

Author response on paper: Turbulence Kinetic Energy budget during the afternoon transition – Part 2: A simple TKE model, by E. Nilsson et al.

Dear Editor,

On behalf of the coauthors of the manuscript and after the discussion stage of ACPD, I would like to submit a new version of this paper. We have taken into consideration the comments and recommendations from two anonymous reviewers and we have changed the manuscript accordingly. These referee comments and their responses and most significant modifications to the manuscript are attached to this letter.

After our initial submission the journal asked for some modifications before the typeset files were published. Because of these changes we have used the text from these tex-files but we were having trouble using the entire typeset latex files as is (with some of the new figure files asked for by the reviewers). We therefore used our initially submitted figure files in .eps format as well as modified new figures in .eps format. We also had some technical trouble with the modified reference list and therefore modified our separate bibliography file that we used at our initial submission. The restructuring of several sections and addition of some subsections asked for by the reviewers also made the use of a latex-diff between files difficult to implement as it then was unclear which files we should take the difference in between.

We thank both reviewers for their relevant comments, which helped us to improve the manuscript. Below is a copy of their comments with included answers to each point. References are made to the modifications in the revised manuscript. We have followed through on all the suggested modifications. We are most willing to work with the journal staff on the technical issue on how to best place the modified figures and references, and also on any other remaining issues if needed.

Yours sincerely,

Erik Nilsson

We thank the reviewer for many interesting and useful questions and comments (C) which we respond (R) to below and give our suggested modifications (M) to our manuscript when needed. Because of a significant restructuring of the paper we list the most significant modifications made to the manuscript below. In addition there may be a few minor wording changes made throughout the manuscript not included in this list of changes. None of which makes any change to results but helps to clarify the structure of the manuscript.

Reviewer 1:

“Using time series of the observed surface fluxes of sensible and latent heat, the near surface wind and the boundary layer depth, the authors construct a simple model of the TKE budget of the boundary layer, giving expressions for the profiles of each term in the budget as functions of the timeseries. The predictions of the simple model are compared with a wider selection of observational data and the model is then used to investigate how the rate of decay of TKE through the evening transition varies with different forcing scenarios.

Traditionally, transitional boundary layers have been comparatively neglected in the literature, so the current focus on these cases, involving field experiments such as BLLAST, is to be welcomed and this paper makes a useful contribution to this growing literature. However, I feel that the clarity of presentation could be improved in several respects and so I recommend publication subject to major revision.”

1 Major Comment

1. In its current form, the paper is quite long and hard to absorb. I think some restructuring and subdivision of the longer sections would improve the clarity of the presentation.

R: To this initial comment we agree that a restructuring of the manuscript can improve the clarity of the presentation.

M: This has now been implemented in accordance with the reviewer suggestions. The most significant changes include that we now introduce the model first in section “2 Model main goal and description” so that it becomes more clear what our purpose is when we introduce the various data sets and processing of the data in section “3 Observational data and processing”. We have also in the model subsection 2.4 gathered the comparison with the TKE budget model from Lenschow (1974) in a new subsection “Differences and similarities compared to the model from Lenschow (1974)”. In section 4 we now have two subsections, one about evaluation of TKE budget terms and one about evaluation of near-surface TKE. In section 6 “Parameter exploration for near-surface TKE” we now split the content in seven subsections “6.1 Setup of different scenarios”, “6.2 Results from varying the afternoon length”, “6.3 Results from varying the boundary layer depth”, “6.4 Results from varying the sensible heat flux”, “6.5 Results from varying the wind speed”, “6.6 Comment upon turbulence decay laws” and “6.7 A simple equilibrium model”. See further reviewer comments below about some more specific aspects of the introduced modifications.

C 1. (a) Since the various terms in the model are taken from the preceding paper (Part I), I suggest introducing the model first, before discussing the observational data. It will then be more obvious what features of the data are relevant.

R 1. (a): We agree, we have restructured the paper in accordance with the above response.

M 1. (a): We have after this restructuring of course changed the section numbering as well as some titles of sections and subsections. We have chosen to keep the data corresponding to June 20 and some data from the small tower (levels at 2 to 8 m) in section 2 when introducing the model, rather than placing it in a separate subsection. For the CD curve relationship in figure 1 and transport fraction in figure 5 this is essential as it is a part of the model description and placing it after section 3 (which now describes the data sets) would not be logical. Some rewrites were needed because of this restructuring since the more exhaustive description of data now comes later in section 3 in accordance with requests from the reviewers.

