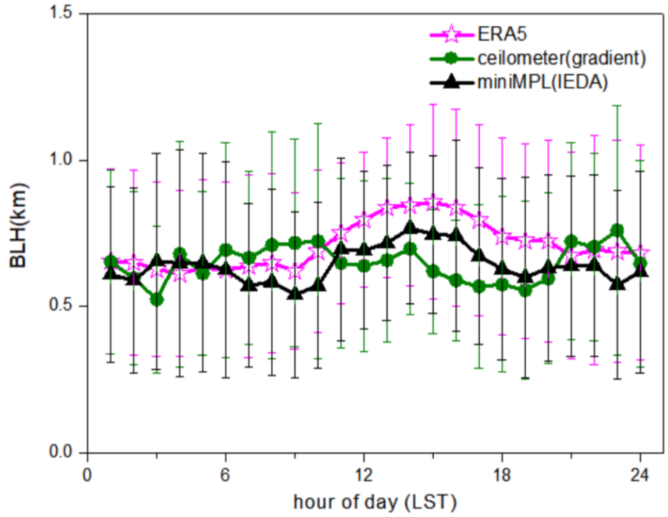


Figure 5. Resulting BLH for the whole observing period with 1-h averages from miniMPL (IEDA), ceilometer (gradient method) and ERA5. ERA5-estimated BLHs are shown as magenta stars.

4. Color scales of figures. Many figures in your manuscript use the rainbow colorscale, which *must be avoided, because of issues for colorblind people, and also note that it does not have uniform color changes. The ACP website itself has good links for choosing better colorscales, and you should also read: Crameri et al., Nat Comm (2020) https://www.nature.com/articles/s41467-020-19160-7*

Response: Thank you for the references. We now chose the "temperature" as the new color scales for Figure 9,10 and 11(now presented as Figure 10,11and 12).

Minor Comments

1. *Title: "Real-time.." You are doing post-processing to determine the BL height and properties. So you can't use 'real-time' in your title, abstract etc, it is incorrect to state this. Please remove this phrase and check carefully throughout your manuscript to catchall other instances.*

Response: We have removed the phrase "real-time" throughout the manuscript.

2. Line 19: does 'their' refer to the BL?

Response: "their" indicates the meteorological information (wind). We have clarified it in the revised sentence.

3. Line 25: 'generally performed better' you are making this statement relative to whatreference? WRF? Radiosondes?

Response: We reanalyzed the data and revised the statement as "No single approach can identify BLH under all conditions. After comparing with the ERA5 reanalysis data, we would suggest for the BLH retrieval under cloud-free days and more specifically IEDA for the miniMPL and gradient method for the ceilometer."

4. Line 32-33: I find it difficult to believe that much wet sea-salt production is occurring /being transported from continental Australia.

Response: Sorry for the confusion. We have rewritten the sentences as "The increasing extinction coefficient and depolarization ratio with wind speeds may be attributed to the mixture of increased sea salt production during the marine flow and regional transported continental aerosols from the mainland Australia."

5. Line 63-64: There are now a few studies which do discuss vertical profiling of marine aerosols in the Southern Ocean region (maritime or near-coastal sites) which the authors may not be aware of. These include, but are not limited to Bohlmann et al. (ACP, 2018), Radenz et al. (ACP, 2021)

Response: Thank you for the suggestion. We have added more related references in the revised version, including the ones you suggested.

6. Line 66: the phrase 'most straightforward and least expensive' should be removed. It seems hard to prove this. You should simply state that lidar is effective at deriving PBL/MBL heights when atmospheric conditions allow.

Response: We have reworded the sentences according to your advice.

7. Line 68: what does 'least interference with its environment' mean? Radars also don't interfere much. I suggest this phrase needs to be removed.

Response: We have removed this phrase.

8. Line 69: only some lidars can measure trace gases, such as ozone. Reword this sentence.

Response: We have reworded the sentence as "This optical technique, with the high temporal and vertical resolution, measures the aerosols or trace gases of the atmosphere."

9. Line 73-74 this sentence about lack of vertical marine aerosols study repeats that at line 63-64 and can be removed. You could cite the papers I noted above, plus any others you deem relevant, at lines 63-64

Response: Thank you for the reminder. We have removed the sentence in this paragraph and included related references in the previous paragraph.

10. Line 101: '... to water vapour and cloud in addition…'

Response: We have revised it.

11. Line 116: I'm a bit confused here. Lewis et al. (2013) start out with the WCT method, but you refer to the 'gradient profile' method in your text. Why is that? I think you should follow the literature and use the 'WCT' term if you follow Lewis. If you are using your ownmethod, then why do you cite Lewis paper? It seems Lewis et al. perform image feature analysis on the WCT analyses (their Section 2.1). Note that the WCT method is well established and robust. See Baars et al (ACP 2008) for a thorough description and discussion on the WCT.

