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 do not always give all the inserted or modified text under each point here, but will include all necessary corrections in the revised 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.

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 will restructure the paper.

M 1. (a): We will now place much of the content of the previous section 3 ”Model description and evaluation for the afternoon of 20 June 2011” before section 2 ”Data and methods” and change the section numbering as well as titles. We will also move the discussion of June 20 into a separate subsection placed after the introduction of the model and data and methods where we reference back to some of the figures already used to describe the model. Some rewrites will with this new structure of the paper also be needed. These are too many to all be mentioned here in our suggested modifications, please see our revised manuscript for marked up changes. The new titles of the three parts are suggested to be ”Model main goal and description”, ”Observational data and process” and ”Evaluation on June 20”.

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). Se also other suggested modifications in response to other comments.

M 1. (b): The rewritten sentences now read: ”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.”

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 and one about the evaluation of near-surface TKE. 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 subsubsections 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. Because several things are kept in common between different scenario we propose first to 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”, ”Constant wind speed”, ”Increasing wind speed” and ”Decreasing wind speed”. The three last subsections could be placed together such that 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 suggest adding a subsection at the end of the new model section in the revised manuscript which gathers the discussion of the model from Lenschow (1974) in one place. We also add an illustration of our modelled profiles in Figures A1 and A2.

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 "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 will include one or two sentences as suggested in our revised manuscript covering some of 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 will make sure our revised manuscript is more autonomous with regards to this.

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: 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 = 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_{bmax} 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 zi0/zi 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 zi0). 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 zi, but if we consider the layer depth normalized with zi0 it becomes about 38.5% with our choice of -0.15 as entrainment parameter.

For the value of Tbmax it will depend on B0(t) and the specified near surface transport fraction Tf. The relationship becomes roughly Tbmax=(0.4/sqrt 2)*B(0) which corresponds to approximately 28% of the near surface buoyancy production value.

M 2.5: We will add some sentences to the revised manuscript to describe these approximate model relationships.

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.

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 can be swapped with only minor modifications to the text.

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.

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 "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: "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 may add a sentence in the revised manuscript about this aspect of our results.

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 $E = w_*^2 \left(\frac{0.6 l_\epsilon}{z_i}\right)^{2/3}$ Boundary layer depth was variable in Caughy and Palmer but lets assume 1000 m which is their value at midday. For heights between 1 and 100 m (0.1zi) this would mean that E would be between $0.175 w_*^2$ and $0.413 w_*^2$.
Let us compare this to using some expressions that Caughy and Palmer (1979) use to discuss their data. For vertical wind variance and z<0.1zi they discuss their data in comparison to $w_*^2 1.8(z/zi)^{2/3}$ 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 $0.35 w_*^2$
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.
Caughy 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 sugggest to add two sentence to our revised manuscript: "For a CBL with little shear production our expression reduces to $E = w_*^2 \left(\frac{0.6 l_\epsilon}{z_i}\right)^{2/3}$ which gives a relatively low TKE in the surface layer of about $0.175 w_*^2$ and $0.413 w_*^2$, 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 $0.359 w_*^2$ and $0.544 w_*^2$. 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 $zi^{2/3}$. 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: Ok, we adjust 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 will correct 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 will remove it from the caption. We will incorporate it in the text which partly, however, already have similar information. So we will make sure that we do not just reiterate the same information.

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 will clarify this in the revised text.

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