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Comment on acp-2022-104

Anonymous Referee #2

Referee comment on "Measurement Report: Remote Sensing of the Coastal Boundary Layer and its Interaction with Meteorology at Cape Grim, Australia" by Zhenyi Chen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-104-RC2, 2022

Review of Chen et al. (2022), submitted to ACPD

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 discuss some 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. I detail my comments below.

The full citations of literature (those not already cited in your manuscript) referred to in my 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, 15-18hours).

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 provided clear 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, removal of 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.

2) Cloud screening. On line 138 – 139, you state you use 'gliding' method following Platt et 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 commonly used signal gradient method to determine CBH (see their Figure 2). I suspect this would be 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.

3) 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.

4) 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.

5) 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

Minor Comments

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 catch all other instances.

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

Line 25: 'generally performed better' you are making this statement relative to what reference? WRF? Radiosondes?

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

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)

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.

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

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

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

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

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 own method, 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

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).

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

Line 125: ‘the most considerable change…’ do you mean the ‘most significant change’ or the ‘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.

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', it would be much easier for the reader to do so if they were logarithmic plots.

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 here too, if they are available. Then you can perform a statistical evaluation of both your BLH algorithms in clear-air conditions.

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

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.

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

Line 215: remove 'our site'

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

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

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.

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

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

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.

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.

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

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

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

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

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.

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.

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.

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.

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

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)

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.

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.

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.

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.

Figures

Figure 1: Need a colorbar scale for the topography.

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

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

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

Figure 6 caption: what does ‘subsequent stabilisation’ mean?

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)

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.

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.

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.

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).

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

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.

References

Baars et al., ACP, 2008, 'Continuous monitoring of the boundary-layer top with lidar', www.atmos-chem-phys.net/8/7281/2008/

Bohlmann et al., ACP 2018, 'Ship-borne aerosol profiling with lidar over the Atlantic Ocean: from pure marine conditions to complex dust–smoke mixtures', https://doi.org/10.5194/acp-18-9661-2018

Freudenthaler et al., Tellus 2009, 'Depolarization ratio profiling at several wavelengths in pure Saharan dust during SAMUM 2006', DOI: 10.1111/j.1600-0889.2008.00396.x

Geerts et al, BAMS, 2022, 'The COMBLE campaign: a study of marine boundary-layer clouds in Arctic cold-air outbreaks', in press, DOI 10.1175/BAMS-D-21-0044.1

Mamouri and Ansmann, ACP, 2016, 'Potential of polarization lidar to provide profiles of CCN- and INP-relevant aerosol parameters', doi:10.5194/acp-16-5905-2016

Radenz el al., ACP, 2021, ' Hemispheric contrasts in ice formation in stratiform mixed-phase clouds: disentangling the role of aerosol and dynamics with ground-based remote sensing, https://doi.org/10.5194/acp-21-17969-2021