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 1 comments (in italic and blue), with a detailed response to each points.

Responses to Reviewer 1 :

GENERAL COMMENTS:
The paper deals with an interesting problem of the boundary layer meteorology. The turbulence decay at the sunset is reproduced and analyzed using a LES model coupled with experimental data that are acquired during the BLLAST field experiment. This experiment is an international cooperation between some European Institutions and NCAR. The results show some interesting and new aspects that merit to be published. Besides that, I have the following two questions.

- The first main conclusions drawn from this study is that the decay process is generically divided in two phases, which is the early afternoon (0-0.75tau_f) and late afternoon (0.75 tau_f - 1.0 tau_f). In which way this is compatible with the recent findings in which the exponent of the decay rate has instead three different scaling regimes (t^{-1}, t^{-2}, t^{-6}) ????

Our finding is that the TKE decay follows two main stages: we define the first period that we call the early afternoon in the temporal range from 0 to 0.75 τ_f and then the late afternoon (from 0.75 τ_f to 1.0 τ_f). This result remains consistent with the recent findings of three stages in Rizza et al. (2013a), based on LES. The difference comes from the way of defining those stages. Figure 1 represents the temporal evolution of the coefficient -n which governs the TKE decay following a power law in t^{-n} when representing TKE/w_*^2 as a function of t/t_* with a log-log representation. The expression reads:

$$\log\left(\frac{TKE}{w_*^2}\right)=-n\log\left(\frac{t}{t_*}\right) \quad .$$

This coefficient evolves all along the afternoon period (see Fig. 1). Since the coefficient tends progressively from 0, to very large negative values of -n, we find actually somehow arbitrary to define a specific value for this coefficient that would characterize the different phases of the TKE decay. For example, Nadeau et al. (2011) defined two stages defined by the power laws t^{-2} and t^{-6} (see Figure 3 (b)) whereas Rizza et al. (2013a) added a preliminary phase in t^{-1} (see Figure 2 (a)). Our first stage includes t^{-1} and t^{-2} power laws, and the second one includes t^{-6}. Note that the goal of our study is rather to link the decay stages with the evolution of the turbulence structure.

We have rewritten the manuscript to connect our findings in relationship with the recent findings in section 5.4, lines 807:

"The two stages of the TKE decay found in this study remain consistent with previous results found by Nadeau et al. (2011) and Rizza et al. (2013a). Both authors showed a decrease of the TKE following a t^{-n} power law with a continuous increase of n. Nadeau et al. (2011) defined two main stages characterized by n around 2 and 6 respectively. Rizza et al. (2013a) added a preliminary stage with n equal to 1. Our first stage includes t^{-1} and t^{-2} power laws, and the second one includes t^{-6}. However, it seems somehow arbitrary to characterize our two stages by a specific value of n since it evolves continuously. This study focuses on the link between the structure of the turbulence and the TKE evolution."

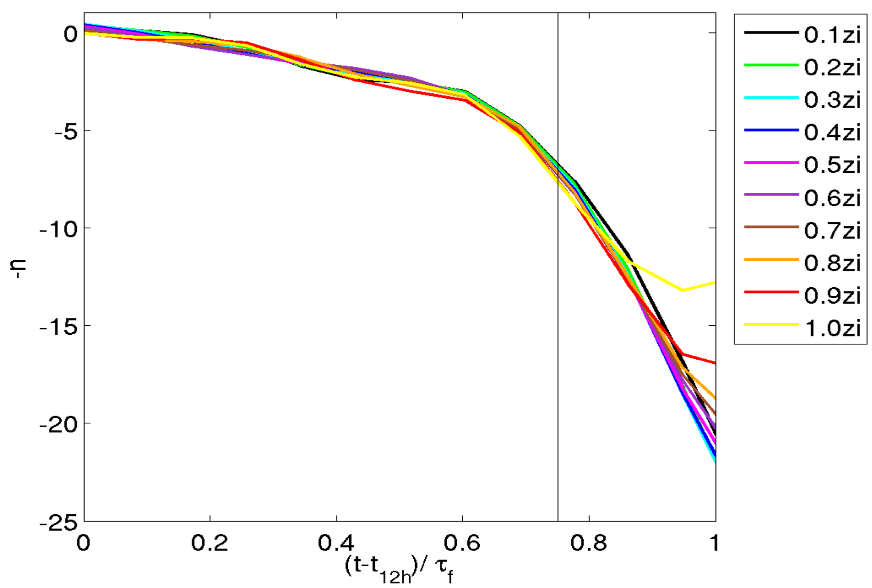


Figure 1: Temporal evolution of the coefficient n which governs the TKE decay following a power law in t^n when representing TKE/w_*^2 as a function of t/t_* with a log-log representation with LES data. Time is normalized so that the afternoon transition starts at 0 and ends at 1.

