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.

Reviewer 2:

C1: *"The paper describes the TKE budget during the afternoon. The authors emphasize the transitional nature of the afternoon boundary layer. The main results in the paper are about the TKE budget in the surface layer. The analysis doesn't appear to reveal features that reflect non-stationarity. The other significant result is the dependence of the dissipation rate on the depth of the boundary layer."*

R1: To this initial comment we agree with the reviewer but want to make a clarification regarding the sentence that: *"The analysis doesn't appear to reveal features that reflect non-stationarity.'* This is partly due to our choice of presenting the mean hourly behavior of the turbulence kinetic energy budget and that there may be more effects of non-stationarity that can be explored at shorter time scales. However we consider that it is very important as a first step to describe the mean evolution of the studied turbulence parameters as they reflect the most probable evolution that is linked to the slow changes that occur in mean wind and radiation conditions.

C2: *"The results are interesting and worth publishing. However, I would like to see the analysis of the TKE budget extended to include more of the stable data. This may move the paper away from the afternoon transition but should help in determining the neutral values of shear production and dissipation. As part of this more detail of the local area is needed since Fig 1 suggests that it is not simple with trees and buildings in the immediate area of the masts."*

R2: We propose to keep the objectives on the afternoon transition. We will make this even more clear in the text. We did one exception from this in the manuscript and included slightly stable conditions in Figure 9, and discussed that it may be interpreted to imply that different normalized shear production in neutral conditions can occur. The consensus value of 1.0 was then observed. Please also see our response to the comment regarding our results in comparison to Högström (1990).

M2: On page 29749 line 6-7 we wrote: "The afternoon transition period can be defined as the period from mid-day maximum heat flux until zero buoyancy flux (Nadeau et al., 2011)."
We add the following sentence directly after this: "In this paper we use this definition and focus our study on the afternoon transition period." Please also see our responses to other comments that are on the same 'theme'.

C3: *"In the analysis of the dissipation rate profiles the authors use averages over the whole afternoon. This implies there are large changes in stability as the buoyancy flux decreases, and changes in the shear production which may not be directly related to the diurnal cycle. I'd suggest that the authors check that they can reproduce the result using a shorter averaging period, one that is restricted to periods when the buoyancy flux is large."*

R3: Please note that we only used the whole afternoon averaged dissipation profiles in Figure 10 and 11 to explore and illustrate the observed dependence on distance above the surface and boundary layer depth. In the final test presented in Figure 12 (b) we do use the shorter averaging time scale of 1 hour to be consistent with the previous analysis and make comparison to the investigated dissipation dependence on Obukhov length.

M3: On page 29770 line 6 we wrote: "In Fig. 10, dissipation is shown as a function of $E^{3/2}/z$ averaged for the afternoon." We suggest to add the following sentences to clarify: "Here we first carry out an investigation of the dissipation dependence on measurement height and boundary layer depth using data averaged for full afternoons. Then later we also test our findings using data with a shorter averaging time of 1 hour to be consistent with our hourly TKE budget analysis."

C4: *"I didn't find section 4.1 particularly useful. It seems to just say that the time variations are a bit complicated and that it is necessary to account for shear production and buoyancy production. This is done using surface layer similarity in the next section. The important question for this paper is whether the similarity theory provides a reasonable description of the data, or whether there are problems that appear to be related to the non-stationarity during the afternoon transition. Section 4.1 doesn't help answer these questions."*

R4: We respectfully disagree that section 4.1 does not provide much useful information. It includes a discussion and classification of the different afternoons based on the calculated TKE budget terms. In this way we present quantitative information contrasting the different afternoons in actual real units of m^2s^{-3} that can be very useful also for evaluation of models in future work as proposed in a recently submitted manuscript evaluating NWP models using BLLAST data (Couvreux et al. 2015).

Discussion and presentation of the data in normalized form in relationship to similarity theory as suggested by the reviewer is presented in section 4.2. Please also note other responses about non-stationarity of data. We do recognize that section 4.1 is a bit long and suggest splitting it into two parts and shorten the discussion somewhat. One part would then give the overview of the observed TKE budget which includes among other things the important results regarding correlation of mean TKE tendency to buoyancy production and shear production, which to our knowledge is not previously well-described using surface layer measurements. The second part would be about the classification of the different afternoons which we consider very important to characterize and describe what governs TKE during different afternoons.

M4: We suggest to split the previous section 4.1 into two sections. We intend to split the section such that the sentence on page 29765 line 6 which starts "We do a broad summarizing classification…" starts the second section with a new title "Classification" which then exclusively describes the results corresponding to Table 1. We consider this description relatively short consisting of essentially 4 paragraphs and suggest keeping this.

For the first of the two now splitted sections we suggest renaming it to "4.1 Overview of observed TKE budget for 10 IOP days" and significantly shorten it and also move some parts of the discussion around table B1 and B2 to Appendix B. In total this will shorten this section to about half of our initial submission. Our marked up text with comments is given below:

Start of M4 text with comments:
In Fig. 5, we present the observed hourly TKE budget for each afternoon transition period from 12:00UTC (normalized time 0) to zero buoyancy flux (normalized time 1) for all 4 levels of the small Divergence Site tower. The measurement levels (2.23, 3.23, 5.27 and 8.22 m) are shown as dashed, dash-dotted, full and dotted lines.

For buoyancy production (in blue), only very small height variations are observed and a general decrease during the afternoon is observed for all days. On 30 June, this general picture is partly interrupted by the presence of clouds changing the energy balance. The warmest days, 26 and 27 June with maximum temperature reaching about 32 _C, had less buoyancy production in comparison to for instance 19, 24 June and 1 July which were colder (19, 18, 24 _C).

Kommenterad [EN1]: observed near the surface

Kommenterad [EN2]: decrease with time

Kommenterad [EN3]: Suggested to be removed (information covered in appendix A).