For instance on line 190-194 we now write " Observations for one BLLAST case (20 June) are shown as the model terms are introduced, even if the observations are described in more details in section 3. A more extensive evaluation of near-surface TKE budget terms is given in section 4."

Line 241-243: " In first instance, we will prescribe these, as determined in Part 1, from observations made at 3.23m from the ground (see later in the text for more details)."

Line 304-308: "In Fig. 1, the measured and modeled u^* values are plotted as a function of wind speed. Measurements shown here come from a set of IOP (Intensive Operation Periods) days during the BLLAST field experiment, at 3.23m and 8.22m above ground."

C 1. (b) Either at the end of the introduction or early in the section on the model, there should be a statement of what the inputs are (B , u and z_i) and what it is intended to predict (profiles of terms in the TKE equation): this will give the reader more sense of direction in reading the description of the model. The inputs to the model are indeed mentioned at the beginning of the abstract, but it is not clear what is meant by the TKE budget at that stage – a profile, a quantity integrated over the whole boundary layer, or something else – either way, repeating this just before introducing the model will do no harm. I would include the fact that the model is initialized at the morning transition, and I would also consider whether the formulae for the various terms of the budget could be presented more directly for readers who wish to refer back to the paper. Equation 16 is ideal, but the expressions for T could be presented more concisely. (See also below.)

R 1. (b): We agree. We suggest to include some more information in the first sentences in the beginning of the model section (which is now section 2). See also other suggested modifications in response to other comments.

M 1. (b): The rewritten sentences now read (lines 181-190): "In this section, we describe a simple model for the atmospheric boundary and surface layer turbulence kinetic energy. From inputs of time series of near-surface buoyancy flux, wind speed at one height in the surface layer and boundary layer depth estimates the model predicts vertical profiles of terms in the TKE budget equation as well as TKE. The model is initialized in the morning transition and gives an approximate description of the surface and boundary layer evolution in terms of TKE and its budget terms during unstable conditions until the end of the afternoon transition."

Please also note that in the abstract we write about the model on line 5-13:

" It is based on an idealized mixed layer approximation and a simplified near-surface TKE budget. In this model, the TKE is dependent on four budget terms (turbulent dissipation rate, buoyancy production, shear production and vertical transport of TKE) and only requires measurements of three available inputs (near-surface buoyancy flux, boundary layer depth and wind speed at one height

in the surface layer) to predict vertical profiles of TKE and TKE budget terms.”

C 1. (c) Section 6, and to some extent section 4, are very long and would be better divided into subsections. This will make it easier to locate information when referring back to earlier parts of the paper on rereading. In the case of section 4, a separate section for each term in the budget would be appropriate and in the case of section 6, there might be subsections for each scenario, then one on the rate of decay of TKE and finally one about the simple equilibrium model.

R and M 1. (c): Regarding section 4 we find it appropriate to split section 4 into two sections one about the evaluation of TKE budget terms (section 4.1) and one about the evaluation of near-surface TKE (section 4.2). A natural point to split between these two new subsections would be on page 29829, line 24: “We therefore find the modeled results of TKE at the 2.23 m level and 61.4 m level ...” where we also suggest to remove the word “therefore”. This would make the two new subsections of section 4 of about equal length. Any further subdivision of all the different budget terms in separate subsections would make most sections very short (just one paragraph long) and we therefore suggest not to do this.

Regarding section 6 we agree that it is appropriate to split into several subsections and have now done so in accordance with our first overall response about restructuring. Because several things are kept in common between different scenario we first have a subsection about “Setup of different scenarios” followed by short subsections about the different results for testing “Afternoon lengths”, “Boundary layer depth”, “Sensible heat flux”, “wind speed”. The last subsection contain all 3 different scenarios when wind speed forcing was varied. In this way a different topic or governing variable is addressed in each section. As suggested by the reviewer this is then followed by a subsection “Comment upon turbulence decay laws” and finally a subsection called “A simple equilibrium model”. This restructuring also includes taking into consideration some additional reviewer comments (see more specific responses below and to questions raised by reviewer 2).