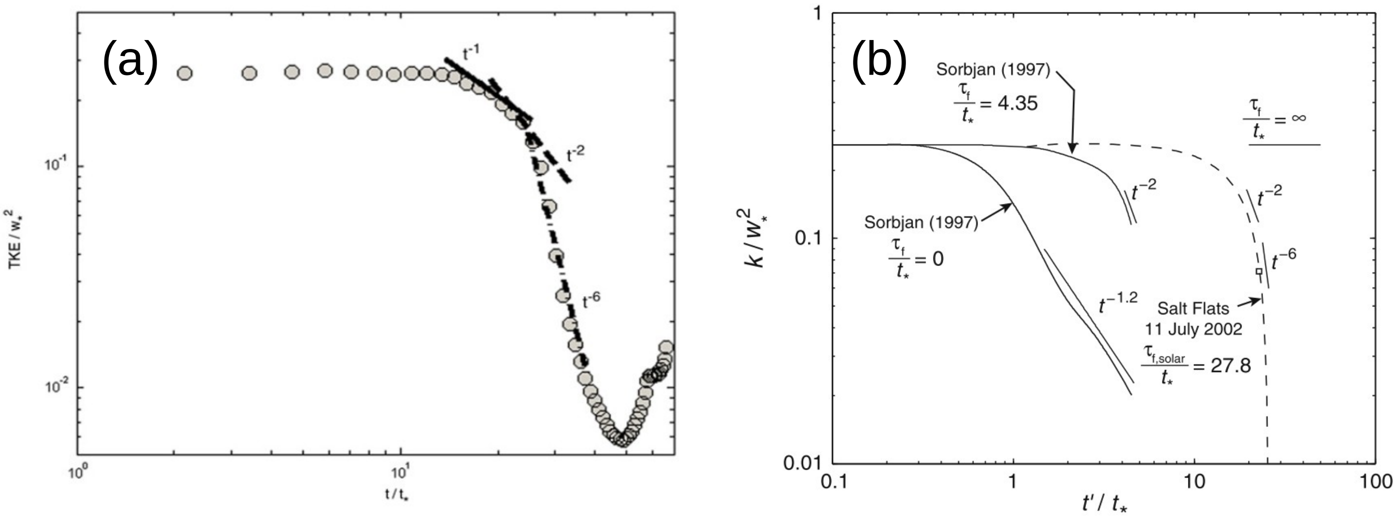


Figure 2: Temporal evolution of the volume averaged TKE over the boundary layer depth from (a) Rizza et al. (2013)a and (b) Nadeau et al. (2011).

- The second important point concerns the "turbulent evolution along the vertical". An important conclusion is that the decay occurs first at the top of the boundary-layer then it propagates downward toward the surface. But as the authors declared, there are some "noticeable differences" (pag.32503, line 15) between the observed and simulated mean profiles (fig.4) and for the TKE (fig.7) as well. Furthermore, the estimation of zi underestimates the observations while the surface stability conditions are not discussed at all. So, I think that these aspects should be more deeply investigated and in particular if this conclusion is not influenced by the poor LES prediction of mean and turbulent quantities.

As the reviewer correctly mentioned, there are some noticeable differences between the LES and the observations. But our aim was not to reproduce a real case. We have modified the text in Section 3, l.263 to clarify this point:
"As a complementary tool, a LES is initialized with the BLLAST observations to study turbulence decay over an homogeneous and flat surface. The observations of the 20 June are used to guide our simulation, like the 1 July and 25 June guided the studies of Blay et al. (2014) and Pietersen et al. (2015) respectively. Our aim is not to reproduce a real case but rather to use the BLLAST dataset as a benchmark to simulate a boundary layer with the same range of thermal and dynamical instabilities than those observed during BLLAST."