Also, the dissipation rate (in black) is observed to have a general decrease during the afternoon transition for 8 out of 10 IOP days. Most significant deviations are found on 27 June and 5 July. These days indicate a clear increase of dissipation rate at the end of the afternoon, related to an increase in shear production as a response to the 8m wind speed increase during the afternoon. Also, 19 June and 1 July have temporary increases in the hourly mean local dissipation rate estimates. This is most clearly seen at the lowest measurement level in response to variations in local shear production during these afternoon periods. Hence, shear production plays an important role near the surface in the TKE budget for most of these 10 IOP days, but clearly has the most pronounced height dependence out of all budget terms. This implies that its effect as a production term is more localized and acting near the surface compared to the buoyancy contribution. The strongest dissipation rate is also found closest to the surface, but the height variation of dissipation is smaller.

Given that the TKE tendency (in green) is much smaller (two order of magnitudes) than the other budget terms this implies that the sum of turbulent and pressure transport (in magenta) compensate for remaining height variation in the budget. Because the tendency term of TKE is much smaller than the other budget terms we will refer to the hourly TKE as evolving in a quasi-stationary way. We note that this is sometimes considered to have the more strict definition that an equilibrium between production terms and dissipation exist, such that the tendency term becomes small (under assumption of small transport). Here, we use the term quasi-stationary to mean that the tendency of TKE is small in comparison to the other budget terms without requiring that the transport term be smaller than the dissipation or production terms. This result of quasi-stationarity is consistent with the observed slowly evolving mean TKE levels in LES for a large part of the afternoon of 20 June as described in Darbieu et al. (2015). Although, the TKE tendency then increased somewhat in the late afternoon in the large-eddy simulation a threshold of about $-1.1*10^{-5}$ m^{2} s^{3} was used in Darbieu et al. (2015) to indicate the faster decay, and this is still quite a small TKE tendency.

The height variation of transport is found to mainly be linked with local shear production. We will refer to the sum of turbulent and pressure transport as simply transport unless stated otherwise. It is worth noting that the transport term calculated as a residual (as discussed in Sect. 2.2.5) should be regarded as the most uncertain term in the budget, but despite this, it is consistently a negative term in the TKE budget. This implies a transport of near-surface produced turbulence to the surrounding environment and upper parts of the boundary layer. On 30 June, in relationship to changing cloud cover, the transport was found to be temporarily a positive term in the budget. On a few other occasions (such as for instance 19 June) this also occurred when the dissipation estimates were found to be more uncertain (or variable as noted from calculation of a standard deviation value of the dissipation within hour, not shown here).

To investigate general differences between the different days, we calculated statistics for each budget term during the afternoon period. These statistics are provided in Appendix B and some of the most important findings are discussed here. In Tables B1 and B2, we report the mean value (and standard deviation) for wind speed, shear production, buoyancy production, transport and dissipation. Table B1 refers to the 2.23m level and Table B2 the 8.22m level. Note also that a scale factor of 10^{-3} has been used for the budget terms.

It is important to note from Tables B1 and B2 that the variation between highest and lowest mean value for the different afternoons for shear production is as large as $6.7*10^{-3}$ m^{2} s^{3} for the 2 m level (and $3.5*10^{-3}$ m^{2} s^{3} for 8.22 m level). This can be compared with the buoyancy production variation that is only $1.5(1.4)*10^{-3}$ m^{2} s^{3} between the different afternoons. As we observed that these two terms are the dominant production terms in the near-surface budget and transport acts as a sink term transporting TKE out of the near-surface layers, we could expect variations in dissipation and transport between different afternoons to be mostly related to variations in shear production this close to the surface. To some extent, the less dominant variations in

Kommenterad [EN4]: Suggested to be reworded as:

Most significant deviations are found on days with an increase in shear production during the afternoon, leading to a clear increase of dissipation.

Kommenterad [EN5]: Suggested to be removed

Kommenterad [EN6]: Replace with:
. It

Kommenterad [EN7]: …terms with higher values near the surface.

Kommenterad [EN8]: Suggested to be removed

Kommenterad [EN9]: Suggested to be removed. See comment C17.

Kommenterad [EN10]: Suggested change in response to comment C17:
Here, we use the term quasi-stationarity to mean that the tendency of TKE is small in comparison to the other budget terms."

Kommenterad [EN11]: Suggested to be removed (already stated section 2.2.5)

Kommenterad [EN12]: The transport term is…

Kommenterad [EN13]: Replaced by:
Only a few occasion with positive transport term was observed in connection to changing cloud cover and more variable dissipation estimates.

Kommenterad [EN14]: in Appendix B and only briefly restated here.

Kommenterad [EN15]: Moved to Appendix B buoyancy production on different afternoons explain variations in near surface dissipation (and transport) as already seen from the overall decreasing trend of dissipation rate and buoyancy flux in Fig. 5. This is a main basis for simple modeling attempts of turbulence decay (Nadeau et al., 2011) in convectively dominated conditions. However, our data reveals that the role of shear and transport may be equally if not more important to take into account for modeling of sheared convective surface layers. It is worth commenting on the wind. Although weak (the afternoon mean values are always less than 3ms^{-1}) the relative importance of shear is stressed here. The variation between maximum and minimum afternoon mean values for transport is as large as 4.4(1.9)*10^{-3} m^2 s^{-3} and for dissipation 4.0(3.5)*10^{-3} m^2 s^{-3} for 2.23 (8.22) m. Larger variations in both the transport and dissipation term compared to the buoyancy term is observed for both measurement levels.

In Table B3, we show TKE mean values for the afternoon, early afternoon (between 12:00 and 13:00 UTC) and late afternoon (last 30 min), as well as the average TKE tendency for the afternoon. Values are given both for 2.23 and 8.22m level. Comparing TKE mean values and mean wind speed for the afternoon from Tables B1 or B2 does show that the three lowest TKE mean values occurring on 30 June, 2 and 5 July had the lowest wind speed and 25 June, which had the highest wind speed also had the highest mean afternoon TKE value.