C 1. (d) The comparison with Lenschow’s (1974) model could be integrated more effectively into the paper. There are sporadic references to this from section 3.3.1 onwards. It would be useful to discuss differences of formulation from Lenschow’s model at the end of the section on the model, where the forms of each term could be compared and contrasted. Figures A1 and A2 should show both Lenschow’s profiles and yours.

R and M 1. (d): Ok, we have added a subsection 2.4 (line 635-738) at the end of the model section in the revised manuscript, which gathers the discussion of the model from Lenschow (1974) in one place. We also added our modelled profiles in Figures A1 and A2 by using the model runs for June 2 which had times with the same overall stratification in terms of z_i/L . By gathering the “sporadic references” in one place and reducing the text in the Appendix the new addition of this subsection does not lengthen the manuscript as a whole. Because we added our modeled vertical profiles to Figures A1 and A2 we also added some text to explain better some of the differences. Most significant new introduced text with regard to this is on lines 642-648:

”In these figures we have also included vertical profiles from our model for July 2 from times when the same strength of overall stratification in terms of z_i/L of -1000 and -1 occurred. It should be said that z_i was 980 m and 950 m respectively for the 2 July cases and thus differ only 5% from the 1000 m used for the model from Lenschow (1974)”.

Also on lines 695-719 when discussing the differences in dissipation in very near-neutral) (end of afternoon conditions some further explanation was included:

"From Fig.A2 it is clear that the overall dissipation in our model can appear large in comparison to the model of Lenschow (1974) at the end of the afternoon. Our modeled profile is, however, taken from the very last minute before buoyancy flux becomes zero, when all surface forcings are quite small. The dissipation term in mid-boundary layer is in fact only about $-2.6 \cdot 10^{-5} \text{ m}^2 \text{ s}^{-3}$ at this point, but in a relative sense it is larger than in the model from Lenschow (1974). The reason for this larger dissipation term in our model in the overall boundary layer is because we linked our dissipation to the level of TKE. This introduces a slight memory effect of conditions that happened earlier in the simulation that is not present in the model from Lenschow (1974). It is worth to note again that TKE tendency is assumed to be exactly zero in Lenschow (1974) and no prediction of TKE is therefore provided by this model. We included our TKE tendency term as blue lines in Fig.A1(b) and Figs.A2(a) and (b). In convective conditions it is clear that the tendency term is very small compared to the other forcings. For our very close to neutral case at the end of the afternoon the TKE tendency is also small in actual units ($-2.5 \cdot 10^{-5} \text{ m}^2 \text{ s}^{-3}$ at $0.5z_i$) but is not a negligible term in comparison to other budget terms in mid-boundary layer. There it will be mostly determined by the available TKE that influences our modeled dissipation term."

2 Minor Comments

C 2. 1. p29809 L5. Is this the best reference? Emanuel himself refers back to Stull (1988) in his discussion of the mixed layer.

R 2.1: Ok we use the standard text book reference to Stull instead in our revised manuscript.

M 2.1: The changed sentence now reads (line 47-49) "These large eddies are generated by a strong surface heat flux but are also influenced by wind shear (Stull 1988)."

C 2. 2. p29810 L10. Please insert one sentence on the aims of BLLAST.

R 2.2 and M 2.2: We have included in the introduction the aims most relevant for this study and also reference the overview paper (Lothon et al. 2014) and present BLLAST as the context of our study. We feel we may have missed some of this information here in Part B, being the second part of a two-part paper, and have tried to make sure our revised manuscript is more autonomous with regards to this. Line 91-98 now reads: " These studies did not, however, specifically address the representation of the evolution of TKE. Special attention was paid to the evolution of the turbulent kinetic energy during the Boundary Layer Late Afternoon and Sunset (BLLAST) field campaign. TKE was sampled by a combination of independent instruments and this was a specificity and one of the aims of the field campaign (Lothon et al., 2014; Couvreux et al. 2016)."

C 2. 3. p29818 L9. This is just a standard iterative approach to obtaining u^* . Would it not suffice simply to say that u^* is obtained iteratively from u and B ? Figure 1 could then be deleted.

R.2.3: We consider it could be useful to include this information on the iterative procedure because what can be considered "standard" may differ between different research groups. Figure 1 also shows the CD curve relationship which was specified from measurements and the sometimes large impact the iteration have on the u^* values that will actually be used in the model during runtime.

