Response to reviewers

Turbulence vertical structure of the boundary layer during the afternoon transition

We thank the reviewers for their helpful and constructive comments, which helped us to improve the manuscript.

All of the specific comments have been taken into account.

In response to the reviews we have modified some parts of the manuscript, including :

- An improved discussion concerning the different TKE decay regimes found in our study compared with literature,
- An improved discussion concerning the 'top-down' decay of turbulence found in LES.
- A more exhaustive bibliography connected with our findings,
- Complementary information on the LES model and the numerical settings (subgrid model, boundary conditions...),
- Missing acronyms definition
- Revisiting the spectra shape and scales changes based on a better analysis of Figure 8,
- Rewriting of the sentences which were not clear to the reviewers,
- Changing Figure 4: the new Figure 4 shows less profiles, with different colors used for more clarity, as suggested by reviewer 1.
- Adding the initial LES profiles of θ and wind direction on Figure 2.

Below there is a copy of the reviewer 2 comments (in italic and blue), with a detailed response to each points.

Responses to Reviewer 2 :

This paper concerns an LES of the afternoon transitional boundary layer, focusing particularly on the spectral characteristics of the decaying convective turbulence. The simulation is based on a well-observed field experiment and comparisons with the observations are included. It is a useful addition to the literature on the BLLAST experiment and on transitional boundary layers more generally. My comments are mainly requests for small clarifications, but, more substantively, I think that additional discussion of the budget of TKE, shown in Fig. 6, would be useful.

1. p. 32498, L. 16. Does advection here include subsidence

Yes, we prescribed total advection, which includes horizontal and vertical (due to subsidence) advection.

2. p. 32499, L. 14. What is the height of the flux measurements over the individual surface types? Does the 60m tower have a large enough footprint to give a domain average, even in the most unstable cases, or is the predominance of the moor surface type the key point?

The flux measurements were made between 2 and 5 m over the different surfaces, depending on the vegetation height (see Lothon et al. 2014).

We think that the 60m mast measurements are representative of the region except for some wind directions : for instance in case of south west winds, the measurements might not be representative because a large forest has then more influence in the measured flux (see a study on area averaged flux in http://bllast.sedoo.fr/workshops/february2015/presentations/Hartogensis-Oscar_area-averaged-flux.pdf).

Besides, about 40 % of the plateau is covered by moor and grasslands (very similar types of surfaces), which make the moor fluxes the most representative of the region.

In the revised manuscript, we have specified the reason why we consider the moor vegetation as the dominant vegetation over the plateau, in section 2.2, l.240 :

“As such, H measured at 60 m height is encompassed in all the others and is close to the moor and grass, the dominant vegetation, representing about 40% of the covers over the plateau.”

3. p. 32499, L. 15. Do you necessarily expect the latent heat flux to reach 0

In case of larger surface wind speed, we could have expected the latent heat flux not to be zero, but during the BLLAST campaign low surface winds were observed and consequently, almost zero latent heat flux. However we wrote the sentence in a more general way:
“This delay is observed for all the intense observation periods (IOP) of the BLLAST campaign implying that the latent heat flux reaches its minimum value systematically later than the sensible heat flux.”

4. p. 32500, L. 1. The boundary conditions need to be described more prominently. I think moving the sentence "A simulation is initialized...advection." to the top of section 3.1 and adding "observed surface heat flux at the moor site" would make this more obvious. The sentence on lines 17 and 18 of this page could then be removed.

The suggestion to specify the boundary conditions in the beginning of the paragraph has been taken into account in the revised manuscript. We also realized that we forgot to mention that the lateral conditions are cyclic. The new paragraph in section 3.1, l. 273 is :

“Our LES is initialized with early morning radiosoundings and forced with homogeneous surface fluxes, based on those measured over the moor surface. Temperature and humidity advection are prescribed. The side wall boundary conditions are periodic.

The LES code from National Center for Atmospheric Research (Moeng (1984), Sullivan and Patton (2011), Patton et al. (2005), Lohou and Patton (2014)) is based on the Boussinesq equations, including conservation laws for momentum, mass and the first law of thermodynamics.”