There are of course exceptions from the rule that a higher wind speed lead to a higher TKE level that needs to be further discussed. In Fig. 6, we show the mean wind profiles for the 10 afternoons and have placed the same color on the two most similar profiles to facilitate further discussions to come. It is directly clear that 24 June and 5 July (in red) have essentially equal mean wind for the afternoon as a whole, yet from Table B3 we note that average TKE values are higher for 24 June. This is likely related to a higher mean buoyancy production of about 3.4*10^{-3} m^2 s^{-3} (the highest in the data set) in comparison to about 1.9*10^{-3} m^2 s^{-3} for 5 July, which is the lowest in the data set. Hence, several terms need to be considered to understand the observed variations in TKE. It is also worth noting that the higher mean TKE value for 24 June in comparison to 5 July is due to the early afternoon TKE being higher, but in the late afternoon the TKE level is higher for 5 July. This is mostly related to an increase of wind and shear production at the end of the afternoon on 5 July (see Fig. 5). This is only one example mentioned to illustrate the need for several explanatory factors when interpreting the behavior of TKE and TKE decay during the afternoon transition, as well as the evolution of the “forcings”.

Nevertheless, it is interesting to note that a relatively high negative correlation (-0.69) between the mean afternoon TKE tendency and mean afternoon buoyancy production exist as shown in Fig. 7a. This is interpreted to imply that in the case of a strong buoyancy production (both before and during the afternoon) TKE levels in midday are higher and therefore TKE decay rate during the afternoon can become higher. However, it is always small in comparison to other budget terms. A weaker positive correlation (0.33) is found between TKE tendency and shear production, implying that turbulence will decay more slowly during a more shear-driven afternoon as seen in Fig. 7b. This is in general agreement with reduced TKE decay rates for the afternoon found in large-eddy simulation when including wind shear (Pino et al., 2006) and is also discussed using a theoretical spectral model and LES data by Goulart et al. (2003, 2010). Best linear fit expressions have been included in both (a) and (b). Attempts were done to non-dimensionalize surface layer TKE tendency itself with measurement height and friction velocity and correlate it with various non-dimensional parameters such as z/L, z_i/L but it gave decreased correlation in comparison to relating tendency directly to buoyancy production as in Fig. 7a.
End of M4.

Kommenterad [EN16]: Suggestion to remove ”attempts”

Kommenterad [EN17]: Here we added a short section to give a summarized version of the parts that is moved to Appendix B. The text now reads:

”Variations in shear production between afternoons in table B1 and B2 was found to be significantly larger than buoyancy production. Variations in dissipation and transport between different afternoons was thereby found to be mostly related to varying shear production this close to the surface. Larger variations in both transport and dissipation term compared to the buoyancy term was observed, meaning that buoyancy alone can not explain in between afternoon variability for these terms. The three lowest TKE mean values in table B3 occuring on 30 June, 2 and 5 July had the lowest wind speed and 25 June which had the highest wind speed also had the highest mean afternoon TKE value.”

Kommenterad [EN18]: Moved to Appendix B.

Kommenterad [EN19]: We suggest removing these sentences to shorten the text.

C5: *"Page 29749, Lines 10-14. The length of the afternoon'transition' is 5-6 hours which is significantly larger than the turbulence timescale. This suggests that turbulence could be described as quasi-steady, at least for a significant portion of the afternoon. The evening transition is more of a transition period, but is not really part of the afternoon transition or the subject of the paper (e.g. 29754 Lines 19-21). Why do the authors think the transitional nature of the afternoon should be emphasized."*

R5: We agree that it is important to distinguish between the different periods (afternoon transition and evening transition) and will adjust the manuscript in response to this as we interpret this comment and a couple of other reviewer comments to suggest this.

Although it is natural to assume that buoyancy flux may respond in a quasi-steady way for long afternoon lengths (under the further assumption that sensible and latent heat flux evolves continuously and is not strongly influenced by for instance cloudiness) we are not sure that this is sufficient to motivate directly an assumption about quasi-steadiness for other variables such as for instance TKE without providing some observational evidence. This because TKE is also influenced by other processes which is not just related to the diurnal cycle but also by, for example, synoptic variability as discussed for wind in (Couvreux et al. 2015). For practical reasons we felt a need to limit our study and choose to focus on the afternoon period but we appreciate and share the reviewer's interest also in stable stratification. Included in the manuscript is some discussion about processes and situations specific on the site in stable stratification. This is done to still provide some overall background material for potential future studies which so far has been carried out mainly as very nice case studies by Roman-Cascon (2015), Nauta (2013) and others. We added some text to the manuscript to additionally emphasize our motivation and interest in studying the afternoon period.

M5: The sentence "The turbulence regime also changes from a mid-day well-mixed convective regime to a more heterogeneous and intermittent state with a residual layer overlying a stably stratified surface layer when entering into the evening transition (Stull, 1988)." on page 29749 line 11-14, is modified and directly after an additional sentence regarding motivation is added. The modified sentences now read:

"The afternoon transition starting in a mid-day well-mixed convective turbulence regime has an important influence for the onset conditions for the usually more pronounced regime change to a heterogeneous and intermittent state with a residual layer overlying a stably stratified surface layer when entering into the evening transition (Stull 1988). The differences between the very different convective regime and stable regime have a great influence upon for instance atmospheric dispersion as shown in e.g. Taylor et al. (2014). We focus here on the afternoon period before stable stratification starts as we consider there has been a lack of focus on this in previous studies and better understanding the onset conditions for the evening transition is of great importance."