Because the shear production term in the TKE budget was found to be one of the most difficult to model we suggest to keep Figure 1 and our description of the iteration procedure used.

C 2. 4. p29820 L20. Please state the actual number of the equation.

R 2.4: Ok, we now give a more specific reference for Part 1, section 2.2.5 which only includes one equation (but no equation number).

M 2.4: The sentence now reads (line 352-254): "It is, however, important to remember when interpreting these results that transport is calculated as a residual from other budget terms as described in section 2.2.5 of Part 1."

C 2. 5. Section 3.3.3. If I have understood the model for T_b correctly, $Z_{i0}/Z_i = \text{square root of } 2$. It would be useful to state this. An immediate consequence is that the depth of the entrainment zone is 40% of that of the mixed layer. Similarly, $T_{b_{max}}$ is completely determined by $B_0(t)$.

R 2.5: Yes, thank you for pointing this aspect out. Due to the symmetric assumption the ratio of z_{i0}/z_i is constant and equal to square root of 2 (except for a small difference that was caused by using fixed height levels and always using the nearest model level as z_{i0}). We will adjust the text to point this out.

The depth of the entrainment zone (if we count the layer with negative buoyancy production term) is in accordance with Figure 2 going to be dependent on the specified value of the entrainment parameter because it also reaches below z_i , but if we consider the layer depth normalized with z_{i0} it becomes about 38.5% with our choice of -0.15 as entrainment parameter.

For the value of $T_{b_{max}}$ it will depend on $B_0(t)$ and the specified near surface transport fraction T_f . The relationship becomes roughly $T_{b_{max}} = (0.4/\sqrt{2}) * B_0$ which corresponds to approximately 28% of the near surface buoyancy production value.

M 2.5: We have added some sentences to the revised manuscript to describe these approximate model relationships (Line 492-508):

The profile of total transport is shown in Fig.4. A couple of things can be pointed out about the modeled profile. Due to the symmetric assumptions the ratio of Z_{i0}/Z_i is constant and equal to $\sqrt{2}$ except for a minor adjustment related to using fixed height levels in the model. The depth of the entrainment zone is also constant and about 38.5% of the boundary layer depth (Z_{i0}) for our choice of an entrainment parameter of -0.15 taken from LES simulation (Darbieu et al., 2015b).

The value of $T_{b_{max}}$ will depend on $B_0(t)$ and the specified near surface transport fraction T_f which, however, can be approximated as about 0.4. The relationship for $T_{b_{max}}$ becomes roughly $T_{b_{max}} = (0.4/\sqrt{2})B_0$ which corresponds to approximately 28% of the near surface buoyancy production value. This value will also dominate the maximum value in the total transport profile because the transport due to shear production is generally much smaller (at least for the wind speeds encountered during the BLLAST field campaign).

C 2. 6. p28923 L22. I tend to feel that this is excessively elaborate, given the scatter in your data and the apparently systematic difference between the morning and afternoon data, and that a constant value of about 0.4 would be equally good. In fact you yourselves make just this approximation in section 6. The only real justification for the more elaborate expression seems to be that it is derived from terms from Part I.

R 2.6 and M 2.6: We felt it would be good to point out where this comes from so that our definition of T_f is clearly defined from the normalized budget terms somewhere in the paper. We agree however that it will make only a small change for the modeled results if it is replaced by 0.4. To assume this feels however as a secondary step to us. If we would jump directly to this we would not give proper reference to where the 0.4 is coming from. We also think it is useful to present the observed difference in data between afternoon and morning period and would suggest to keep Figure 5 and the associated discussion but we will add a short comment that reflects that we are aware that T_f could be replaced by 0.4. This will also make it more clear later in the manuscript text why we are doing so for the simple equilibrium model. We now write on lines 482-484: Changing the near-surface transport fraction value to a constant of about 0.4 would also be possible and will only affect our simulation results slightly.

C 2. 7. Sections 3.3.5 and 3.3.6. Should these not be swapped? You need initial conditions before you can determine the evolution of the TKE.

R and M 2.7: Agreed the sections have been swapped with only minor modifications to the text. See later reviewer comments for the introduced changes and added references about morning conditions.