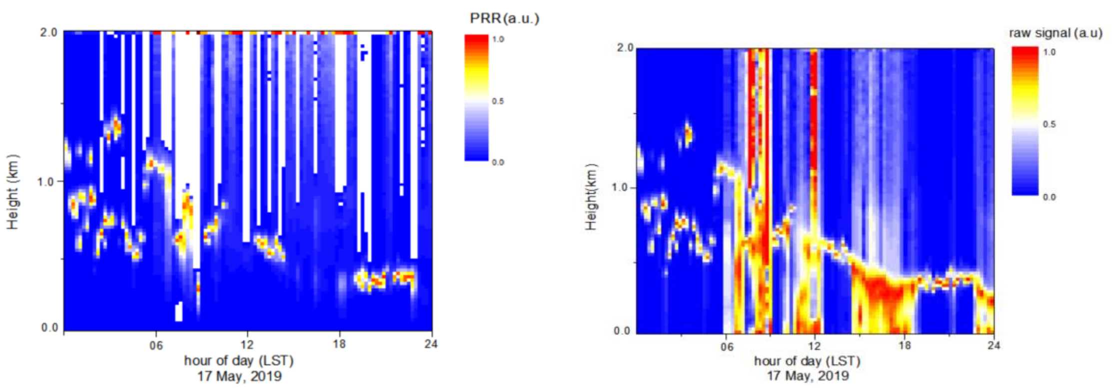
Response: Lewis et al (2013) indeed used a combination of the wavelet technique and image processing. We have corrected it.

12. Line 117: Regarding the statement that image processing method(s) are 'not easily affected by clouds' for determining the BLH. I disagree strongly with this statement when it applies to BL clouds, and indeed your own results in Figure 3 show that the image processing (IEDA) method fails in the presence of optically thick clouds, and clearly gives incorrect results (see Major Comment above).

Response: Yes, cloud layers impede the detection of the BLH, whatever which method was utilized. For example, the gradient method will mistakenly identify the large gradient of the low cloud layers as the BLH, while the IDEA-derived BLH more corresponds to the top of the cloud with higher results. We have removed the cloud, improved the IEDA procedure and corrected the related conclusion. Please see our response to major comment 1.

13. Line 120: can you confirm whether you range correct your signal too?

Response: Yes, we have rang-corrected the signal. Figures below shows the comparison of the raw signal and rang-corrected signal.



(a) (b)

Figure. (a) The range-corrected back-scattered signal and

(b) the raw signal from the ceilometer on 17 May, 2019.

14. Line 125: 'the most considerable change…' do you mean the 'most significant change' orthe 'largest change'? I guess 'significant', but please clarify. Note of course that 'significant' implies you are performing some statistical test to confirm that the edges detected are in fact significant.

Response: Yes, it means "significant". We tested the IEDA through the campaign and found it works better than the gradient method, but only for miniMPL. We also have clarified it in the revised version.

15. Line 131: But you don't show SNR on Figure 3. So you should not refer to it in the text unless you add an SNR plot (which would be useful for both the ceilometer and MPL). Indeed with these color scales and linear plots, it is hard to visually detect any 'noise', itwould be much easier for the reader to do so if they were logarithmic plots.

Response: Thank you for the suggestion. We have changed the color scale and plotted with the logarithmic scales to illustrate BLH detection under a cloudy day in Figure 3.

16. Line 142: WRF is a good choice. But I would advocate you also include a reanalyses product, such as ERA5, as an additional and straightforward check on WRF and your observations. Were radiosondes launched for this campaign? They should be included heretoo, if they are available. Then you can perform a statistical evaluation of both your BLH algorithms in clear-air conditions.

Response: Thank you for the suggestion. We have included the ERA5 reanalysis data for BLH comparison according to your suggestion. Unfortunately, we didn't have radiosonde data available in this campaign. More details could be found in our response to major comment 3.

17. Line 173: Given the topography visible in Figure 1, do the CALIOP overpasses pass overto topography, which is (a) at altitude and thus (b) may distort the BLH and BL structure compared with Cape Grim?

Response: Apologies I didn't quite follow you with this comment. However, the CALIPSO orbit was presented in the following figure, when it passed over near the Cape Grim site on 21 and 26 June 2019. It is cloudy on 21 June, so only the CALIPSO's PLDR on 26 June is used for miniMPL's PLDR comparison. I guess you may mean the CALIPSO-derived BLH, but we didn't analyze it and not sure whether it may distort the BLH and BL structure.

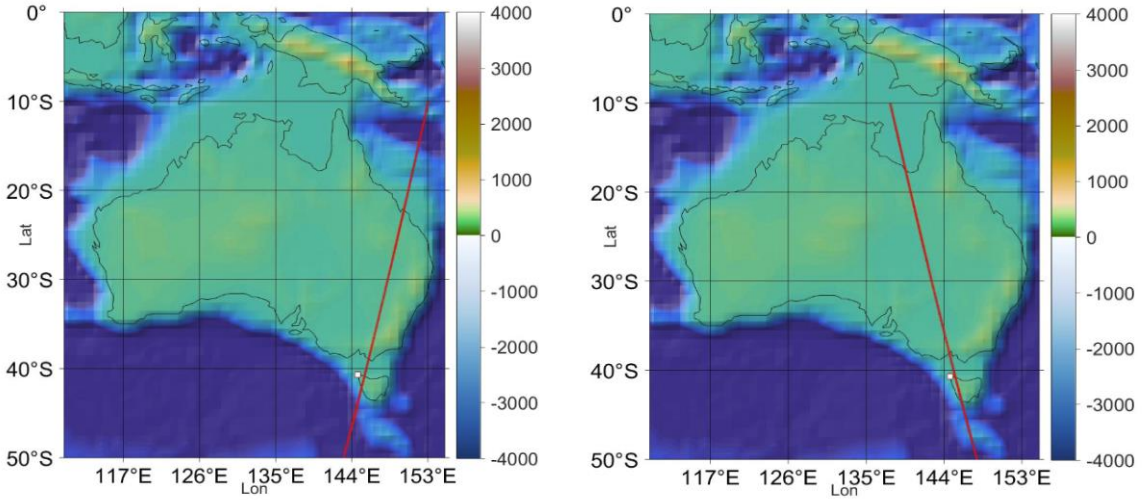


Figure. The CALIPSO orbit on 21 June and 26 June, 2019

18. Line 175 (Section 3.1): Given my Major Comments above, I suspect this whole section needs a rewrite once you have correctly identified and removed cloudy profiles.