C6: *"Page 29750, Line 13. Grant (1997) is about the evening transition after the surface buoyancy flux changes sign, as is Pino et al (2006). The problem is that the afternoon transition has not really been defined but seems to include various periods, such as quasi-steady evolution and the evening transition which are distinguished by different physical processes. I think the authors need to clarify this."*

R6: We appreciate the comment and will make it more clear in the manuscript. We added a modification M2 in response to a similar comment. The afternoon period was defined in Nadeau et al. (2009) and as now stated in the introduction this is the definition we use consistently throughout the manuscript.

M6: Further clarification regarding the referenced studies are suggested here. The sentence: "This has also been studied by Pino et al. (2006) using large-eddy simulation, Grant (1997) by observations and by Goulart et al. (2003, 2010) with a theoretical spectral model and LES data." is now modified to read:

"This was also studied by Pino et al. (2006) using large-eddy simulation by prescribing an instantaneous change to zero buoyancy flux, similar to Niewstadt and Brost (1986) but with the additional effect of shear production. Grant also provided an observational study for the evening transition and Goulart et al. (2003, 2010) studied the afternoon decay period in still unstable stratification with a theoretical spectral model and LES data."

C7: *"Page 29752, Lines 18-20. This seems a bit obvious, the smoothing is just giving weight to the present observation. Why should there be a robust value for S."*

R7: We remove these sentences as we agree that it does not read well. Garcia (2010) uses the wording of a *"robust value"* for when the method reaches a stopping point and value for the parameter S without being a priori chosen by the user. We agree that there is no such value for the current situation and even if we would temporarily have data for which such a value would be found by the algorithm it would not be general for another time period or data set.

M7: The sentence: The larger the value S, the more smoothing obtained, see Garcia (2010) and accompanying matlab function, s*moothn*. No robust value of the smoothing factor S was obtained. Is now removed and the beginning of the following sentence now reads: The performance was deemed … meaning that a ", however," was removed.

C8: *"Page 29752, Line 22-24. The early morning and the period before sunrise are not relevant to this study. They are sufficiently early that they are unlikely to have any effect on the afternoon data, even with smoothing. Periods over night are also not relevant for the study (although see main comments)."*

R8: We agree that it does not influence the results of the afternoon very much. We are hesitating at this point whether or not the reviewer considers that it is preferable to remove all stable data completely and not show it in the paper? We choose to keep it included in the paper because of the reasons discussed in previous comments (see for instance R5) but clarify in the text that it will not influence the results in the afternoon period which is our main focus.

M8: The following sentence is modified: "Also on some other periods in the morning or stable nighttime conditions the performance is less good than in unstable conditions."

So that it now reads: "Also during some other periods in the morning or during stable nighttime conditions the performance is not as good as in unstable conditions, but this will have little or no effect for the afternoon periods that is our main focus."

C9: *"Page 29752, Line 25. Why filter the wind-direction, given that wind direction is likely to be very variable for light winds (as mentioned). Wouldn't filtering the components of the wind vector be better ?"*

R9: Thank you for this comment. We clarify that in fact this is exactly what we do. We smooth the vector field with the routine provided by Garcia 2010. The impact of the filtering can however appear to be larger for wind direction than wind speed.

M9: The sentence on line 14, page 29752 is now rewritten to read: We used software from Garcia (2010) to do gapfilling and smoothing of the wind vector field.

C10: *"Page 29753, Lines 18-22. Again stable conditions not relevant to the present study."*

R10: Ok, we suggest to remove part of this paragraph

M10: The paragraph is now reduced to a single sentence and included in the previous paragraph: This procedure may seem restrictive, but most excluded data belonged to non- IOP days and/or stable conditions which is not in focus here.

C11: *"Page 29753-29754, Lines 27-3. Is the run of wind implied by the averaging time really relevant to the effects of topography on the data. Irrespective of the averaging time the air flow will have been determined by the same upstream topography."*

R11: Influenced by upstream topography, yes, but a shorter averaging time will surely limit the low-frequency variability which will be influenced to some extent by also the not so close surroundings. We will adjust the text to better explain that it is the low-frequency variability that is removed.

M11: The following text: “For a moderate boundary layer wind speed of $5 ms^{-1}$ a 10 min sample would in a simple calculation with Taylors hypothesis correspond to a distance of about 3 km. Topographical differences within 2–4 km around the site, on the plateau, are smaller than further away from the site. Therefore, choosing this averaging time may limit some complexity related to topography.”
Is modified to now read: “Our choice of a 10 min averaging period helps remove the sometimes observed large low-frequency variability, which we speculate could be partly connected to the larger topographical differences that exist outside of the ‘Plateu de Lannemezan’ area more than 2-4 km from the site.”

C12: *"Page 29755, Lines 10-15. In the list of causes of fluctuations in the 10 min averages of E statistical sampling error is not included. This may well be the most important source of variability compared to the physical sources given, so some comment seems to be needed."*

R12: We agree and now include it to the list of possible reasons and also give reference to some literature review on the subject.

M12: We add one sentence on page 29755 line 13-14: “Statistical sampling error is also a large source of variability both for variances and turbulent fluxes (Billesbach 2011).”

And in the reference list the recent comparison of methods is added: “Billesbach, D.P: Estimating uncertainties in individual eddy covariance flux measurements: A comparison of methods and a proposed new method, J. Agricultural and Forest Meteorology, 151, 394–405, 2011.”

C13: *"Page 29756, Lines 20-21. Would it be better to average the log of the dissipation rates from the 8 periods since epsilon must be positive. This will make a difference if the variability of the eight periods is large."*

R13: A procedure with averaging the log of the dissipation would give more weight to the low values than the high values and we do not understand why this would be appropriate to use for one of the terms in the TKE budget? In that case to be consistent we should probably do something similar on all the other terms as well? But we do not think this is appropriate for terms which could potentially also change sign. We suggest to keep our way to estimate an average dissipation rate for the hour using a simple arithmetic mean value.

