Measurement Report: Real-Time Remote Sensing of the Coastal Boundary Layer and its Interaction with Meteorology at Cape Grim, Australia

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This paper presents measurements from a lidar, ceilometer and sodar at Cape Grim, from which the boundary-layer height is derived. Although Cape Grim is an interesting station for measurements of pristine Southern Ocean air, it is not clear to me what the purpose of this paper is meant to be. Most of the results show three case studies which are little more than examples – they don't lead to any useful conclusion beyond this particular dataset.

At the heart of the difficulties encountered in this paper, and not discussed at all in the introduction, is what is meant by the BLH over the ocean and how this relates to the distribution of aerosols. The BLH is essentially a thermodynamic concept, but the marine boundary layer can be stable, inhibiting the vertical transport of aerosol. To effect a meaningful comparison with WRF, for example, one would need to understand what WRF means by BLH and whether that is relevant to the distribution of aerosols (i.e. are they likely to be mixed throughout the BL or confined to low levels by inversions in the temperature profile?). The issue is compounded here by the very different 'BLH' derived from the two remote sensing instruments, strongly suggesting that a simple definition is inappropriate.

Regarding the conclusions of the paper:

1. You shouldn't validate measurements against a model!
2. The comparison of the two algorithms is superficial and the discussion of fig 3 (see below) is misleading. The IEDA gives smoother curves because I suspect it is designed to do so, but there is no evidence that it is 'better' than the gradient method.
3. The effect of clouds on BLH measurements is barely mentioned, other than to state without proof that the IEDA method is better in this regard.
4. The result that DPR for pure oceanic air masses is lower than that for a continental air mass with a dust component is hardly new.

For this paper to be publishable, the authors must decide what scientific story they want to tell and structure their paper accordingly. I'm aware that this is a measurement report, but even so the standard of presentation falls well below what is expected, and the relevance of the measurements to atmospheric science generally (rather than locally to Cape Grim) is not explained.

Detailed comments (major)

The authors present in fig.3 a derivation of the BLH using the IEDA and gradient methods for 17 May 2019, concluding that the former method is best for the lidar and the latter for the ceilometer. I find this conclusion difficult to reconcile with the figure. Taking the left hand panel, the BLH is around 0.7 km from 3 to 12 h, falls to around 0.4 km by 15 h where it remains for the next few hours, before rising to a maximum around 0.5 km at 21 h and falling sharply to around 200 m at 24 h. The IEDA data, where shown, are consistent with the circles on the left hand panel. Contrast this with the right hand panel, where the IEDA line from 6 – 12 h is around 0.8 – 0.9 km, gently falling to around 0.7 km by the evening. This is almost twice the height from the ceilometer!! Furthermore, many of the circles on the right hand plot (notably between 14 and 18 h) are consistent with those on the left hand plot. The authors do not even comment on this discrepancy, and based on this evidence I conclude that they do not have a reliable method of deriving BLH from their data.

According to the final sentence of p.5, the shaded period E1 in fig 5 contains a sea breeze, and E2 a 'sea breeze and offshore interaction pattern', whatever that is. It is not clear at all how the authors come to these conclusions – how do they define a sea breeze? A reference to Caicedo et al 2021 is given but surely the criteria are not that complex? Then on l.198, we are told that during E1 and E2 the wind speed varied from 4 to 12 m/s, which is not consistent with the diagram. In E1 the wind varies from 2 to 8 m/s and in E2 from 2 to 6. The peak of 12 m/s occurs in the unshaded section between them, during westerly flow. During E3 the maximum wind speed in 5b is 12m/s, not 14 m/s as on l.200. On l.203 the wind vector is said to veer from SW to W from 20 to 23 June, but that isn't what is shown on 5a. Finally, the statement on l.208 that the ceilometer BLH were higher than the lidar BLH from 20-24 June is only true some of the time – at other times the ceilometer BLH is lower. Quite simply, this whole paragraph is inconsistent with the data presented and reflects carelessness on the part of the authors. Furthermore, it only becomes evident in section 3.2.3 why the periods E1, E2 and E3 were selected – this should be mentioned at the beginning of 3.2.

The interpretation in section 3.2.3 leaves much to be desired. For a start, it is repetitive and doesn't obviously make a point. E1 and E2 were periods of sea-breeze flow, so it is not surprising that the signal of land convection is seen as the air sloshes out to sea and back. On l. 244 the wind direction is said to change from SE to SW at 0900 on the 20th, which is simply not true – the change in direction is around 50°, not 90°, and the sodar shows a change to southerly (U=0). On l.247 a throwaway sentence attributes the overestimate of BLH in WRF to 'non-accurate prescription of surface roughness and induced turbulence', with no evidence at all. How did the 'weak sea breeze interact with the uplifted strong offshore wind' (l.250)? What does 'interact' mean here? On l.272 reference is made to lidar extinction – how was this measured? We've only seen backscatter mentioned up to now.

This is further compounded in 3.2.4 by a complete section on extinction coefficient. What algorithm was used to derive it? The backscatter signal does not give the aerosol backscatter coefficient (and hence the extinction coefficient by the assumed lidar ratio) directly. BLH may be derived from the signal profiles, but extinction coefficient requires a retrieval (as indeed is mentioned on l.288).

Detailed comments (minor)

p.4 l.120 and fig.2. More details are required of the IEDA algorithm as it is not possible to reproduce this study from the description given. For a start, axes and axis labels are needed in Fig 2 so that we can see the scales being discussed. Secondly, the colour scales should be shown, especially the grayscale in panel b where the white colour seems to correspond to an intermediate value of backscatter (why?). Thirdly, explain better how you go from 2b to 2c – a Gaussian kernel smooths a plot and you take a difference between this and the original plot in some way but I'm not sure how. I realise you give a reference to Xiang et al, but this paragraph needs to be clearer. And do you really mean 'corrosion' on line 124?

p.4 l.133 I can't see any green circles in fig 3b, though I can see a lot of scatter in the gradient method

p.6 l.222. You haven't analysed the characteristics of aerosols, you have used a trajectory model to calculate the source region of the air masses that passed over Cape Grim.

p.6 l.226 period

p.7 l.238 the nocturnal BLH is more like 200 m p.7 l.239 what is special about 0900? The change of wind with height is no different to the preceding 9 hours; if anything it is less

p.7 l.259 why is the low level jet baroclinic? The front is nowhere near Tasmania.