Response: Yes, we have rewritten this section in the revised version.

19. Line 193: I'm a bit confused here, is Figure 6 the MSLP surface or is the 850hPa surface?

Response: Figure 6 shows the MSLP surface chart. We have corrected it.

20. Line 215: remove 'our site'

Response: We have removed the phrase.

21. Line 217: Do you mean 'not much cloud' or do you mean 'cloud at low altitudes'? Low-altitude BL clouds are trapped beneath the descending air in high pressure systems at times

Response: We mean "not much cloud" and we have clarified it as "low cloud cover (nearly cloud-free or only sporadic cloud exists)".

22. Line 218: On the phrase 'cold air outbreak' (CAO). A CAO is not likely at all from my reading of your analysis charts – the last chart shows warm prefrontal NW flow over Cape Grim. Further, a cold air outbreak (CAO) has a specific meaning, see e.g. Geerts et al. BAMS, 2022

Response: Thank you for pointing this out. From the last chart, a cold front is approaching our site on 27 June. It undercut the displaced warm air, disrupt the stability and lead to the temperature drop in the next several days. But it occurred after 27 June, we have removed the phrase of "cold air outbreak".

23. Line 220: change 'northeastern' to 'northeasterly'. Check your whole text carefully again, there are many occasions where this needs to be changed, for varying wind directions.

Response: We have checked the whole manuscript and corrected all the related phrases about the wind directions.

24. Line 222-223 this sentence makes no sense. Reword.

Response: We have removed the sentence.

25. Line 239: I cannot detect any 'drastic speed increase' at 0900.

Response: We have re-plotted the wind vector. For the revised wind vector on 20 June 2019, the wind direction shifted to southerly with height at 09:00 LST. Then it slightly varied between 160^{o} to 180^{o} (SE to S) in the next three hours. It shifted to the SE completely at all heights from 12:00 LST and maintained the SE in the afternoon. At night it varied back to the southern direction. For the wind speed, it increases with the height, ranging drastically from 2.2 m/s up to 15.1 m/s from 00:00 to 09:00 LST. After 09:00 the wind speed varied more leisurely with the height from 1.6 to 5.7 m/s until 13:00 LST. Therefore, the changed wind speed and shifted direction from 09:00 LST could indicate the onset of MBL development.

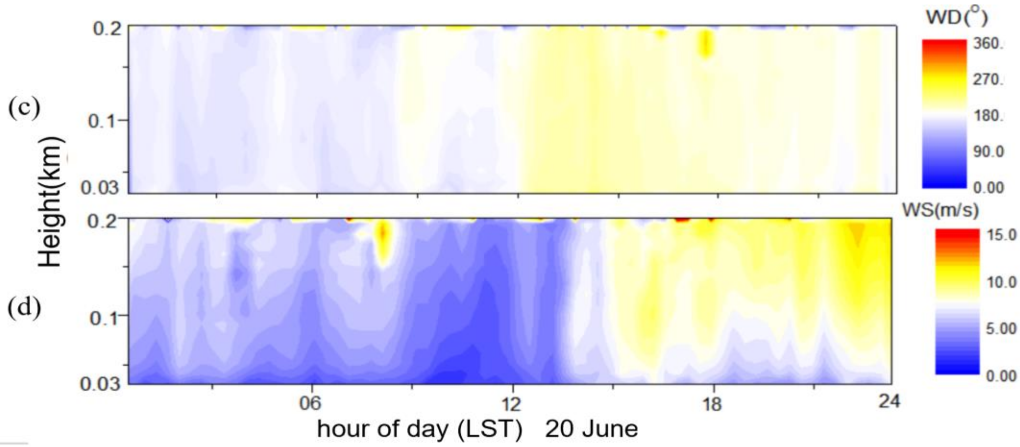


Figure 10c and 10d. Wind direction and wind speed on 20 June, 2019. More details could be found in the section 3.2.

26. Line 240: it would be really preferable (and easier for readers and this reviewer) to show a horizontal vector wind plot rather than two separate zonal & meridional contours, since you discuss e.g. southeasterly winds (line 240). It is hard for readers to piece together what a southeasterly wind would look like in zonal and meridional components, without interrupting the flow of the manuscript. Make it as easy as possible for readers to understand your message.

Response: We have re-plotted the wind information in Figure 9,10 and 11 (now presented as Figure 10,11 and 12) in the revised version.

