Referee 1 comments

1. The heart of the paper is the comparison of simulations initialized with two different profiles, with (RL) and without (nRL) a residual layer. The profile without a residual layer has considerably less total heat than the residual layer profile. After the BL grows to nearly its final height, the nRL simulation must necessarily have a lower temperature. This is not really a flaw in the design, but it must be taken into account when interpreting the results. For example, the first paragraph of section 3.3 should be reconsidered with this in mind.

   We do not know if the referee has misspelled the comment. Obviously, the nRL numerical experiment has larger temperature for most of the day. Through a budget analysis, taking into account that the same amount of heat is introduced in the mixed layer at the surface, temperature is larger mainly due to the lower boundary layer height simulated for the nRL cases (see figure 5) and larger entrainment heat flux (see figure 6). We have clarified this fact in the new version of the manuscript.

2. p.31536 line 20ff: It would be helpful to show profiles just before and just after the residual layer is incorporated. These are shown in figure 4, but not called out as such, and forward referencing is generally not allowed.

   We have changed the order of section 3.1 and 3.3 in the new version of the paper. Therefore, we can call out the figure at p.31536 line 20 because it is already incorporated.

3. p.31538 line 8: The roughly constant BL depth in the afternoon may be partly due to subsidence, but since the mean BL temperature also is roughly constant, advection cannot be ruled out.

   We do not agree with the referee. As it is clearly shown in figure 4 (old figure 2), the observed potential temperature in the BL decreases during the afternoon. Cold heat advection could exist during the analyzed day, but it is influence is small or it's only acting during late afternoon because without including its contribution we are able to approximately reproduce the observed temperature.

4. last paragraph of section 3.3: The interpretation is unclear here. The incorporation of the residual layer and the increase in magnitude of heat flux are not (cannot be) simultaneous. While the residual layer in being incorporated, the BL is in a "free encroachment" regime and the entrainment heat flux must be nearly zero because the temperature jump is zero. After the BL reaches the RL top, the jump is re-established and the flux takes on a new value. This could be shown by including the BL top trace on figure 5.

We have included in figure 6 (old figure 5) a line, which shows the evolution of the entrainment heat flux at the top of the RL before the merging with the CBL. Moreover, we have included new sentences accordingly.

5. Related to the previous comment, the authors rely on eq. 2 for much of their interpretation. They should keep in mind that the two terms interact and compensate. A large jump should lead to a small growth rate, for example.

   We agree with the referee. The sentence has been modified accordingly.

6. A note on language: I prefer that authors express themselves in their own voice as much as possible. While some of the English in this paper is imperfect, I found it clearly understandable throughout.

   We have reduced the passive voice it in the new version of the article.

Specific comments:

1. p.31537 line 15: The main reason that the effect of subsidence can only be appreciated at the end of the afternoon is that its effect is cumulative.

   We have modified the sentence.

2. Figure 3: It should be noted that the determination of z from the first sounding is erroneous, the analyst or algorithm has mistaken the residual layer top for the BL top.

   We have changed the color of the point to avoid confusions in the analysis of the figure. Moreover, we have included a comment in the caption of this figure (new figure 5).

3. p.31546, line 19: The winds are mentioned here for the first time. Because shear is important to entrainment, wind profiles (initial and at other important times) should be included in table 1 and added to the appropriate figures.

   We already described in section 2.2 the initial and geostrophic wind characteristics. Nevertheless, we agree with the referee and we have included the information about the initial wind characteristics in Table 1 and in the new Fig. 2. Moreover, in the new version, when TKE is analyzed the role of wind shear is also discussed.

   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 the numerical simulations are 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 synoptic processes, such as mountain-valley flows (mesoscale conditions).

Technical corrections:

1. Abstract, line 19: "buoyancy heat flux" should be either buoyancy flux or heat flux.

   The referee is right. We have avoid the use of buoyancy heat flux in the new version.

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 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 the 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, 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 complete- ness. 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.

   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.

List of all relevant changes made in the manuscript

- We have included Fig. 1 showing the observed temporal evolution on 1 July 2011 of sensible and latent heat fluxes and the prescribed evolution to drive DALES numerical experiments.
- We have included Fig. 2 c and d showing the vertical profile of wind speed and wind direction observed by the radio soundings launched at 01:30 and at 07:30 UTC and the vertical wind profiles for initializing DALES.
- We have moved the Eq. 1 and Eq. 2 to the beginning of section 3.
- We have modified the order of the results. In the new version, section 3.1 deals with potential temperature vertical profile, section 3.2 with mixed–layer potential temperature temporal evolution and section 3.3 with boundary–layer depth temporal evolution.
- We have created a new section about the evolution of the entrainment heat flux during the morning. The results of this section were previously located in the section about potential temperature vertical profile.
- We have modified Fig. 6 (old Fig. 5) including the entrainment heat flux at the top of the RL.
- We have modified Fig. 7 (old Fig. 6) and 8 (old Fig. 7) to include DALES results with subsidence and the BL depth (Fig.7). We have included some sentences describing the importance of subsidence.
- We have discussed more in detail the importance of shear in the analysis of the turbulent kinetic energy budget.
- We have modified the conclusions accordingly to the new manuscript.