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Interactive Comment

Interactive comment on "Role of the residual layer and large-scale subsidence on the development and evolution of the convective boundary layer" *by* E. Blay-Carreras et al.

E. Blay-Carreras et al.

estel.blay@upc.edu

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We would like to thank the positive review of the referee. All the comments have been really useful to create an improved new version of the manuscript. Below we answer all the comments made by the referee.

Referee 2 comments

1. On the wind structure: the paper is not clear on the treatment of the wind. No wind profiles are given, neither the criteria why these particular profiles have been chosen or what are the surface boundary conditions (z0, u*). Since shear production is a major

point here, it is necessary to document well all these points and to put them in relation to observations. In particular I question myself on how these choices are related to the observations in the campaign, where heterogeneity of the terrain was significant. Also the jump from the BL to the FA seems very large (from 3.5 to 10 m/s). How this transition is imposed? Do these 10 m/s correspond to actual observations or numerical model analysis? In the initial part of the runs (when CBL is not yet developed) do you take 10 m/s just above the surface inversion? If so, is this in agreement with the observations that night? As mentioned, the paper would become much clearer if all these issues were well discussed and the decisions taken well justified.

The description of the wind profiles was already included in the previous version of the manuscript. Nevertheless, we have included in the new version of the manuscript the wind characteristics in Table 1 and an additional plot in the new Fig.2 showing the observed wind profile at 07:30 UTC and the initial wind profile used in the numerical experiments. Additionally, we include the value of roughness length used in DALES.

However, it is important to note that, taking into account that DALES numerical experiments do not include real topography of the site, it is almost impossible that numerical simulations were able to fit the observed values of the wind speed or direction because on the day under study the dynamics of the atmosphere was dominated by mesoscale processes such as mountain-valley flows.

Regarding heterogeneity of the terrain, it was not considered in DALES numerical experiments. The paper is focused on the role of the residual layer during the morning transition.

2. On the LES choices: the domain is large enough and the statistics make sense. The resolution (not explicitly given) is 50 m in the horizontal and 10 m in the vertical. Some justification on why these resolutions are taken is missing, especially since this is a sensitive issue at the entrainment zone. A more elaborate description on why the prescribed fluxes are used as a sinusoidal form (only mentioned in table 1) is needed,

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especially when there is so much observational information. At least a comparison of these formulae with the observations at the central site would be needed and justified.

We have extended the explanation about the LES numerical settings. Additionally, a figure (new Fig. 1) showing the observed and prescribed surface heat fluxes has been added.

To justify the LES setup we have included new references dealing with similar resolution.

3. On the interpretation of the TKE budget: the focus is put on RL versus no-RL in absence of subsidence. I miss the interpretation with/without subsidence for completeness. Besides it is difficult to comprehend the role of shear production (SP) without knowing how the wind behaves (see comment #1). Please clarify how the different terms are computed (for instance, average of the vertical gradients or vertical gradient of the averaged profile?). I find very interesting the result in figure 7 that indicates that SP is much smaller without RL. It is partially attributed to a smaller vertical integration domain. I believe this interpretation would be much enriched if the profiles of the fluxes and the gradients were shown for both cases (or the 4 cases even better). Assuming that within the central part of the CBL the gradients of the mean wind are small, most of the contribution to this term should come from the surface and the entrainment layers. If most of the differences between RL and no RL take place at the top of the CBL, this would mean that SP there is smaller when RL is not present, which is a result that deserves further discussion.

In the new version, the effects of subsidence on the TKE are analyzed by using figures 7 (old figure 6) and 8 (old figure 7).

We have added an additional plot to Fig. 8 (old Fig. 7) where influence of subsidence on the evolution of TKE terms can be analyzed. New sentences commenting this new figure have been included.

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If the referee refers to the old figure 6, DALES output directly provides every 5 minutes the vertical profile of each TKE term. However, DALES calculates every time step the momentum fluxes and the gradient of mean wind. Consequently, the profiles shown in Fig. 7 (old figure 6) are time and space averaged from DALES output.

We have included a sentence describing why the differences in the integrated TKE cannot be due to a different integration domain.

We have included in the discussion of TKE the role played by the wind characteristics.

4. On the significancy and novelty of the results: I believe more detail is needed in the conclusions on how this study is bringing new insight in the study of the sheared CBL, especially through the use of the observations of the BLLAST campaign.

The present research is not focus on analyzing sheared CBL. Therefore, the conclusions are not focus on this topic. The main objective is to analyze the importance of RL on the BL evolution. Nevertheless, we have extended the discussion about the role of the shear in the TKE.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31527, 2013.