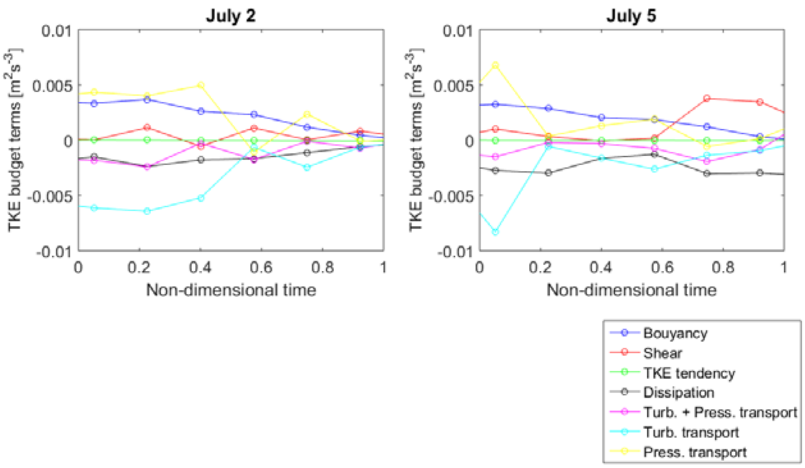
C14: *"Page 29757 Line 10. How were the vertical gradients of the third-order moments calculated, over the individual 10 min periods or the hourly averages. Could you calculate the contribution of the vertical component of the TKE flux to the transport term and compare it to your estimate of the transport term as a residual."*

R14: We added figures below for the TKE budget at 5.2m including turbulent transport directly calculated from measurements of third order moments using the 8.2 and 2.2 m level. We here choose the upper most and lowest measurement level to make the gradient calculation less sensitive. We still consider it very difficult that gradients of third-order moments will help us on a regular basis in our interpretation of the individual hourly budget. It is calculated here based as hourly averages (it is more variable if done at 10 min averages).

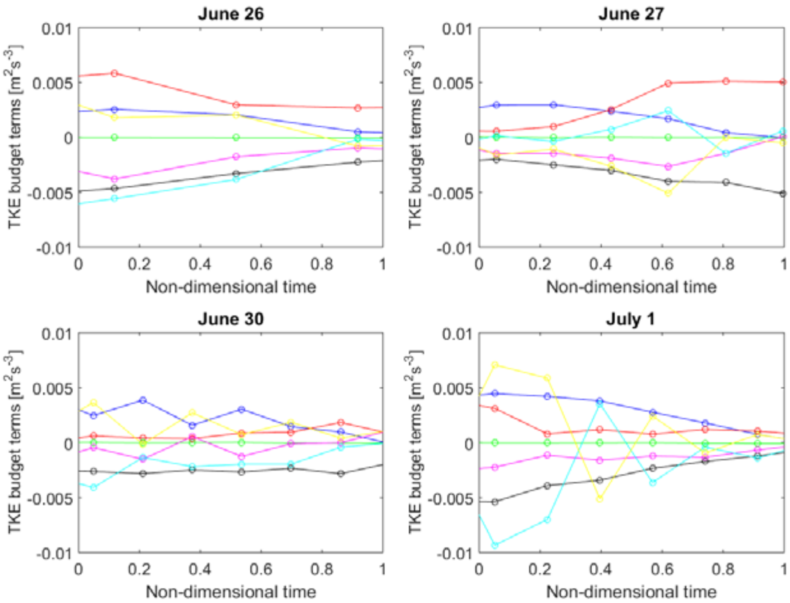
We do not see a clear link between the turbulent transport term to the other budget terms except for the pressure transport term which here is now calculated as the residual from all the budget terms (including the turbulent transport). There is anti-correlation between the pressure and turbulent transport terms and the change from one hour to the next can clearly be larger for these two terms compared to any other budget term. Although the turbulent transport term is often negative and the pressure transport often is positive which would be consistent with for instance LES simulations by Moeng and Sullivan (1994) and field measurements by for instance Högström (1990) there is also times with the opposite behaviour. In Högströms case the pressure transport was however large enough such that the net transport (sum of turbulent and pressure transport) was positive. This was not usually the case for our data.

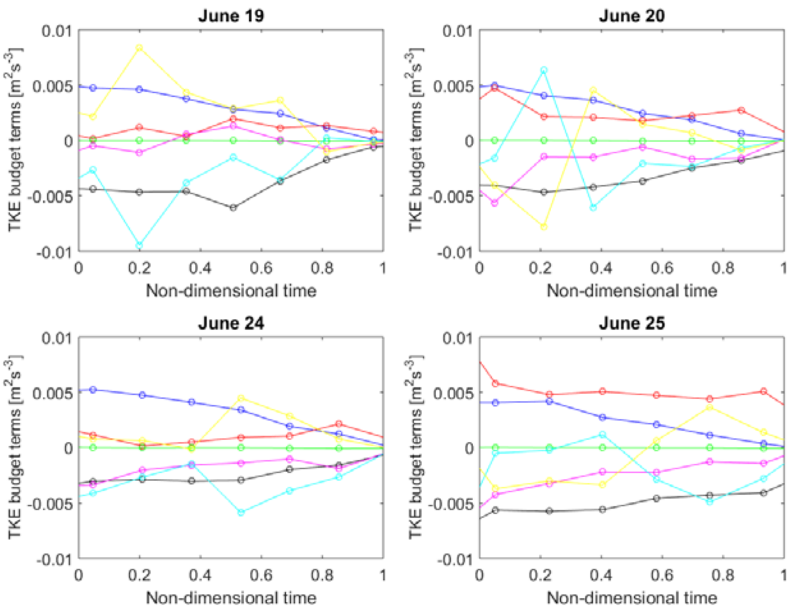
We have also included separate figures for only the turbulent transport term calculated with combinations of different measurement levels. This also illustrates that unlike some of the other budget terms it is difficult to find a clear height dependence for this term on a regular basis.