The difference in terms of stability and wind shear are discussed in the response to the specific comments.

Moreover, despite the simulation shows a 'top-down' decay of turbulence, we are not sure that it actually occurred in the reality for this day. Indeed, we claim in the article that the simulation shows this 'top-down' evolution likely because there is no wind shear at the top of the boundary layer to maintain the production of turbulence. In the observations, the directional wind shear at the top of the BL might have maintained the production of turbulence in the upper layers, so that the 'top-down' process might be reduced. We can not confirm the 'top-down' process with aircraft data but the temporal evolution of the TKE dissipation rate obtained from the UHF on 20 June 2011 (see Figure 3) reveals a slight 'top-down' process, less pronounced than in the simulation, which is consistent with the presence of more important shear at the top of the BL.

The following paragraph has been included in Section 5.1, l. 616:

"In this study, the simulation shows this top-down evolution likely because the shear in wind direction at the top of the boundary layer is weak and does not maintain the dynamical turbulence production. We can expect a reduced top-down effect in the reality since there is shear in direction which is not simulated."

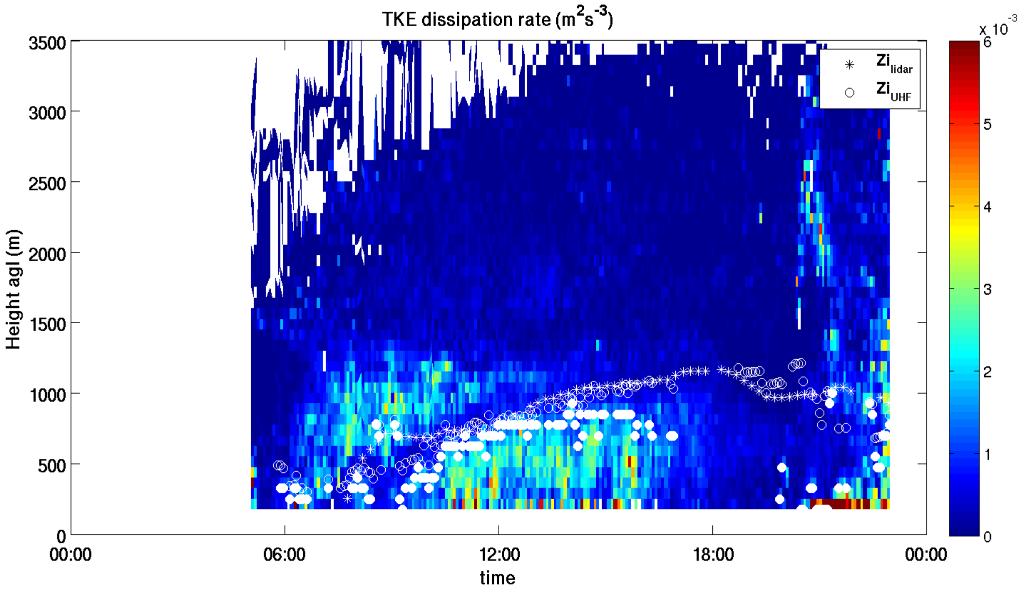


Figure 3: Temporal evolution of the TKE dissipation rate obtained from the UHF on 20 June 2011. Two estimates of z_i from lidar and UHF are also represented by white symbols.

Regarding the formal aspect of the manuscript; (i) the references, especially in the introduction are not really exhaustive, there are recent works that are not mentioned at all and (ii) the description of LES model is quite concise: which SFS modeling and geostrophic forcing ????

The revised manuscript includes a larger bibliography and more description of the LES model. These points will be discussed later in the specific comments. We thank the reviewer for the references suggestions.

