We thank the reviewer for many interesting and useful questions and comments (C) which we respond (R) to below and also give our suggested modifications (M) to our manuscript when needed. Because of a significant restructuring of the paper we do not always provide all the inserted or modified text under each point here, but will include all necessary corrections in the revised manuscript.

Reviewer 2:

This work is a part II of boundary layer studies based on BLLAST field campaign. In the first paper they have studies the problem of the Turbulent Kinetic Energy budget during the afternoon transition. The BL description is based upon experimental profiles while the TKE budget is calculated from surface observations that are acquired during the BLLAST field experiment. Here, the authors have presented a simple TKE model for sheared/convective atmospheric conditions. TKE depends on four budget terms that are parameterized following "idealized mixed layer approximation and a simplified near-surface TKE budget". The principal goal is to study the TKE budget during the afternoon transition. However, I think that the paper may be improved in several aspects, so I recommend publication under major revision, as described below.

The legends of almost all figures are in general too long, please report only the description of the figure itself and put comments in the text.

R: We will adjust figure texts on some figures in response to this and other reviewer comments.

C1: Pag.29813, line 16 Where the "Plateau de Lannemezan" is located ????

R1 and M1: We will now include a better description of this in the revised manuscript.

C2: Pag.29816, line 19 I think that here authors should explain why they have used this approach (simple parameterization ….) and why not the classic first order closure (eddy diffusivity/viscosity) !!

R2 and M2: We add a couple of sentences about why we took this approach. This stems from us originally aiming only at a simple surface layer parameterization which still takes into account of simplified mixed-layer effects. Somehow this explanation was overlooked and missing from the manuscript and we feel it is important to add this idea when describing the goal of the model and potentially also in the revised abstract.

We agree that the eddy diffusivity concept can be another effective approach for single-column modeling although typical assumptions such as for instance relating transport of TKE to the gradient of TKE can be questionable as shown in Puhales et al. (2015). For the interpretation of final modeled results we consider the chosen approach as simpler than eddy-diffusivity closures and still effective enough to convey our overall results and conclusions.

C3: Pag. 29817 ch 3.2 What is the relations between B0 (eq.2) and w'T' in the definition of L.
R3: The buoyancy term in the TKE budget is: $B_0 = \frac{g}{\bar{\theta}}\overline{w'\theta_v'}_0$ and L in the simplified model is the Obukhov length scale $L = -\frac{\bar{\theta}u_*^3}{kg\,\overline{w'\theta v'}_0} = -\frac{u_*^3}{kB_0}$

C4: Pag. 29822 The transport term is usually modeled considering turbulent and pressure transport terms in the TKE budget (Stull, 1988). Here it is parameterized following a methodology (Mangia et al, 2000) that was applied for dispersion parameters in a Gaussian Model for tall stacks. So I think that a more complete description should be provided. Furthermore, there is a recent paper with LES that have considered this term in greater detail. I think that it should have been taken into consideration (Puhales et al., Physica A: Statistical Mechanics and its Applications 392.4 (2013): 583-595.)

R4 and M4: Ok, we suggest adding proper reference to the study by Puhales et al. (2013) where they show that a simple parameterization for transport terms can be made with second-order polynomial fits to LES data (either normalized with TKE or by w* scaling). The general shape of the profiles shown in Puhales up to zi for total transport is in qualitative agreement to our transport term with one negative layer in the lower part of the boundary layer and one positive layer in the upper part.

The fitted expressions in Puhales did not seem to integrate to exactly zero in a closer examination. This could potentially make their original expressions act as artificial sink or source terms in the TKE budget instead of just transport terms. The approach taken in Puhales is, however, very interesting and perhaps a closer fit to LES TKE budget terms is one way to improve models and understanding. Several additional fitting parameters may on the other hand make interpretation of the final model results more difficult to interpret at times and we choose to keep as few parameters as possible for the simplified mixed-layer that we assumed.

It is true that the total vertical transport is the sum of a pressure and turbulent transport which can often also be of different sign. We will clarify this more carefully in our revised manuscript. We have commented already upon our choice to keep some transport related to the near-surface shear produced turbulence and some related to the buoyancy production and that it is a simplification we do. The comments from reviewer 1 may suggest to shorten the description of this term and here it is somewhat unclear what is meant with a more complete description. Our suggestion is to include the clarification that vertical transport consists of both turbulent transport and pressure transport and that we model only the sum of these two and give reference to Puhales et al. (2013) which also show nicely that more advanced eddy diffusivity closures using the vertical gradient of TKE can be questionable in comparison to LES data.