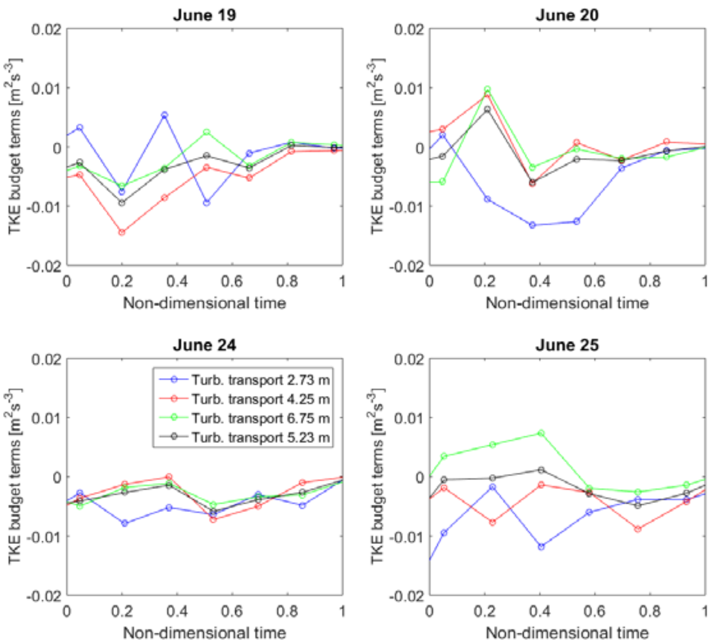
We propose to not include the turbulent transport term estimates in the manuscript as we consider these estimates too uncertain as previously stated in our manuscript.



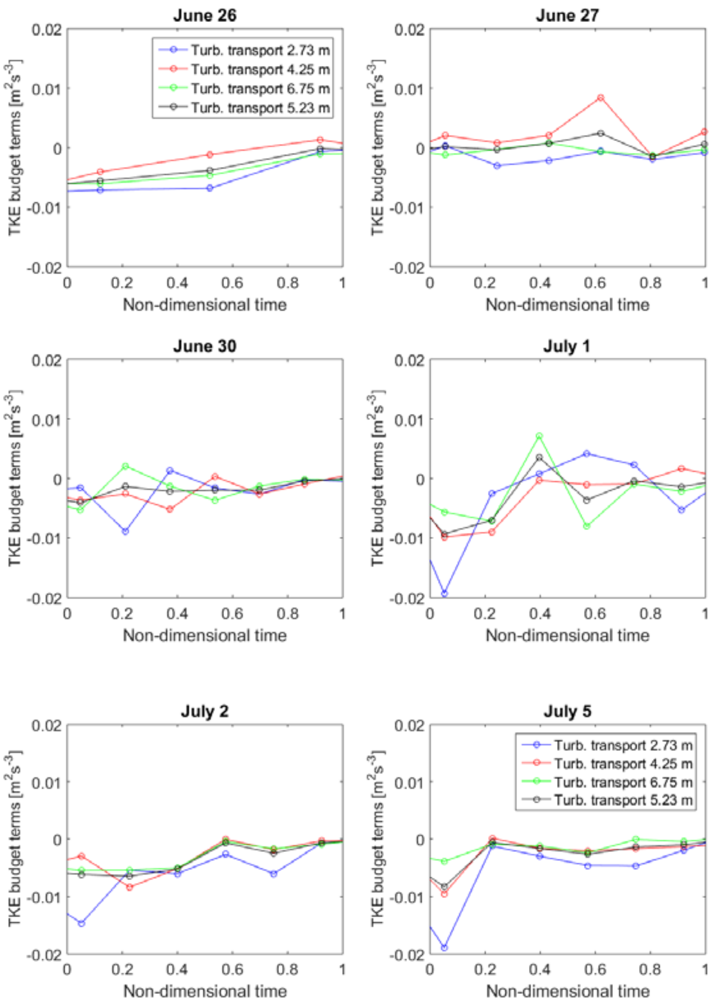
July 2
July 5
TKE budget terms [m^2s^-3]
Non-dimensional time
0.01
0.005
0
-0.005
-0.01
0
0.2
0.4
0.6
0.8
1
Bouyancy
Shear
TKE tendency
Dissipation
Turb. + Press. transport
Turb. transport
Press. transport


June 26
June 27
June 30
July 1
TKE budget terms [m^{2}s^{-3}]
Non-dimensional time
0.01
0.005
0
-0.005
-0.01
0
0.2
0.4
0.6
0.8
1


June 19
June 20
June 24
June 25
TKE budget terms [m^{2}s^{-3}]
Non-dimensional time
0.01
0.005
0
-0.005
-0.01
0
0.2
0.4
0.6
0.8
1



June 19
June 20
June 24
June 25
TKE budget terms [m^{2}s^{-3}]
Non-dimensional time
Turb. transport 2.73 m
Turb. transport 4.25 m
Turb. transport 6.75 m
Turb. transport 5.23 m


June 26
June 27
June 30
July 1
July 2
July 5
Turb. transport 2.73 m
Turb. transport 4.25 m
Turb. transport 6.75 m
Turb. transport 5.23 m
TKE budget terms [m^{2}s^{-3}]
Non-dimensional time

C15: *"Page 29760, Lines 1-9. Is the shallow night time drainage flow relevant to this study. My impression is that it isn't. It would be better to focus only on the afternoon period that is relevant."*

R15: We do feel that in this section it is important when describing the overall situation on the site to discuss that we have several complexities on the site that occur just when stable stratification starts. We explain on line 18-26 in section 5 page 29773 the main reasons why this is very important to mention and also in the paragraph just below the one indicated by the reviewer.

C16: *"Figure 5. These plots are rather unclear given the number of lines on them."*

R16: We suggest to change Figure 5 into a landscape format with subpanels as large as possible and remove some information from some of the axes when this does not limit the interpretation of the figure.