SPECIFIC COMMENTS
pag.32494, line 10 There are recent works that should be mentioned, among the others:
- Carvalho JC, Degrazia GA, Anfossi D, Goulart AG, Cuchiara GC, Mortarini L (2010) Simulating the characteristic patterns of the dispersion during sunset PBL. Atmos Res98:274–284
- Taylor, Alexander C., Robert J. Beare, and David J. Thomson. "Simulating Dispersion in the Evening-Transition Boundary Layer." Boundary-Layer Meteorology 153.3 (2014): 389-407.
pag.32494, line 13 I would mention also the analytical studies of: A.G. Goulart, G.A. Degrazia, -
- U. Rizza, D. Anfossi, A theoretical model for the study of the convective turbulence decay and comparison with LES data, Boundary-Layer Meteorology 107 (2003) 143–155.
- A.G. Goulart, B.E.J. Bodmann, M.T.M.B. de Vilhena, P.M.M. Soares, D.M. Moreira, On the time evolution of the turbulent kinetic energy spectrum for a decaying turbulence in the convective boundary layer, Boundary-Layer Meteorology 138 (2010) 61–75.

pag.32494, line 25 I would add: Rizza et al (doi:10.1016/j.physa.2013.05.009) as 2013b

These references have been included in the revised manuscript. As already mentioned above, the discussion concerning the TKE decay has been enlarged in order to better articulate our study among the previous ones.

In addition to this, we have referred to Goulart et al. (2010) several times in the revised manuscript

pag.32497, line 20 Please explain acronyms Ibimet and Isafom

These acronyms have been defined in the revised manuscript.

Pag.32499 – chapter 3 LES - Which SFS model have been used ? the standard NCAR code use: P.P. Sullivan, J.C. Mc Williams, C.-H. Moeng, A subgrid model for Large- Eddy Simulation of planetary boundary layer flows, Boundary-Layer Meteorology 71 (1994) 247–276.

The following sentence has been added in the revised version in section 3.1, l. 283:
“The subgrid-scale model includes a turbulent-kinetic-energy eddy-viscosity model suggested by Deardorff (1980), used by Moeng et al. (1984) and improved by Sullivan et al. (1994).”

- The NCAR-LES code use the geostrophic wind as a surrogate of the large-scale horizontal pressure gradient, please comment which value is being used.

Reviewer 1 pointed out an important information that was not clearly given in the manuscript. We did not prescribe any geostrophic wind. We simply initialize the simulation with a vertical wind profile obtained from a simplified 0515 UTC radiosounding, and let it evolve without any forcing. Consequently, the simulation is not able to reproduce the change of wind direction that is visible in the observations.
We have clarified this in the text in section 3.1, l.304:
“The wind, potential temperature and specific humidity initial profiles were deduced from the 0515 UTC radiosounding (see dashed lines in Fig. 2 for temperature and wind speed). No geostrophic wind is prescribed. This simple representation of the wind leads to a simulation with very low wind speed as it is the case in the observations, but does not allow to simulate the shear in wind direction.”

Pag.32500 – line 15 What “simplified” does it mean exactly ???

The wind and potential temperature profiles have been added on Figure 2 (in black dashed lines) in the revised paper and show approximately two distinct layers.
In the revised manuscript, we have better introduced the initial profiles and fig 2, in section 3.1, l. 304:
“The wind, potential temperature and specific humidity initial profiles were deduced from the 0515 UTC radiosounding (see dashed lines in Fig. 2 for temperature and wind speed).”

Pag.32500 – lines 19-23 Concerning the large-scale advections, why (u,v) predictions from AROME model are not used ????
As we already mentioned earlier, the goal of the simulation was not to simulate a real case in its whole complexity, but rather to build a reasonable mean BL structure, that is close enough to the observed structure, in order to analyze what turbulence structure evolution would be built in the LES. We did not find appropriate to prescribe wind advection with such an idealized wind profile.
We better explained our goal in Section 3, l. 265:

“Our aim is not to reproduce a real case but rather to use the BLLAST dataset as a benchmark to simulate a boundary layer with the same range of thermal and dynamical instabilities than the one observed during BLLAST.”

Pag.32500 – chapter 3.2 - Figure 4 is a bit confusing, please use less profiles with a different choice of colors (red-blue-black) and thicker lines. I think that there are significant differences between LES and OBS profiles for all variables, especially for the wind speed. For example for the 1800 UTC (yellow line) profile, the observed wind speed at 1100 m is almost zero while the LES prediction is 6 ms-1, while at the same hour the DT (LES-OBS) is almost 3K. - Furthermore it would be interesting to see a zoomed view of WS in the first 100m. - The LES vertical domain is 3072 m while the figures are up to 2000m.