C5: Pag.29822, lines 7-8 Please provide references of such "sheared convective large-eddy simulations".
R5 and M5: We used the large-eddy simulations of Darbieu et. al. (2015) and complementary simulations with changed wind forcing in comparison to the reference simulation which was part of the doctoral thesis of Clara Darbieu. The analysis of these simulations were to a large extent agreeing with earlier simulation results e.g Moeng and Sullivan (1994). The first-author also previously made some idealized simulations of CBL turbulence during his Phd work, with prescribed constant sensible heat flux and geostrophic wind (Nilsson et al. 2012). A set of flat terrain convective boundary layers with a fixed geostrophic wind of 5 m/s but different levels of fixed surface sensible heat flux (runs

ZC1-4 and ZN1 from Nilsson et. al.) was also used as inspiration for formulation of TKE budget terms. We will give the references in the revised manuscript.

C6: Pag.29825, lines 15-16 Please provide references "LES for this day did not show a pronounced maxima in dissipation rate".

R6 and M6: This was from the large eddy simulation by Darbieu et al. (2015) and we will add the reference. In response to one of the reviewer comments of Darbieu et al. (2015) the different TKE budget terms in the afternoon of June 20 is available in Figure 1, online at: http://www.atmos-chem-phys-discuss.net/14/C13233/2015/acpd-14-C13233-2015-supplement.pdf

C7: Pag. 29826 Ch. 3.3.5

The time evolution of TKE is calculated by a finite difference forward in time, with dt=1sec and dz=1m, in which the budget terms (S, B, T and D) are parameterized considering: "idealized linear profiles of buoyant production for a quasi-steady, horizontally homogeneous boundary layer following Lenschow et al. (1980)". So, we have a numerical model with a one second time step, but the budget terms on the rhs of eq.17 are steady-state ???? I wonder if it is not the case to put d(TKE)/dt=0 following Lenschow (1974). Please explain better this point !!

R7 and M7: Ok, First we will explain better that if the TKE tendency would be completely zero there could not be any evolution of the TKE, so there is a difference between true steady-state and quasi-steady conditions. We later show that as a decent approximation it is possible to put the tendency term to essentially zero and solve for TKE from our dissipation parameterization (at least very near the surface) so I understand your question. We will clarify that our model only contains a numerical finite difference time-stepping scheme, but in all other ways is just a simple parameterization. We will also clarify that what we meant was that our different budget terms is in general agreement to the proposed shapes (based on measurements) from Lenschow (1980) and the model of Lenschow (1974).

C8: Pag.29826 Colors in figure 7 are not easily distinguished

R8 and M8: We had a similar comment from reviewer 1 and will adjust the color scheme.

C9: Pag.29828 The middle row of figure 8 is unclear

R9 and M9: We think the reviewer is referring to that due to our choice of using fixed limits on the y-axis for all 9 days it can be difficult to distinguish between different colored lines for the wind gradient at different heights. Especially this will be so for days with weak wind, when there was essentially no significant wind gradient, and in fact most lines should naturally lie close to each other. We find it difficult to avoid, but will do our best to improve the figure (potentially removing the black lines corresponding to the middle level).

C10: Pag.29828, lines 18-20 "In this case, it is clear that wind gradients shift rapidly and the model captures some of the low frequency variability of the observations."

Change with: In this case, it is clear that wind gradients shift rapidly and the model, as a consequence of our simplifications, captures only some of the low frequency variability of the observations.

R10 and M10: Ok, we adjust the text.

C11: Pag.29830, lines 3-4 "This is probably mostly related to uncertainty in the way we define initial profiles of TKE for neutral morning conditions." I wonder if it is possible to start the numerical procedure with experimental neutral morning conditions.

R11: This was difficult as boundary layer depth estimates and other required data was not always available with the BLLAST field campaign more intensely focusing on the afternoon period. Instead we did comment that we have tried some sensitivity test for initialization and at midday and afternoon the results were not so sensitive. Please also see our responses to reviewer 1 and comment (C 2.8) about adding some more references and sentences related to the morning transition period.