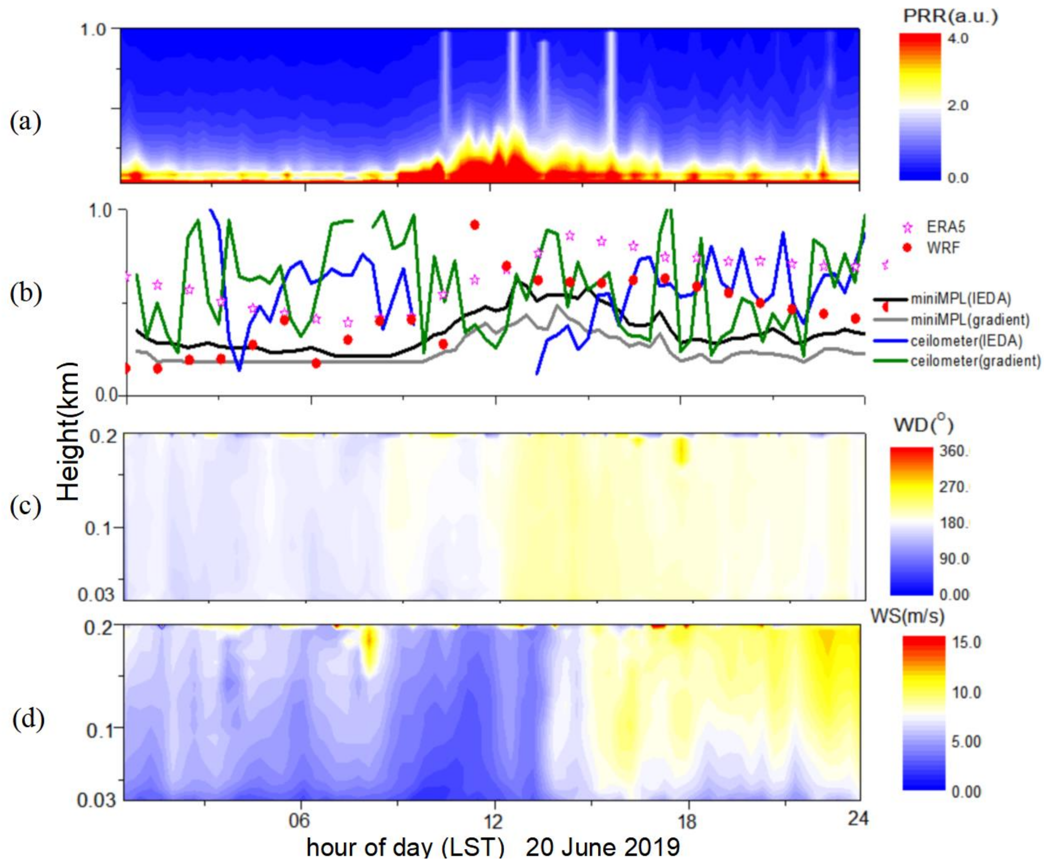


Figure 10. An example illustrating the retrieval of BLH using the lidar measurements obtained on 20 June 2019 over the Cape Grim observatory. The heights in all panels are in km AGL. (a): The normalized lidar backscattered signal using the miniMPL. The vertically aligned color bar (on a linear scale) on the right indicates the intensity in arbitrary units (a.u.). (b) Black lines, gray lines, blue lines and green lines indicate the 20-min averaged BLH estimations from the miniMPL (IEDA), miniMPL (gradient), ceilometer (IEDA) and ceilometer (gradient), respectively. The magenta star and red circles indicate the 1-h BLH results from the ERA5 reanalysis and WRF model. (c): wind direction(°) and (d): wind speed (m/s) from sodar.