C 2. 8. Section 3.3.6. The initial condition requires more justification. During the morning transition (eg. Angevine, Baltink Bosveld (2001) 'Observations of the morning transition of the convective boundary layer', BLM, 101, pp. 209–227) a shallow mixed layer develops within the stable boundary layer and deepens rapidly when its potential temperature attains that of the residual layer. At that point the buoyancy flux is not 0. Since you later take the initial value of z_i to be 150 m you probably intend to start the model from just before this point. That said, since, as you subsequently show, the TKE remains close to its quasi-equilibrium value, the initial conditions may not matter too much.

R and M 2.8 We understand your point of view and suggest to add some references to this work on morning transitions and comment that we make our choices of initial conditions as a crude approximation to a more complex reality. We did comment in the last sentence of section 3.3.6 that model tests of changed initial conditions showed small differences in results for the evolution of modeled TKE at midday and afternoon. We suggest to clarify in the revised manuscript text that this is very reasonable if TKE remains near quasi-equilibrium and also because there are many hours from our typical starting point around 0500 until midday. We now have included in the manuscript on lines 565-577:

“Our choices for initial conditions and modeling of morning transitions should be recognized only as a very crude attempt to represent a much more complex reality. Angevine et al. (2001) for instance showed that during the morning transition a shallow mixed layer develops within the stable boundary layer and deepens rapidly when its potential temperature attains that of the residual layer. This indicates much more

complexity for the growing phase of turbulence than what we assume here in our simple modeling. Initial conditions can, however, have limited influence on our results in midday and for afternoons if TKE remains close to its quasi-equilibrium value and also because there are many hours from our typical starting point around 05 UTC until midday.”

C 2. 9. p29828 L21. The implication here is that the gradient is the main source of error in predicting the TKE. Perhaps it’s worth actually saying this. In one way, that’s a bit surprising since u^* is derived from the gradient.

R 2.9: We would argue both the wind gradient and u^* cause errors in the model. We also noted that our observed u^* value could not always be considered as height constant as often shown at more homogenous sites, but we nevertheless used this as a first-order approximation when determining the CD-curve presented along with measurements in Figure 1.

M 2.9: We suggest adding a sentence discussing these effects. Suggested sentence introduced in the manuscript on lines 889-892 “The too rapidly decaying shear production term with increasing height in comparison to measurements stems from both deviations in the assumed wind gradient and height dependence of frictional stress.”

C 2. 10. p29828 L24. Because the source of TKE depends on u_*^3 , it is likely that missing periods of high wind speeds will systematically underestimate the generation of TKE. The importance of these excursions will depend on how quickly departures from quasi-equilibrium are damped.

R 2.10 Thank you for this very useful comment, we agree. We suggest adding two sentences about this in the manuscript.

M 2.10 Suggested sentences introduced on lines 904-909: “Underestimation of the generation of TKE from missing periods of high wind speed is natural because the source of TKE depends on u_*^3 . The importance of these excursions will, however, also depend on how quickly departures from quasi-equilibrium are damped.”

C 2. 11. p29838 L25. The success of the simplification of ignoring the time dependence is interesting and possibly worth stating more prominently (abstract/conclusions). Although, on the other hand, it’s worth bearing in mind that by using prescribed functions of z for each term, you force all levels of the boundary layer to respond together. It’s possible that this may underestimate the role of time-dependence in the real boundary layer.

R 2.11, M 2.11: We agree and this is one of the the reasons we did not emphasize this result more prominently. Our suspicion based upon comparison of TKE at 61 m (and also 30 and 45) is that at increasing height additional excluded processes such as horizontal advection may also play a bigger role. So if we make a statement more prominent about the success of this quasi-equilibrium assumption we consider that we need to restrict it to the very near surface layer. We have added a sentence in the revised manuscript about this aspect of our results on line 1356-1362: This success of simplification and ignoring the time dependence for the very near-surface TKE is interesting, but we should bear in mind that by using prescribed functions of z for each term. We thereby force

all levels of the boundary layer to respond together. It is possible that we therefore underestimate the role of time-dependence in the real boundary layer.

C 2. 12. p29839 L5. This equation seems to predict rather low values of TKE near the surface in very convective conditions. E appears to scale on $z^{2/3}$, which is what would be expected for the variance of vertical velocity, but the variances of the horizontal velocities show little variation with height in the mixed layer. Have you compared the profile of TKE with published results, such as Caughey and Palmer (1979), "Some aspects of turbulence structure through the depth of the convective boundary layer", QJRMS, vol. 105, pp. 811-827?