C12: Pag.29831 – Chapter 5 This chapter is it strictly necessary ??? In its current form, the manuscript is rather long and hard to absorb. I think some rearranging would improve the clearness of the paper.
R12 and M12: Reviewer 1 has made some more specific suggestions for restructuring of the manuscript, please see our previous responses to those comments and suggested adjustments. We insist that the results of chapter 5 should be kept in the manuscript because the formation of weak turbulence first in the upper part of the boundary layer, also found in large-eddy simulations, is an important result of the study. It will be shortened slightly.

C13: Pag.29834 – Chapter 6 In this section are discussed three aspects that in my opinion are not very well correlated each other, so please give an exhaustive introduction.
R13 and M13: It will be restructured using several subsections following suggestions from reviewer 1 and we will also add an introduction to the section as suggested here to make the links more clear.

C14: Pag.29834 – lines 8-9 "The sensible heat flux used in these model runs are provided by a cosine function as in Sorbjan (1997) and several other earlier studies:"
Change with: The sensible heat flux used in these model runs are provided by a cosine function as in Sorbjan (1997) and several other earlier and subsequent studies:
R14 and M14: Ok, we will adjust the text.

C15: Pag.29834 – lines 15-16 Is there any reference ??? I meant for the zi modeled with a sine function.

R15 and M15: We also got a earlier comment from reviewer 1 and will now give more references about the morning transition. Due to that TKE remains relatively close to quasi-equilibrium the specific modelling choice for the morning may not affect the results very much in mid-day and afternoon.

C16: Pag.29835, line 19 Why table II and table III are splitted ???
R16 and M16: This was done because all the information would not fit into one table when using the journal style files and table environments provided. We suggest to keep it as is.

C17: Pag.29838, lines 12-14
"Whereas early LES studies (Nieuwstadt and Brost, 1986; Sorbjan, 1997) lead to decay exponents of 1.2 and 2, surface layer measurements (Nadeau et al., 2011) pointed out the existence of a range of exponents (e.g., 2 through at least 6). "
change with: "Whereas early LES studies (Nieuwstadt and Brost, 1986; Sorbjan, 1997) lead to decay exponents of 1.2 and 2, surface layer measurements (Nadeau et al., 2011) and recent LES (Rizza et al., 2013) pointed out the existence of a range of

exponents (e.g., 2 through at least 6). "
R17 and M17: Ok, we adjust the text.

C18: Pag.29838, lines "Therefore, and in the light of the above simulation results, which show both faster and slower than linear decay rates (and even increasing TKE for afternoons with increasing wind speed), we conclude that at heights near the surface there is unlikely any general simple decay exponent value for turbulence kinetic energy." I don't agree with this conclusion. There are some important aspects that merit to be mentioned. First at all the LES studies have concerned with the bulk averaged TKE (<TKE>), while Nadeau et al (2011) in his modeling just considered a single point in the surface layer but he pointed out the necessity that "LES simulations need to be run to confirm if this behaviour persists after averaging over the entire boundary-layer depth." Another point is that all the LES results have evidenced that the convective decay of turbulence starts slowly, then the influence of stable stratification causes a rapid collapse of <TKE> at the early evening transition" (Nadeau et al, 2011), this means that the "reality" reproduced with LES is a bit more complex than that described here with this simplified model.

R18 and M18: Firstly, Rizza et al. (2013) did confirm Nadeau results using bulk averaged TKE from LES. Secondly, the LES study from Darbieu et al. did consider the height variation of TKE (so this has been done using LES).

We fully agree that the reality is more complex than any type of single-column model or even an LES model and we should be specific about what our conclusion is actually saying. It is only a conclusion about the decay rate of TKE "at heights near the surface" and all results refer to only unstable (afternoon) conditions. We make no conclusions about the potentially faster decay rate in the evening transition. We will clarify the text to better reflect that this is the conclusions we make. From the comments of reviewer 1 we will in the revised manuscript have a subsection about the decay rate and in this subsection we will adjust the text to mention the aspects the reviewer is concerned with. We do insist, however, that it is important to consider the limitations of a simple decay exponent value for TKE. Pino et al. (2006) showed for bulk averaged TKE the effect of shear generation, and we showed with near-surface measurements (Part A) that with significant shear the TKE in the afternoon can even increase or stay more or less constant. When considering such situations we do insist that a simple time-dependent decay law is not very useful.