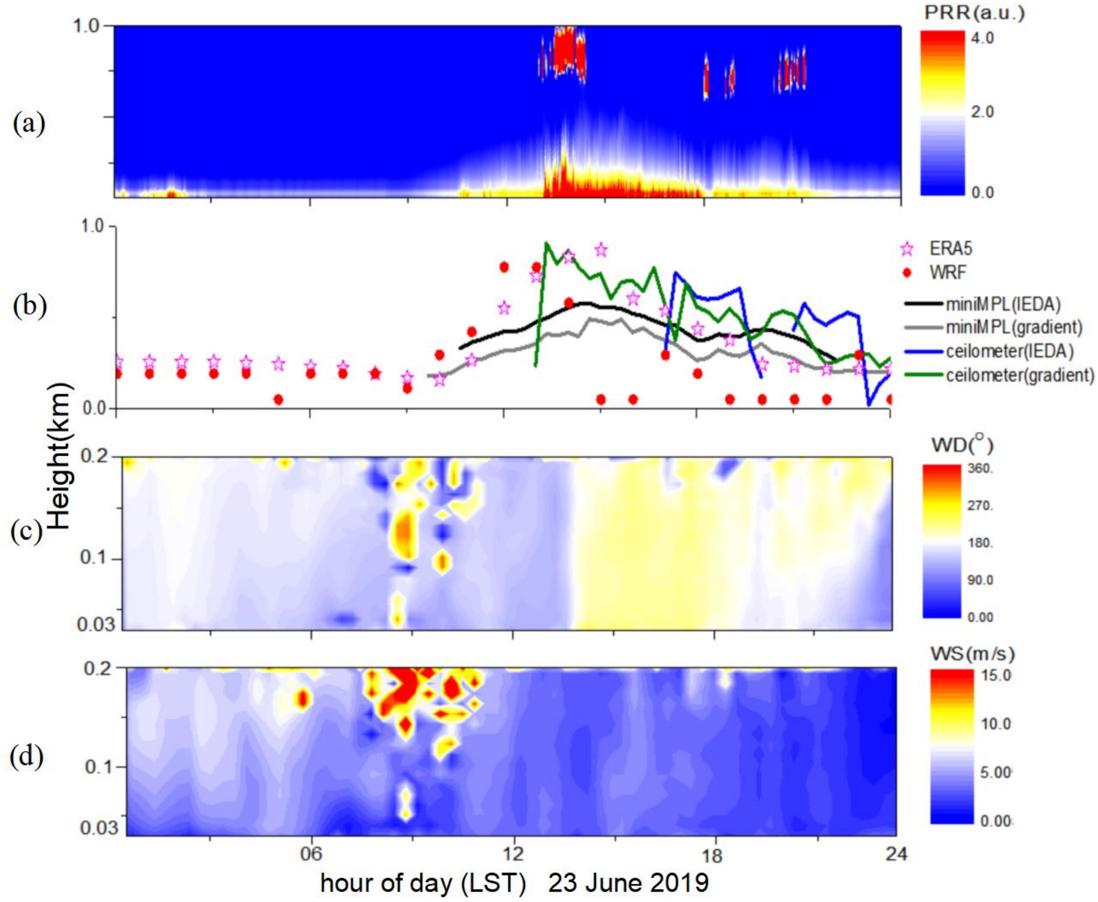


Figure 11. The same as Figure 10 except showing results from 23 June 2019.

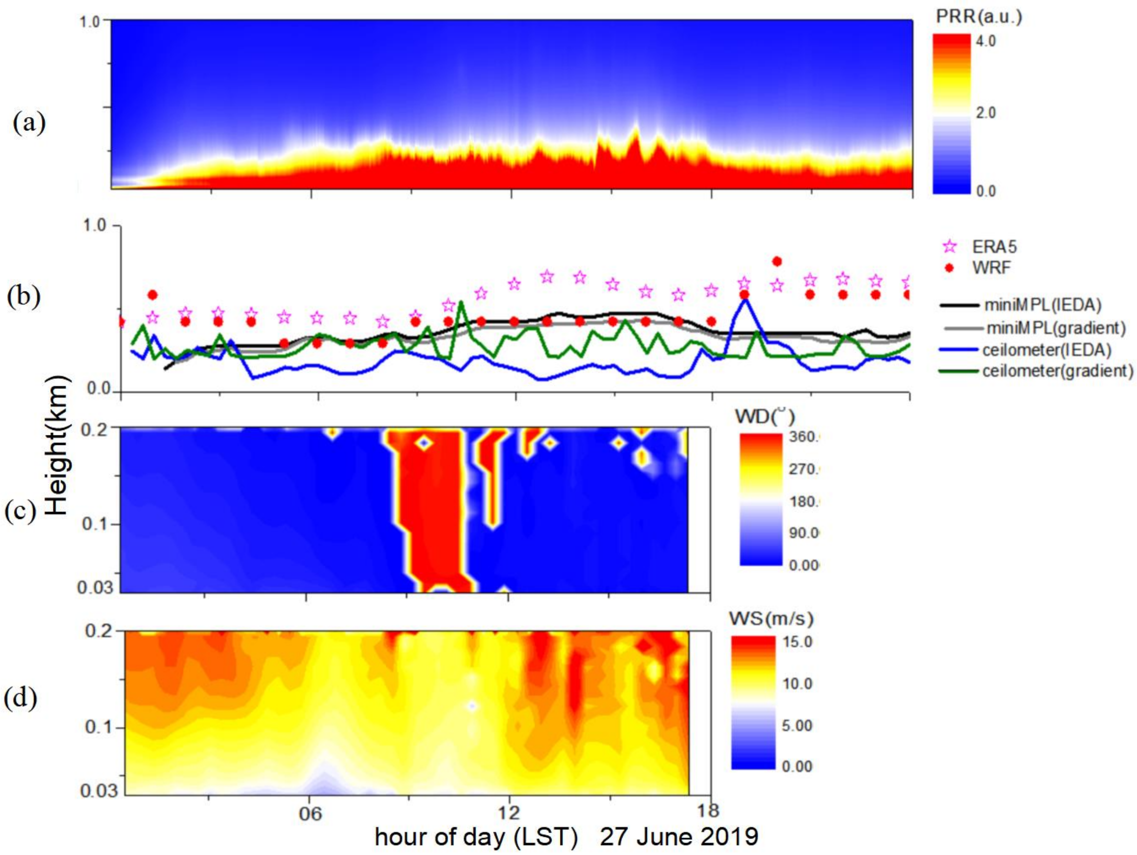


Figure 12. The same as Figure 10 except showing results from 27 June 2019.

27. Line 249: Additionally, to possible issues with WRF, it is quite likely that these differences between WRF and observations are caused by inaccurate BLH determination (see my major comments above). It will be interesting to learn how this changes once you have a more complete cloud removal and BLH determination.

Response: Yes, we have reanalyzed the BLH and rewritten all the comparison results.

28. Line 257: As with my comment above, BL jets would be much clearer if these were plotted as a vector wind plot instead of separate zonal and meridional contours. At present, BL jets are very difficult to see with (a) all the noise points in the sodar data and (b) the poor color scale

Response: We have removed all the jet-related phrases through the paper.