- Figure 4 of the manuscript has been changed. The new Figure 4 in the revised manuscript shows less profiles (at 0530, 1130, 1330, 1730 UTC), with different colors. Since we focus on the PBL evolution and not on what happens far above, the chosen representation is a compromise in order to see correctly both the first 100 m and the entire PBL. Figures 4 and 5 show the zoomed and un-zoomed view of the figure 4. We do not think it is worth to add these figures in the revised manuscript, since for our purpose, there is no specific need to show the profiles above 2000 m.

- In the boundary layer, the evolution of θ is well reproduced. At 1100m, the 3K difference between the LES and the radiosounding at the top of the boundary layer is rather explained by a z_i departure rather than a departure in θ in the BL.

- At 1800 UTC, the observed wind speed at 1100m is almost zero due to the directional wind shear at the top of the PBL, which is not reproduced in the simulation.

- Close to surface, we do not expect the simulation to reproduce perfectly well the observations, since we compare an idealized surface relatively to a complex real surface. In the first 100 m:
  - The differences of wind are small: the wind speed is weak (between 2 and 4 m/s in the PBL) in both observations and LES.
  - The differences of super-adiabatism at 1130UTC might be due to the different location from where the radiosounding was launched, which might be warmer than the moor surface. Moreover, the stronger wind shear might explain why the suradiabatic layer seems thicker in the observations. The difference in stability timing observed on the1730 UTC profiles are also discussed in the revised version.

More discussion has been included in the revised paper, in section 3.2, l.353:

“ In the first 100 m, the differences of stability profile at 1130 UTC might be due to the different locations of the soundings and the moor site where the surface flux is observed. The 1730 UTC LES profile is already neutral, whereas the observations at 1750 UTC still show super-adiabatism. The differences are due to the fact that as soon as the surface buoyancy fluxes turn negative, the LES potential temperature profile becomes stable at the lower layers of the BL. This delay between the time when the buoyancy flux goes to zero and the time when the local gradient of virtual potential temperature changes sign has been observed and analyzed in Blay et al. (2014). It can be of the order of 30 min or 1 hour.”

Figure 4: Zoom of the first 500 m of the vertical profiles of θ, r, wind speed (WS) and wind direction (WD) observed (solid lines and dotted lines for WD) and obtained by LES (dashed lines).