R 2.12: Thank you for this suggestion. Let us for simplicity assume that we have convective conditions with zero u^* for which our expression would give minimal TKE. Our expression would then reduce to $E = w_*^2 \left(\frac{0.6l\epsilon}{z_i} \right)^{2/3}$ Boundary layer depth was variable in Caughey and Palmer but lets assume 1000 m which is their value at midday. For heights between 1 and 100 m ($0.1z_i$) this would mean that E would be between $0.175w_*^2$ and $0.413w_*^2$.

Let us compare this to using some expressions that Caughey and Palmer (1979) use to discuss their data. For vertical wind variance and $z < 0.1z_i$ they discuss their data in comparison to $w_*^2 1.8(z/z_i)^{2/3}$ which means σ_w^2 would be between $0.018 w_*^2$ and $0.3878 w_*^2$ for the same height interval.

For horizontal wind variances $\sigma_{u,v}^2$ the Aschurch and Minnesota average value for mixed layer would be about $0.4 w_*^2$ and they also compared to Panofsky et al. (1977) based on surface layer data recasted to similarity form which would give $0.35w_*^2$

Forming TKE from these numbers would give values of TKE between $0.359 w_*^2$ and $0.544 w_*^2$.

Ok, we agree this is higher than our range of $0.175 w_*^2$ and $0.413 w_*^2$, but not necessarily so much higher. Also in many convective cases there would be some, but perhaps low, wind speed which would increase our modeled TKE.

Caughey and Palmer discusses, and we agree with them, that the level of variance also depends on how much low-frequency scales are included. They state that their values for longitudinal and horizontal wind variances consist of significant variance at scales in excess of $2z_i$ and mention this as one possible reason for why their horizontal wind variances are high in comparison to Willis and Deardorff (1974) tank experiments. They also discuss a more practical issue that longitudinal and lateral components of air motion are on occasion significantly contaminated by balloon motion effects. They discuss that these effects could produce over-estimates of the (horizontal) variances by 15-30% depending on the separation between the balloon and turbulence probe in their study.

M 2.12: We added two sentences to our revised manuscript on lines 1363-1370: "For a convective boundary layer with little shear production our expression reduces to $E = w_*^2 \left(\frac{0.6l\epsilon}{z_i} \right)^{2/3}$ which gives a relatively low TKE in the surface layer of about $0.175w_*^2$ and $0.413w_*^2$, assuming a 1000 m boundary layer depth. This is low in comparison to earlier studies, e.g Caughey and Palmer (1979) gives expressions for about $0.359w_*^2$ and $0.544w_*^2$. Our model can, however, also give some higher TKE when shear production is present."

C 2. 13. p29839 L10. A dependence of the TKE on z_i in the CBL is not surprising, since it must scale on w_*^2 and so on $z_i^{2/3}$. I take it that you mean specifically the first term in Eq. 40.

R 2.13: This comment we made relates to that often in the surface layer, variances (especially vertical wind variances) would be argued to be well-described by only Monin-Obukhov similarity theory and hence once placed on a non-dimensional form using u_* and z they would only become a function of z/L . But we agree with you that it makes sense that it also becomes a function of z_i in a convective

boundary layer. Both terms in equation 20 has a dependence on z_i because the dissipation length scale has a dependence on z_i . The first term would otherwise be possible to recast into a form being purely dependent on z/L .

C 2. 14. Figures. Red and magenta can appear very similar when printed. Consider using green for data at 14.30 UTC in figures 2–4 etc.

R 2.14 and M 2.14: Ok, we have adjusted the color of the figures

C 2. 15. Figure 3. The data for 14.30 UTC are not well represented by the fit. Do you have any comment?

R 2.15: We discussed in the text about the too rapidly decaying shear production term with increasing height but in addition to this we agree that there is too high shear production due to a poor wind gradient near the surface around 14.30 UTC. This is also seen for June 20 in Figure 8 with the blue line (representing about 3 m) being higher than the measurements both for the wind gradient (middle row) and as a consequence also shear production (lower row).

C 2. 16. Figure 5. A minus sign is required before z/L throughout the caption.