29. Line 258: You are not showing the (horizontal) wind speed here, only the components (zonal & meridional). So it is very hard to confirm this statement

Response: We have added the wind speed in three cases. Considering the characteristic of low-level jet (i.e diurnal pattern, significant instability before the occurrence of the jet), we would relate the high-speed wind shift to the intrusion of a robust offshore flow (northern and northwest) at the height of 0.1 to 0.2 km instead. We have re-written the statements.

30. Line 259: Rather than 'turning', I believe the terminology is 'backing' or 'veering', depending whether the wind is moving clockwise or counterclockwise.

Response: We have corrected the phrase as "veering".

31. Line 260: Do you mean that the wind was from the marine sector (clear air sector)? Again, show as a vector plot for clarity.

Response: OK, all the wind plot has been presented as the horizontal wind vector, see our response to the minor comment 26. We have also clarified as the wind from the marine sector.

32. Line 271: A bit confused why you consider altitudes as low at 0.38km here, given that at line 105 you say that you only consider attenuated backscatter above 100m due to the overlap factor *lower down. As you know, you can back out the overlap factor to present data at these very low altitudes (0.38km) with increasing uncertainty, but if this is what you did, you need to explain it here to reconcile with what you wrote earlier.*

Response: We presented the BLH results from 30 m though we only use the signal above 100 m. The BLH was also re-calculated. In the 27 June case, the mean BLH estimated by miniMPL(IEDA), miniMPL(gradient), ceilometer (IEDA) and ceilometer (gradient) are 0.36 km, 0.33 km, 0.19 km and 0.27 km, respectively. For comparison, the mean BLHs from ERA5 and WRF model are 0.57 and 0.46 km, respectively. We have re-rewritten the related statement and made it reconcile with the previous statement about the overlap factor.

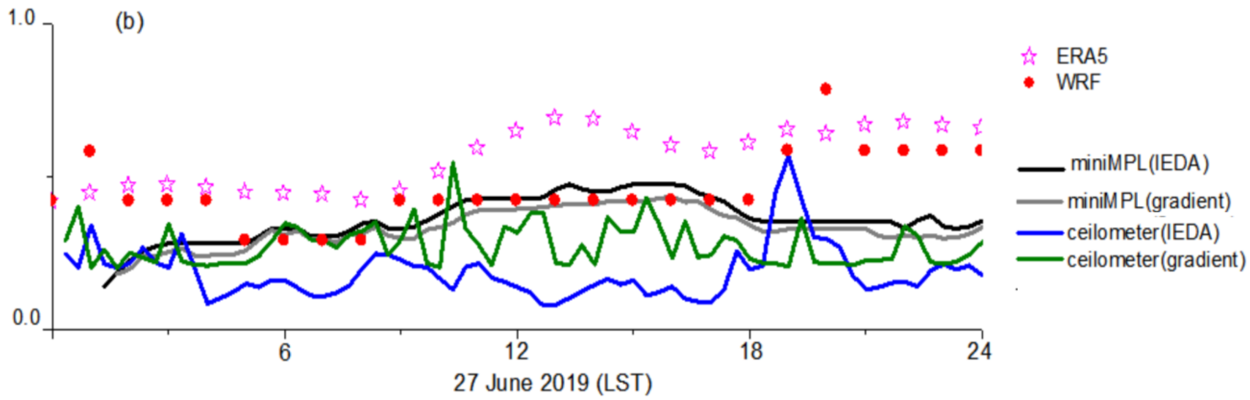


Figure 12b. BLHs comparison from miniMPL and ceilometer against EAR5 and WRF on 27 June, 2019.

33. Line 274: How can you make statements about 'high' and 'low' aerosol concentrations? You have not presented any evidence that you have either calculated or measured aerosol concentrations. With the lidar, you cannot make assumptions about the number concentrations, unless you are following Mamouri & Ansmann (ACP, 2016), which I see no evidence that you are doing here. Remove the comments about low or high concentrations.

Response: It means the high or low aerosol extinction coefficients under continental and marine influences. We didn't use the approach from *Mamouri & Ansmann (ACP, 2016)*. Thank you for the comments and we have removed the misleading phrases.

34. Line 278: Where do you show turbulence parameters and quantify cloud-top radiative cooling? I don't see this in the figures. You need to support this statement with WRF figures, or remove it.

Response: We have removed this statement.

35. Line 294: It seems that you are confusing your DPR (that is, the volume depolarisation ratio) with particle linear depolarisation ratio (PLDR) here. See Freudenthaler et al Tellus 2009 on how to calculate PLDR. It is the PLDR which you need in order to confirm what you are likely seeing e.g. marine or continental aerosols.

Response: Thank you for the reminder. Yes, it is the particle linear depolarization ratio (PLDR) which I used to identify the aerosol types. We have revised it through the paper.

36. Line 297: Where is the evidence presented for dry marine aerosols?

Response: We have removed the phrases of "dry marine aerosols".

37. Line 303: Do you mean that the CALIOP curtain(s), close to Cape Grim, is contaminated by cloud? In this case, how can you trust the DPR (and PLDR)?? Are the clouds optically thin enough? Or is it cloud contamination from the surface lidars? Either way, this points to needing a better cloud removal algorithm (see comments above)

Response: Sorry for the confusion. It is cloudy on 21 June and hence we discard this day's CALIPSO profile and only use the CALIPSO observation on 26 June. We have rewritten the sentence.