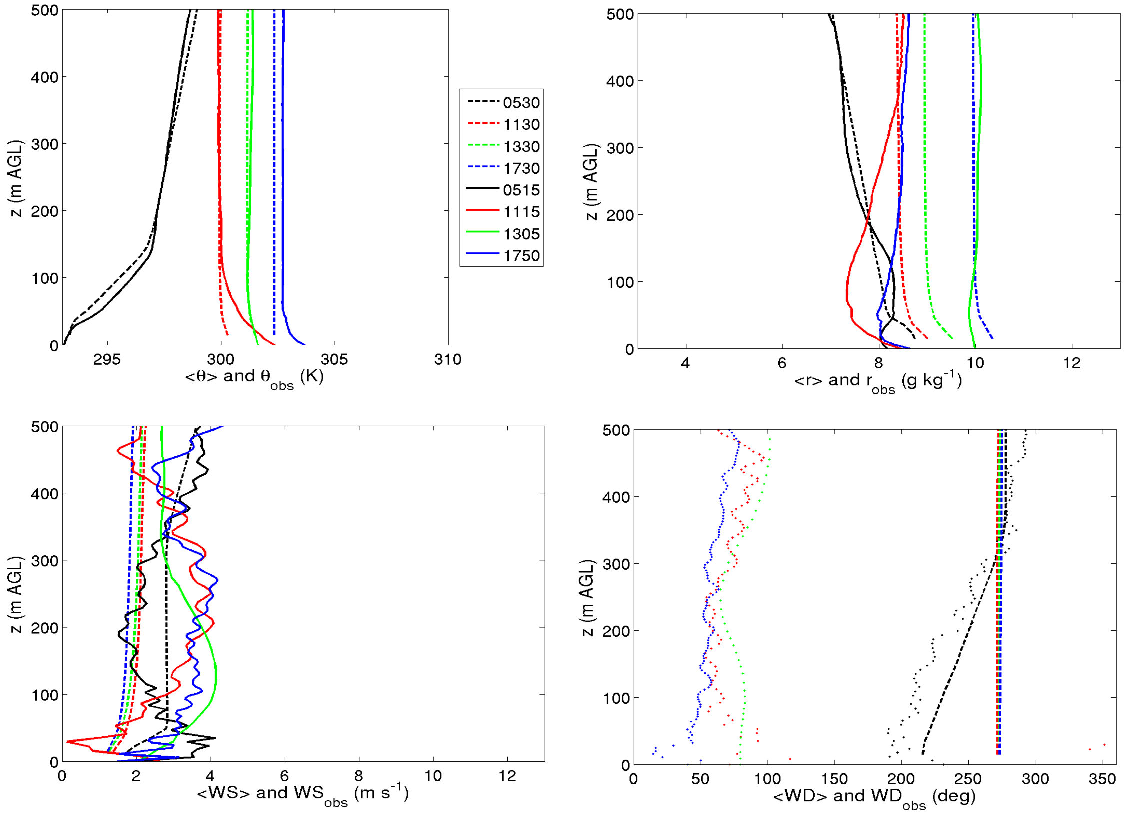
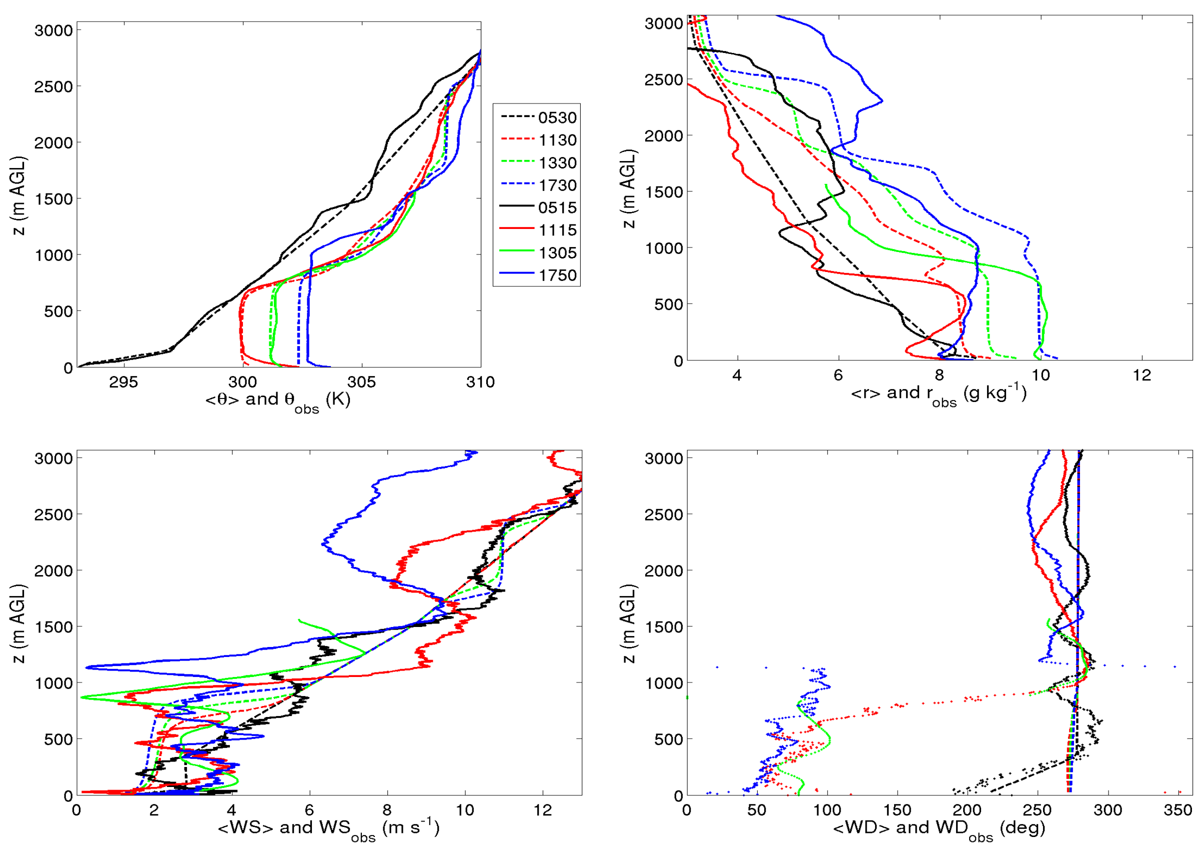