5. p. 32502, L.3. It would be interesting to relate Fig. 6 to the budget of TKE, including production, shear etc., to explain why the region of negative buoyancy is deeper. Perahps figures of the non-dimensional budgets at the start of the AT, at the end of the first phase and at the end of the second phase would be useful. As the authors note, in the real atmosphere there was more shear at the top of the BL, so their idealization will underestimate entrainment, but it should be conceptually helpful in underestanding the decay of convective turbulence.

This is a very good remark and we did verify before the submission of this paper if the TKE budget could help us to understand these two steps in the TKE decrease and the demixing height evolution. As you can see in Figure 1, whereas all the TKE budget terms decrease during the AT (left panel), their respective contribution to the TKE tendency is not evolving significantly and can explain neither the negative layer deepening nor the two stages of the TKE tendency. From our point of view the demixing and TKE decay processes are scale dependent and cannot be seen on statistical moments.

Without adding any figures in the text we introduced a comment on the TKE budget in the discussion, section 5.4, l.825.:

“ The TKE budget evolution in time was of any help to explain the two stages of the TKE decrease. Whilst the different terms do decrease with time, their respective

contribution to the TKE tendency hardly change from the first to the second stages (not shown).”

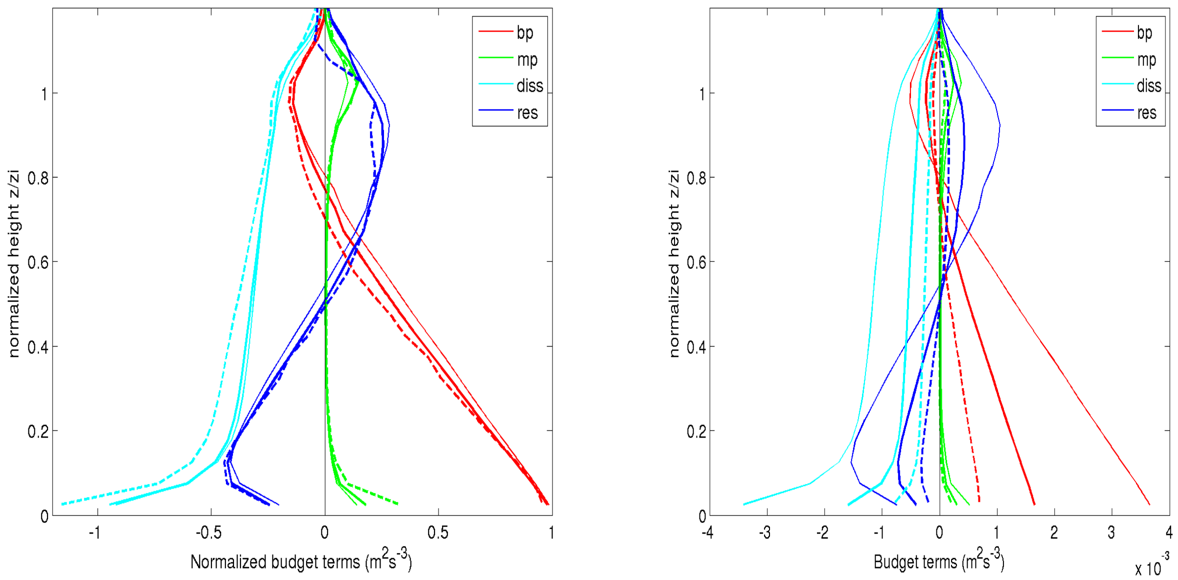

Figure 1: 30-min averaged TKE budget terms normalized (left panel) and not normalized (right panel) at 12:00, 16:00 and 17:00 UTC (thin, thick and dashed lines, respectively). “bp”, “mp”, “diss” and “res” stand for the buoyancy production, the mechanical production, the dissipation and the residual term which should correspond to the transport.

6. p. 32503, L. 13. This paragraph is confusing: "Despite...nevertheless...Despite". It’s not clear whether you think the LES is good enough or not. Please be more specific about which aspects of the LES are expected to be realistic and where caution is appropriate.