38. Line 304: Where is your Discussion section? You need to place your observations and simulations in context of our previous understanding of the MBL, and detail how your work has expanded upon knowledge gained in previous studies.

Response: We have added a separate discussion section 3.3 to summarize the results based on the study.

39. Line 306: 'we evaluated…' you can't say this. You have not performed a proper statistical evaluation. You have only compared (by eye) which looks closer in a couple of brief observation windows. It would be advantageous if you did properly evaluate WRF against observations. But as it stands, you need to remove this phrase.

Response: Yes, please see our response to the major comment 3.

40. Line 308: As noted earlier, I disagree with these comments because of issues related to algorithm performance and clouds. Your conclusion (and abstract) will need a complete rewrite following a refined cloud and BLH algorithm.

Response: We have checked the manuscript and rewritten all the related statements.

41. Line 321: You have too many unsubstantiated and unsupported claims in your conclusions, and are introducing topics which you have not previously raised. You do not demonstrate that you have soil or anthropogenic emissions (these have different lidar ratios and PLDRs, which you cannot calculate using a miniMPL). All you can say with the evidence presented in your manuscript is that it's likely continental sources.

Response: Yes, our miniMPL cannot provide lidar ratios, and it is not trustworthy to indicate the specific aerosol type by single PLDRs. However, we have added the radon concentration to help verify the continental sources (section 3.2 and shown by Figure 6(d)).

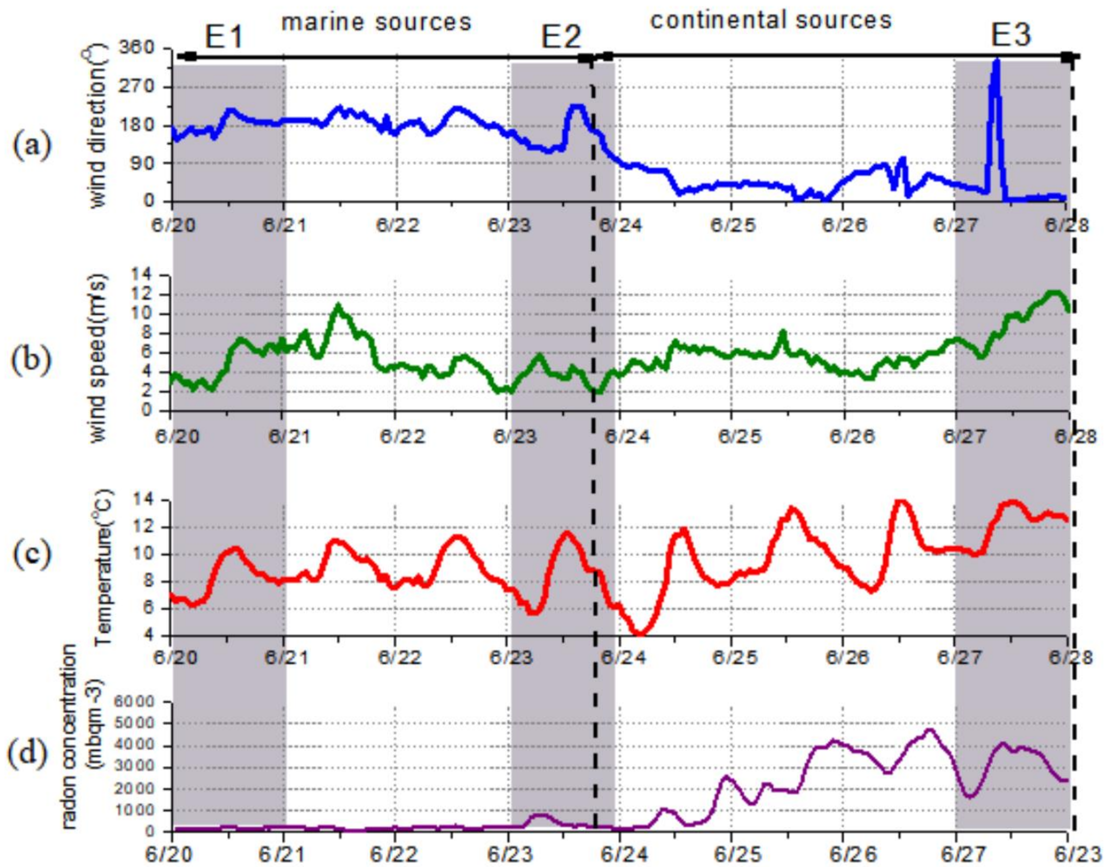


Figure 6. The evolution of surface meteorological parameters (a)wind direction, (b) wind speed, (c) temperature and (d) radon concentrations during the case study period from 20 June to 27 June, 2019. The three episodes selected for closer inspection are marked as E1, E2 and E3.

Figures

1. Figure 1: Need a colorbar scale for the topography.

Response: The topography was obtained from CSIRO website (https://research.csiro.au/acc/capabilities/cape-grim-baseline-air-pollution-station/), which didn't provide a color scale. Therefore I re-plotted the map from the reference (Gras and Keywood, 2017) instead .