M16: A new improved Figure 5 will be in our revised manuscript.

C17: *"Page 29762 Lines 1-9. I don't understand this. Quasi-steady only requires dE/dt is small. Production=dissipation is not required for equilibrium or quasi-steady conditions, it is only a definition of a local balance which is known not to hold for the convective boundary layer."*

R17: We only discuss quasi-stationarity on these lines, but we do not feel strongly that it is important for the manuscript to keep our discussion about how this is sometimes referred to as possible to reach through an equilibrium or balance.

M17: We propose to remove the following sentence: "We note that this is sometimes considered to have the more strict definition that an equilibrium between production terms and dissipation exist, such that the tendency term becomes small (under assumption of small transport)."
Also the following sentence "Here, we use the term quasi-stationary to mean that the tendency of TKE is small in comparison to the other budget terms without requiring that the transport term be smaller than the dissipation or production terms." is proposed to now read:
"Here, we use the term quasi-stationarity to mean that the tendency of TKE is small in comparison to the other budget terms."
These changes are part of the proposed modifications to section 4.1 in response to the reviewer comment C4.

C18: *"Page 29762-29763, Lines 25-18. Is this discussion of the dimensional values of the TKE budget really needed. Surely the question to be answered is whether the terms in the TKE budget can be scaled using surface layer similarity. If they can then this automatically takes account of the variations in shear production and buoyancy in the TKE budget."*

R18: See our response to comment C4 and the proposed modifications given by M4. We have shortened the discussion and proposed to move some parts to Appendix B.

C19: *"Page 29765-29766, Lines 5-20. Is the discussion of the terms in the TKE budget at the level of individual points really necessary."*

R19: Please see our response R4 and proposed modifications M4 related to comment C4.

C20: *"Page 29766 Line 24. Spell out MO (Monin-Obukhov)"*

R20: Corrected

M20: "… as suggested in MO similarity theory." now reads "… as suggested in Monin-Obukhov similarity theory.

C21: *"Page 29767 Equation 5 is not really a very good fit to the shear production data in near neutral conditions. Most of the points fall below the curve. Some discussion of the low shear production should be given. The obvious explanation is that the von Karman constant is smaller than 0.4, although the present data suggest that it would have to be smaller than the Kansas value of 0.35. The other possibility is that it reflects the local heterogeneity of the site. Figure 1. shows buildings and trees not far from the mast, what effect might these have ?"*

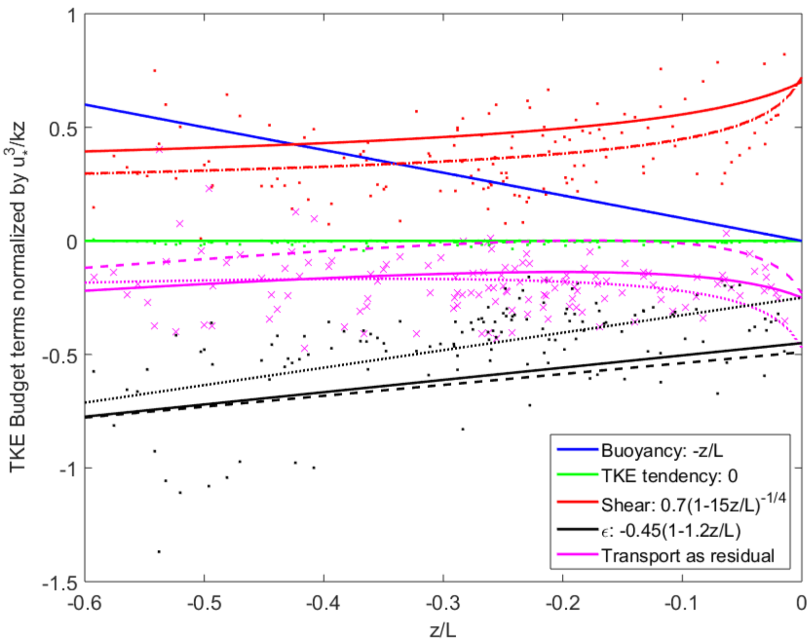
R21: The presented fit of shear production and also dissipation to the present data set needs to be explained as they were adjusted relative to objective least squares results reached for the full range of data (not just near-neutral data). In the figure below we have added a dashed red line for shear production that is corresponding to the curve $0.72(1\text{-}56z/L)^{-0.25}$ which is the best fit result as obtained with the curve fitting toolbox in Matlab® when using a model on the form $A(1\text{-}B(z/L))^{-0.25}$. This fit has a 95% confidence interval for A corresponding to (0.36, 1.07) and for B it was (-66.8, 178.9). Thus the fitted parameters have a substantial uncertainty especially for B and we considered using a value closer to previous results from literature.

If we fit only the more near neutral data with z/L>-0.6 the best fit was almost exactly the same $0.72(1\text{-}57)^{-0.25}$ with a 95% confidence interval for A of (0.39, 1.05) and for B (-63.5, 177). A dotted red line is also include in the figure below but is difficult to see as it is essentially overlayered with the dashed line.

The expression with adjusted fitting coefficients to shear production used in the paper was a way to try to make a reasonable fit also to the residual transport term when fitting the dissipation and using the expression for buoyancy production and the observed small (essentially zero) TKE tendency term. Please see also our response R23 regarding the adjusted fit.

Regarding the reasons for low normalized shear production we must admit that we are unsure if it is fully understood. Issues related to heterogeneity in the landscape is certainly one possibility and we may not be so close to the ideal homogenous stationary conditions at this site for which MO-similarity theory should work. We nevertheless consider it as a useful first approximation also to our situation. We must remember that in a limited data set such as implied by 10 studied afternoons we cannot expect all mean behaviors in the data to be in perfect agreement to the consensus mean behavior of data collected at more ideal sites and longer time periods of months or years. It could not be concluded at a 95% significance level by the curve fitting procedure that normalized shear production at neutral is different from the consensus value 1.0 (which would imply a von Karman constant of 0.4).