We totally agree that this sentence was not clear. We have reformulated this sentence in the revised manuscript, in section 3.2, l. 437:

“In summary, the simulated boundary layer is comparable to the observed one in terms of boundary layer height, wind speed, and dynamical and thermal stability near the surface. The lower development of the PBL height of about 200 m and the underestimated TKE by a factor of 1.5 can be explained by the directional wind shear which is not simulated. The latter might increase the entrainment and the turbulence dynamical production at the top of the boundary layer. Despite these differences on the main PBL structure, the simulation is realistic enough to evaluate how the turbulence evolves in a convective boundary layer during the AT and the comparison of simulated and observed boundary layer will be analyzed accordingly.”

7. p. 32506, Eq. 16. I wondered whether a weighting with SKL89(k) would improve the measure, so as not to overemphasise noise in weaker parts of the spectrum.

From this suggestion, we have investigated the temporal evolution of the index of quality, by weighting the KL89 analytical spectral model.
The formula becomes :

$$IQ_{weighting} = \sum_k \left(S_{KL89}(k) \log\left(\frac{S_{OBS}(k)}{S_{KL89}(k)} \right) \right) \frac{1}{\sum_k S_{KL89}(k)}$$

Figure 2 shows the temporal evolution of $IQ_{weighting}$. This weighting does not improve neither the tendency nor the intensity of the error and does not help to better detect the poorest fits. Considering that, we kept our definition of *IQ*.

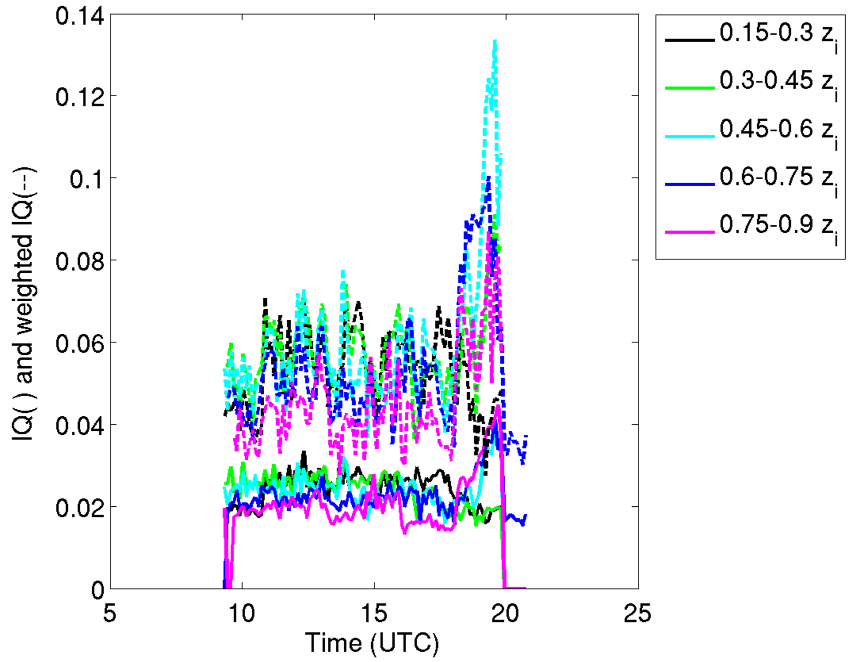

Figure 2: Temporal evolution of the quality index with (dashed lines) and without (continuous lines) weighting of the KL89 analytical spectra.

8. p. 32507, L. 19. I was confused here. Fig. 8 shows higher values of k at 18:00 UTC, implying shorter wavelengths, yet you say Lambdaw increases.

Yes, it was indeed a mistake in our written comment of those figures. Figure 3 represents the temporal evolution of Λ from LES and aircraft observations. Λ slightly increases until 1630 UTC then decreases. With the simple concept that Λ represents the distance between two structures and l_w represents the width of a structure, this means that during the LAT, the thermals become closer from each others whereas the increase of l_w means the thermals become larger. This is consistent with a decreasing skewness of w as time evolves, which we do find in both observation and LES.

We have corrected the article and added this discussion in the revised manuscript in section 5.2.3, l. 731:

“As noticed in Fig. 8, Λ_w drifts slightly toward smaller eddies. Keeping in mind that Λ_w represents the distance between two structures and l_w represents the width of a structure, this means that during the LAT, the thermals become closer from each others whereas the increase of l_w means the thermals become larger. This is consistent with a decreasing skewness of w as time evolves, which we do find in both observation and LES (not shown).”