R and M 2.16: Yes, you are correct we have corrected it.

C 2. 17. Figure 8. The last sentence of the caption is interpretation and belongs in the text.

R 2.17, M 2.17: Yes we have removed it from the caption.

3 Typographical Comments

C 3. 1. p29809 L10. Delete "the" before "atmospheric".

R and M 3.1: Corrected

C 3. 2. p29810 L 5. "slower decay of the TKE"?

R and M 3.2: Corrected

C 3. 3. p29821 L20. "is decaying" → "decays".

R and M 3.3: Corrected

C 3. 4. p29823 L 6. Perhaps f_T would be preferable to T_f notationally.

R 3.4: We are unsure about what the misinterpretation could be if we keep this notation. We suggest keeping it.

C 3. 5. p29823 L 6. "solved for" → "obtained"

R and M 3.5: Corrected

C 3. 6. p29823 L 15. "spreads the transport" is a poor phrase. The TKE is spread. It would be better to say that TKE is transported from the surface layer to the upper part of the boundary layer.

R and M 3.6: Corrected

C 3. 7. p29824 L 16. "compare" → "compares"
R and M 3.7: Corrected

C 3. 8. p29824 L 20. "on" → "at"
R and M 3.8: Corrected

C 3. 9. p29824 L 21. "differ" → "differs"
R and M 3.9: Corrected

C 3. 10. p29825 L 16. "maxima" → "maximum"
R and M 3.10: Corrected

C 3. 11. p29826 L 18. "has" → "have"
R and M 3.11: Corrected

C 3. 12. p29828 L 18. "more smooth" → "smoother"
R and M 3.12: Corrected

C 3. 13. p29832 L 15. Do you mean capped in value or in height?
R and M 3.13: We mean capped in value and have clarified this in the revised text. Lines 1044-1046 now read: We note that the model may overestimate boundary layer dissipation somewhat for 30 June and turbulence may not be as capped in value in the model as indicated from UHF profiler.

C 3. 14. p29840 L 5. You "predict"; "days" do not.
R and M 3.14: Corrected

C 3. 15. p29841 L26. "supports" → "suggests".
R and M 3.15: Corrected

C 3. 16. Figure 12, caption. "shown legends applies" → "legends shown apply".
R and M 3.16: Corrected

C 3. 17. Figure 13, caption. "is showing" → "shows".
R and M 3.17: Corrected

We thank the reviewer for many interesting and useful questions and comments (C) which we respond (R) to below and give our suggested modifications (M) to our manuscript when needed. Because of a significant restructuring of the paper we list the most significant modifications made to the manuscript below. In addition there may be a few minor wording changes made throughout the manuscript not included in this list of changes. None of which makes any change to results but only helps to clarify the structure of the manuscript.

Reviewer 2:

This work is a part II of boundary layer studies based on BLLAST field campaign. In the first paper they have studied 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 and M: We have adjusted figure texts on some figures in response to this and other reviewer comments. On some figures we would agree that it may still appear a bit long but removing more information from the figure texts at this stage would in our opinion mean removing information that helps explain the figures. We are fully prepared to work further on textual changes if needed.

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

R1 and M1: We have now included a better description of this in the revised manuscript. Lines 729-737 now reads: “ The BLLAST field campaign took place in June and July of 2011 in southern France at Plateau de Lannemezan, a plateau of about 200 km² area, nearby the Pyrénées foothills, at equal distance from the Mediterranean sea and from the Atlantic ocean (about 200 km). The surface is covered by heterogeneous vegetation: grasslands, meadows, crops, forest. Several measurement sites were placed in the study area to obtain information of surface fluxes and winds from this heterogeneous landscape (Lothon et al., 2014).”

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. 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. On lines 223-230 we now included:

“Our choice of using a simple parameterization of budget terms instead of first order closure stems from us originally aiming at a simple surface layer parameterization, which still takes into account of simplified mixed layer effects. 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. (2013).”

C3: Pag. 29817 ch 3.2 What is the relations between B_0 (eq.2) and $w'T'$ in the definition of L .