Figure 5: Un-zoomed view of the vertical profiles of θ, r, wind speed (WS) and wind direction (WD) observed (solid lines and dotted lines for WD) and obtained by LES (dashed lines).

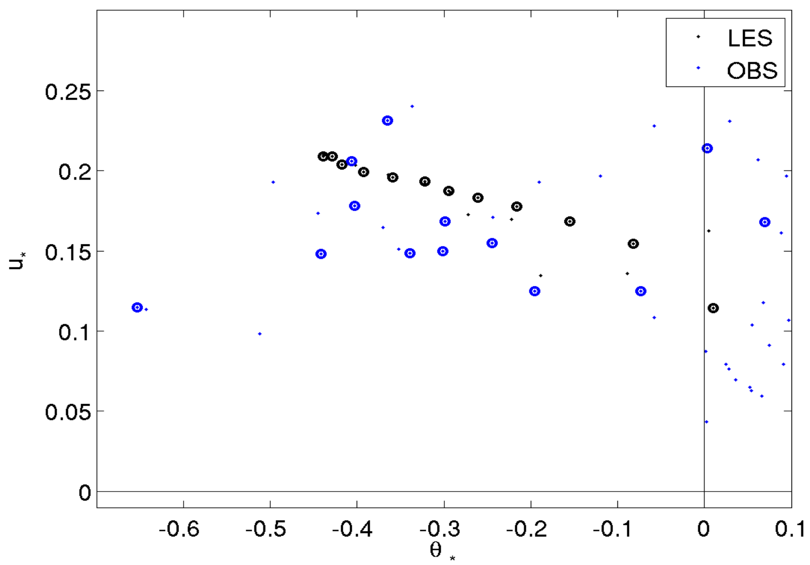


Pag. 32502 - Lines 19-25 – comments about zi

Discussion about zi and differences between LES and observations, as well as a proposition of explanation were already made in lines 26-31. So we did not understand the Reviewer's comment.

After 1800 UTC zi (RS-UHF) is almost 1100 m, while LES prediction is 800 m. Anyway, after 1800 UTC the surface heat flux should have reversed its sign and the PBL should be under stable conditions. Is it realistic ??? I think that authors should provide also a description of stability conditions, perhaps introducing the Richardson number and/or the MO length/velocity scales (L, ustar). These surface parameters are important because in the following it is introduced the concept of delay time between the surface and upper TKE. In these conditions it is important to verify that surface parameters are well reproduced by the simulation.

Figure 6: Comparison of thermal (θ_*) and dynamical (u_*) instabilities with observations (blue dots) and LES (dark dots) every 30 minutes. The afternoon period is represented with open circles symbols.



Due to the difficulty for the LES to run in stable conditions, we did not wish to study the LES results once the fluxes get to zero, or at least, one must take very cautiously those results and the comparison between LES and observation after that time (1800 UTC). Moreover, as soon as the surface fluxes turn negative, the LES potential temperature profile indeed turns immediately to stable at the lower levels, whereas a delay around ~ 30-80 min between the time when the buoyancy flux goes to zero and the time when the local gradient of virtual potential temperature indicates a sign change may be observed, as shown by Blay et al. (2014). The LES is not able to reproduce this delay and it was not the purpose of this study.

Our focus is on the evolution during the decrease of the positive flux.
We show in Figure 6 the comparison of the thermal and dynamical instability for both LES and observations during this period. u_* and θ_* are calculated from Monin Obukov theory in LES. The dynamical and thermal instabilities are very similar in the simulation and in the observations.
A comment about the similar dynamical and thermal instabilities in the simulation and observations has been added in section 3.2, l. 438:
“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 (not shown) near the surface. ”