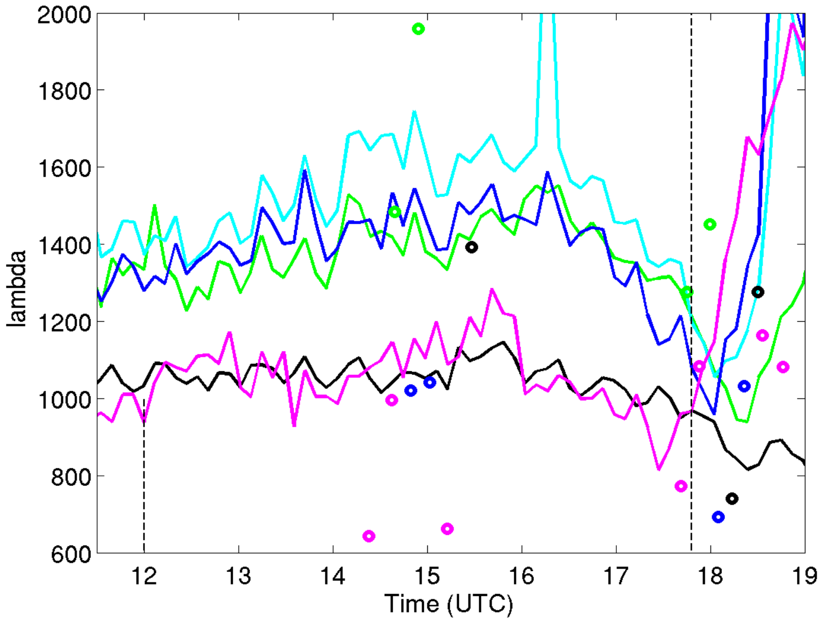

Figure 3: Temporal evolution of Λ observed (open circles) and obtained by LES (continuous lines) at different heights (same color code than for other figures).

9. p. 32508, L. 9. Do you mean "decay of TKE" rather than "decay of TKE dissipation rates"?

No we do not. It is the decay of TKE dissipation rate that we are talking about.

10. p. 32509, L. 13. An explanation of why anisotropy or coherent structures could explain this is needed.

Theoretically, a fundamental hypothesis for the -2/3 slope in the inertial subrange slope for kS(k) is isotropic turbulence. So one may wonder if this slope remains at -2/3 for anisotropic fields.

We have added the following discussion in the revised manuscript in section 5.2.1, l. 665 :

“The theoretical -2/3 slope is based on the hypothesis of isotropic turbulence. Therefore, a possible explanation for these steeper slopes in convective conditions could be the loss of isotropy in real conditions and in particular the role of convective structures and the associated anisotropy. As mentioned before, in section 5.1, they are responsible for anisotropy smaller than one. We believe that the more 'coherent' or organized the w field, the smaller the anisotropy and the steeper the slope. But this explanation needs further work for confirmation.”

Note that in this KL89 analytical spectrum, anisotropy of turbulence is taken into account only by varying integral scale from transverse to lateral spectra. Even considering anisotropy, the spectrum follows the usual -5/3 slope in the inertial subrange.
This comment has been included in the revised manuscript.

11. p. 32510, L. 23. Define LAT

This acronym has been defined in the revised manuscript.

12. p. 32511, L. 18. Does the decrease actually propagate, or does is it simply that surface-driven turbulence does not rise so high?

This is a good point that has been clarified in the manuscript. We agree that “propagates” is probably not the most appropriate word to explain this, and that it is more that surface-driven turbulence does not rise so high. Since the surface fluxes decrease during the afternoon transition, the turbulence produced at surface does not reach the top of the CBL anymore. Since there is also no dynamical production at the top of the PBL in our case, this induces that turbulence decreases first at the top of the PBL whereas it is maintained longer at surface.

We have modified the revised article accordingly in section 5.3, l.754 :

“That is, once the surface flux starts to decrease, the surface-driven turbulence does not rise up to the top of the CBL anymore. This induces that turbulence decreases first at the top of the PBL whereas it is maintained longer under 0.15 z_i.”