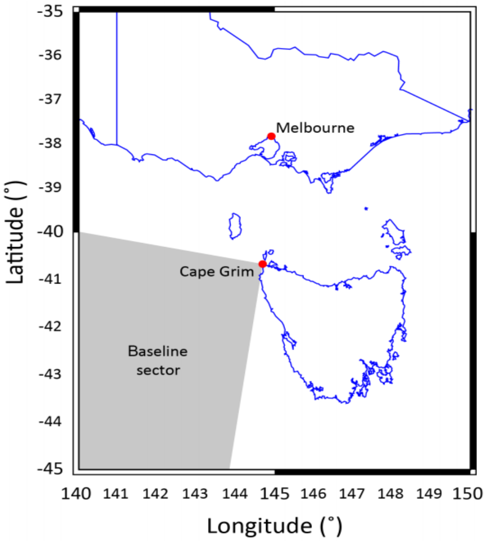


Figure 1a. The topography of Cape Grim

2. Figure 2: Axes required. Describe in figure caption what each panel is. Mention the date of observations in the caption too.

Response: We have added the information according to your suggestions. The processing steps can be found in the "Improved IEDA" part of our response to the major comment 1.

3. Figure 3: Comments on the data are found above (in Major Comments). For the caption, you need to add what day this is.

Response: We have re-plotted the Figure 3 and added the date (17 May, 2019)

4. Figure 4: Where are your standard deviations on Figure 4b?

Response: We have re-plotted Figure 4b (now the Figure 5 in the revised version) and added the standard deviations.

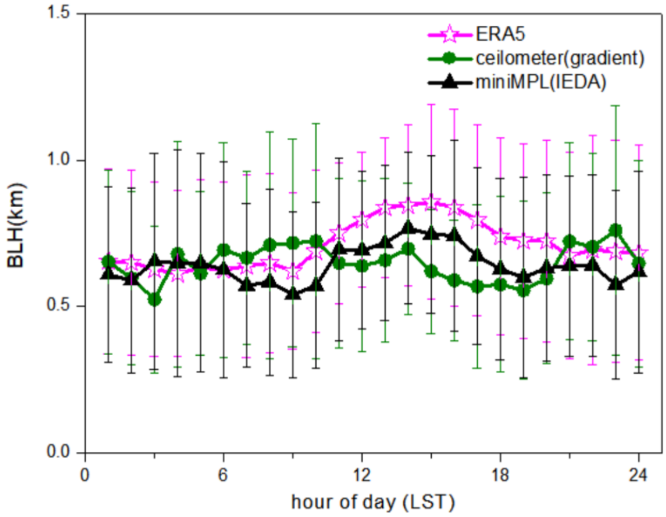


Figure 5. Resulting BLH for the whole observing period with 1-h averages from miniMPL(IEDA), ceilometer(gradient method) and ERA5. ERA5-estimated BLHs are shown as magenta stars.

5. Figure 6 caption: what does 'subsequent stabilisation' mean?

Response: We have removed the phrase.

6. Figure 6 caption: the low pressure system can't be 'back'. It must be a new one! (but it looks like a NW flow over your observations site – the low pressure is miles away deep over the Southern Ocean in Figure 6c)

Response: Yes, our observing site undergoes the strong northwesterly flow under a new low-pressure system. We have corrected the description for Figure 6 (now presented as Figure 7 in the revised version).

7. Figure 9, 10, 11: Suggest chopping panel (a) in all of these at 1.0km altitude. The rest is waste space. Change color scale (as per major comment) – get rid of the rainbow.

Response: We have re-plotted all the related figures under 1.0 km. The color scale has been replaced as "temperature". Plots can be found in our response to the minor comment 26.

8. Figure 9, 10, 11: As noted in comments above, you would be far better showing a vector plot of the horizontal winds than these two zonal & meridional contour plots. It looks like much of the meridional velocity exceeds 10m/s so you should alter your (wind vector) scales too.

Response: Yes, and please see our response to the minor comment 26.

9. Figure 9, 10, 11: You need to perform a noise-removal of the sodar winds before plotting. There are many noisy points above about 150m altitude, and these detract markedly from the message you trying to convey. I would suggest a simple snr threshold removal as a first go but you may well need something more sophisticated.

Response: We have tested a SNR threshold value ranged from 2 to 10 for the sodar winds retrieval. We finally chose the threshold of 5 to obtain the wind vector without losing too much information.

10. Figure 10a: The white dots, from the gradient method for determining BLH, seem suspiciously low during the 12 – 18 hours. What minimum altitude are you setting for the gradient maximum? It is likely you may need to require a minimum of say 200m altitude for a possible BLH, given you need sufficient points beneath in order to calculate the signal's derivative (gradient).

Response: We have set the minimum height of 180 m for BLH retrieval based on the calculated gradients during the whole campaign.

11. Figure 12 caption: Add in the caption what lidar ratio you use for each subpanel in order to calculate the extinction coefficient.

Response: According to the previous measurements (Müller et al., 2007; Omar et al., 2009), we used the lidar ratio of 20 and 60 for aerosol extinction coefficient calculation under marine and continental sources, respectively. We also added a subsection 2.2.3 to describe the extinction efficient retrieval.

12. Figure 12: as noted in comments above, this seems to be the volume depolarisation not the particle linear depolarisation ratio (PLDR), which is what you should probably be showing.

Response: It is the PLDR ratio and I have clarified it in the revised version.

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