Editors review of paper acp-2013-295: Atmospheric boundary layer top height in South Africa: measurements with lidar and radiosonde compared to three atmospheric models, by K. Korhonen et al.

Dear author, co-authors,

I have read in detail the comments provided by the two reviewers as well as your response to their comments and the revised version of your ms on BL measurements and model simulations over Southern Africa. I see that this is indeed a unique dataset providing essential information also needed for air-quality assessments and it is interesting to see the comparison of this essential metric based on three different measurement technologies as well for three different modelling systems. It is also obvious that you have made a strong effort to deal with the comments raised by the reviewers that urged you to provide a major revision. Also because of this I have invited them to re-review this revision to see if your modifications are properly addressing the comments raised by them. In addition, you can find below a number of comments that I want to raise now reading over again in detail your paper. What I still see as one of the most essential issues is a careful analysis of the role of differences in surface energy balance (and how the models treat large-scale vertical motions) in explaining discrepancies between the models and observations.

Regards, Laurens Ganzeveld

“PBL top height is a crucial component in air pollution models because it determines the vertical space and consequently the volume for pollutant mixing, which is a key parameter for assessment of concentrations. Turbulence in air flow due to surface friction also affects the horizontal distribution of pollutants and is an important factor in weather forecast models. The PBL height cannot be directly measured by standard meteorological observations but it is a quantity that can be derived from the observations. The different parameterizations of models affect the precision of the simulated PBL height and therefore validation with measurements is essential (e.g. Hurley et al., 2008).”

“Lidar (light detection and ranging) systems provide continuous measurement of numerous atmospheric quantities including the PBL height”

Comment: First you indicate that PBL cannot be measured directly, must be inferred and then in this follow-up statement you refer to measurements of quantities including the PBL height??

“The authors studied the complex layer structure of Sahara dust and biomass burning aerosols observed at Praia, Cape Verde and how the African plume reached the South American coast. Campbell et al. (2003) have found lidar ratios between 50 and 90 sr, with the Ångström exponent of 1.5-2, for dense biomass smoke events during the South African Regional Science Initiative (SAFARI) 2000 (Swap et al., 2003).”

Comment; this statement contains some details that not all potential readers are familiar with. It would be worthwhile to explain in more detail the meaning of the lidar ratios as welll as the Angstrom exponent

“Secondly, wet soil (due to intensive 8 rainfall) has a greater heat capacity than dry soil, which reduces the adiabatic heating of air by the 9 surface and thus weakens the convective mixing;”

Comment: How much biomass is there around this area; I would deem the main impact of moisture status on PBL dynamics being the partitioning between sensible and latent heat flux where the latter is determined by available biomass, radiation and (soil) moisture. I think in one way you express in in a comparable way but it reads weird this wording,

Page 9: “It is worth mentioning that the models considered in this paper and the lidars (surface and space-borne) are able to detect the convective boundary layer (CBL) top during day-time, but during night-time the lidars detect the residual layer (RL) top and the models detect the top of the night-time stable boundary layer (SBL)”.

Comment: I see that this modification has been essential based on the reviewers comments but how it is now formulated still raises some questions. The behaviour described here reflects the conditions expected when a convective BL develops followed by a (strong) long-wave radiative cooling in the night triggering the formation of a SBL. But how often did this happen. I guess that there might have also been many nights with high wind speeds that would have prevented the formation of an SBL and that might have also affected the mixing conditions during the daytime and the role of the residual layer??

Page 10: “For this study we chose the four closest grid points surrounding the Elandsfontein lidar site, at distances of 24.5, 16.8, 18.8 and 5.9 km”.

Comment; this is not very clear? Did you select the four grid-points upwind of the site at those distances or??

Page 10: “The model gives the height of the stable boundary layer (SBL) in non-convective conditions, i.e. during times when the sun is below the horizon”.

I think it is pretty obvious that you can only get a SBL when there are non-convective conditions.....

From this section is now not that clear if you used the ECMWF data to calculate both the SBL and daytime BL depth or only the daytime CBL height.

Page 10-11; “The TAPM model defines the PBL height through the strength of convective updraft. During day-time, the PBL top is reached at the first vertical level where convective updraft decreases to zero and during night-time when the upward heat flux has decreased by 95 % or more from the surface value (Hurley, 2008)”.

What do you mean here with decreased by 95% of the surface value?? Is the flux at the top of the SBL (you refer to nighttime conditions) upward? What is the direction of the surface heat flux (I guess it will be downward)?? This statement could really be further clarified.

Page 13: “We found that the discrepancy between the PBL tops based on lidar measurements and radiosondes is larger in convective conditions (comparison between all 75 day-time soundings: slope

0.43, R 0.05) than during night-time (89 soundings), when the slope of the fit is close to unity (0.93) and R increases slightly to 0.06".

Comment; it was discussed by one of the reviewers that the lidar observations cannot be deemed representative for the nocturnal boundary layer depth and you indicated that you agreed with this comment . Then this statement about the comparison of the nighttime radiosondes and lidar should also be consistently modified. By the way, it is interesting but also in one way disappointing to see that there doesn't appear to be any significant correlation between the radiosonde and lidar based inferred PBL heights.

Page 14: "As previously stated, the radiosonde launch site (Pretoria) is 120 km from the lidar site, which 7 may explain some of the observed differences in PBL top height determination (Figure 4)."

This statement comes here out of the blue and suggest our to connect this directly to your analysis of the radiosondes versus the lidar or move this to the discussions section.

Page 15: "TAPM (Figure 6c) 2 also gives a similarly skewed normal distribution, but the median of 1200 m is approximately 400-500 m lower. The SAWS model distribution (Figure 6d) has a median value of 1205 m which is close to the value obtained from TAPM, but the standard deviation is higher (810 m versus 520 m)". Seeing these quite large differences between the observations and the models it would be interesting to see some short discussion on how these differences might relate to some of the basic drivers of PBL evolution , surface energy partitioning. How much difference is there in these models in the simulated sensible heat fluxes and what are differences in treating the role of subsidence in these models?

Page 15: "In the subsequent sections, we will present the lidar-model comparison results in more detail. As mentioned earlier, the lidar data set was selected as the base for the comparison with the other techniques, due to its good temporal and vertical resolution".

So you choose the lidar observations as the reference although the comparison of these lidar observations with the radiosondes showed a very poor correlation on PBL depth estimates. Your main argument could be the large distance between these observations but think that this essential assumption for the follow-up analysis requires a stronger justification.

Page 18; at the end of this discussion on the comparison of the models PBL heights with the lidar observations I get the impression that you are missing some essential details of analysing the reasons for some of the discrepancies. The models temperatures might be OK and then some of the differences are attributed to issues on the wind speed. You also shortly mention the role of global radiation but no wording about the simulation of energy partitioning which depends strongly on vegetation properties/activity and surface moisture status. This is key to understand some of the discussed differences.