R3: The buoyancy term in the TKE budget is: $B_0 = \frac{g}{\theta} \overline{w'\theta_v'}$ and L in the simplified model is the Obukhov length scale $L = -\frac{\overline{\theta} u_*^3}{k g \overline{w'\theta_v'}}$ = $-\frac{u_*^3}{k B_0}$

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 z_i 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 now 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. In our revised manuscript the main introduced changes are on lines 397-407 which now reads:

Figure 4 shows the modeled and observed transport term of the TKE budget. The total vertical transport consists of both pressure transport and turbulent transport (Stull, 1988), which often can be of different sign (Moeng and Sullivan, 1994). Here we will model only the sum of these two terms related to the available buoyancy and shear production of

TKE. This is of course only an approximation of a more complex reality but it is shown in Puhales et al. (2013) that also 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 now give the references in the revised manuscript to Darbieu et al. (2015) and Moeng et al. (1994). Because we did not wish to confuse the readers with additional complexity studied in Nilsson et al. (2012) with some simulations (FN1, FN2, FC1-FC4) including effects from surface gravity waves we chose not to include this reference in the 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>
We have now also provided this reference in the manuscript.

C7: Pag. 29826 Ch. 3.3.5

The time evolution of TKE is calculated by a finite difference forward in time, with $dt=1\text{sec}$ and $dz=1\text{m}$, 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 $d(\text{TKE})/dt=0$ 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).

In our revised manuscript lines 579-593 now reads:

”The calculation of TKE tendency is essential for the time evolution of TKE. It was shown in Part 1 that TKE tendency is typically much smaller than the other budget terms, but if it 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.

In our model the evolution of TKE is determined by a finite difference (forward in time) calculation with 1 s time step and 1m vertical resolution from the other budget terms using the TKE budget equation.

The model can be considered semi-analytical in the sense that it contains only a numerical finite difference time-stepping scheme, but in all other ways is just a simple parameterization.”

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

R8 and M8: We had a similar comment from reviewer 1 and have now adjusted 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 have now improved the figure by choosing to adjust the range of the y-axis a little bit. This makes a couple of 10-min values with deviating wind gradients lie outside the shown range but improves the readability of the figure.

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 have adjusted 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 has been slightly shortened in for instance response to multiple given information in figure text of Figure 11 (removed sentence: “ The strong observed fluxes over forest and wheat lead to stronger TKE and dissipation rate in the simulated boundary layer, whereas the weaker fluxes over moor, corn, grass and the Divergence site yield levels of dissipation rate closer to the observed”)

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 is now restructured in several subsections following suggestions from reviewer 1 and we also added an introduction to the section as suggested here to make the links more clear. On lines 1105-1115 we now write:

”In this section, we first discuss the setup and results of modeled near-surface TKE for the afternoon based upon complementary idealized numerical simulations in sections 6.1-6.5. Secondly, we comment upon our results in relationship to turbulence decay laws in section 6.6. We also compare our numerical model results to a simplified analytical expression assuming quasi-stationary turbulence in section 6.7. Here, we also illustrate and discuss the added value of our modeling efforts taking into account variations in wind or u^* compared to only taking into account of w^* as a scaling variable for TKE.”

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 have adjusted the text.

C15: Pag.29834 – lines 15-16 Is there any reference ??? I meant for the z_i 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 as also previously included in the revised manuscript in relationship to other reviewer comments. On line 1130-1135 we now write:” This prescribed evolution is of course a simplification, as discussed in section 2.3.5 the morning transition can have much complexity as shown in Angevine et al. (2001). Our results for midday and afternoon was however relatively insensitive to this modeling choice.”

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 have adjusted 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 ($\langle TKE \rangle$), 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 $\langle TKE \rangle$ 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. (2015) 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 have adjusted 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.

On lines 1307-1322 we now write:

”All results presented here concerns the TKE decay near the surface during the afternoon transition with still unstable conditions. Nadeau et al. (2011) pointed out the necessity for LES simulations to confirm if their observed surface layer results persists after averaging over the boundary layer depth. This was done in Rizza et al. (2013) and in addition Darbieu et al. (2015b) studied the height variation of TKE using LES and measurements. Our conclusions about the limitations of simple decay exponent values for near-surface TKE could similarly be tested with LES. Pino et al. (2006) showed for bulk-averaged TKE that shear-generation can give reduced TKE decay and we showed with near-surface measurements (Part 1) that with significant shear the TKE in the afternoon can even increase or stay more or less constant. These types of situations emphasize the limitations of simple exponent decay laws.”