Prior to the experiment, the fourth co-author ran QUIC-URB (Singh, B., Hansen, B., Brown, M.J. and Pardyjak, E.R., Evaluation of the QUIC-URB fast response urban wind model for a cubical building array and wide building street canyon, Environmental Fluid Mechanics, 8(4), 281-312, 2008) to evaluate the potential impact of the building wakes on the measurement site. It was found that the nearest buildings were far enough away to have limited influence on mean wind flow.



M21: We propose adding a comment in the paper: "Normalized shear production was thus found to be low in the present data set in comparison to previously reported results. The scatter in our data was, however, found to be large enough that a von Karman constant value of 0.4 was found to be within a 95% confidence interval for neutral stratification. The reason for low normalized shear production is unclear, but could be a reflection of both measurement uncertainty, non-stationarity and heterogeneity." Replacing the sentence "The reason for this lower than usual normalized shear production in near neutral conditions should be further explored." on page 29767 line 19-20. And suggest to also delete the word "therefore" from the next sentence.

***C22:** "Page 29768 Figure 9. I would think that Fig 9 is consistent with phi_m(0) = 1 and that the low value given by Eq. 5 is because the data are not really neutral. The authors should comment."*

R22: Yes, it is true as stated in the paper that our slightly stable data with still low buoyancy flux is consistent with the consensus value for shear production of 1.0. How this should be interpreted however is, we must admit, somewhat unclear. Of course there is an uncertainty in the determination of the timing for when buoyancy flux is zero but the timings were independently determined by the first-author and compared to the reported timings of Blay et al. (2014) and found to be within about 10-15 minutes from each other. See suggested modification M21 for the revised manuscript.

C23: *"Page 29768 Equation 6. This is not a good fit to the dissipation data in near neutral conditions, most of the observed points fall below it."*

R23: In the above figure we have replotted the near-neutral data along with the black solid line which correspond to our reported expression -0.45(1 - 1.2z/L) = -0.45 +0.54z/L. Some black points is above the line but the line itself is close too and slightly above the dashed black line which is a least squares fitted line corresponding to -0.49 + 0.48z/L which was found when fitting all the dissipation data.

If only the more neutral data with z/L>-0.6 is fitted the result is different -0.25 + 0.77z/L which is shown as a dotted black line in the above figure. This is a better fit in this range but a worse fit for more unstable stratification.

If we would choose to use the fit to only the near neutral data (the black dotted line for dissipation and red dotted line for shear production) we would obtain a residual transport term expression which corresponds to the purple dotted line. This does not provide a better fit to the transport data and implies that for a very near neutral region the transport term would be a larger sink term in the budget than the dissipation term. There is no evidence of such a regime from the observations and we therefore chose to report an expression that is only marginally different from a linear least squares fit to the whole data set.

If the originally fitted expressions for the full range of data was used (dashed black line and dashed red line) this would give a transport term corresponding to the dashed purple line. This expression would not be a very good fit to the transport data, giving essentially no transport for some intermediate range of unstable stratification (-0.4<z/L<-0.05) for which there is observed negative transport values. This explains why we did not chose to just use the least-squares fitted expressions but chose to adjust them slightly to be in our opinion more overall consistent with the observations when considering the final fits to <u>all</u> the different budget terms and not just a single term by itself for some limited range of stratification.

M23: We suggest to add a short sentence just before the sentence "For the sum of turbulent and pressure transport term (to be consistent with observed small TKE tendency) our expressions in Eqs. (4)–(6) then suggest:".
The sentence suggested is: "Both our shear production relationship and dissipation relationship was determined by first producing least-squares fitted expressions, but these were slightly adjusted to assure that also the transport data in the TKE budget could still be reasonably well fitted by a residual expression."

C24: *"Page 29769 line 19-20. The comparison with Högstrom (1990) needs to be considered more carefully. The imbalance in Högstrom (1990) could be taken to represent the pressure transport plus errors, since the turbulent transport is accounted for explicitly. Högstrom's results could, therefore, be interpreted as implying a transport of energy into the surface layer, which differs from the interpretation of the present data."*

R24: Yes, we agree with what the reviewer is saying and in the manuscript we wrote that Högström (1990) reported positive transport values in neutral conditions and that we only observed such values on a few occasions. The first-author has been in personal communication with professor emeritus Ulf Högström about his study to confirm that we convey the accurate interpretation of his results. His residual term which may be interpreted as a pressure transport in neutral conditions is implying a net transport of 0.49 in dimensionless units into the surface layer. This is for his data compensated by the turbulent transport term sending roughly 50% of this back to upper levels while the other 50% gets locally dissipated which leads to an unusually large normalized dissipation in his case of 1.24. Ulf Högström also discussed with the first-author that these results were unexpected in comparison to previous studies of the surface layer, but has a main strength in that the study was carried out with 3 very accurate MIUU instruments and the data set included much near neutral data that was not under transient conditions. He also informed us that many of the previous studies such as the famous Kansas studies mainly had near neutral data for short periods of time during more transient conditions.

We find these differences very interesting and will include a comment to make it clear from our paper that Högströms data was mainly not concerned with transient conditions. Perhaps the question of transient conditions or not can have an effect on the normalized budget term values. We also believe however that a study about how to determine the most accurate dissipation rate can be important. We should remember that if the dissipation term would be lower in Högströms case (when it was very high) then this would have reduced his residual and pressure transport term. Or alternatively if our dissipation term is too low and would be as high as Högströms this would lead to a positive net transport in near neutral conditions.

M24: The following sentence "Högström (1990) also observed positive transport values in neutral conditions." is suggested to be changed to:
"Högström (1990) also observed positive transport values in an extensive data set of near-neutral conditions under steady conditions (not transitions). This was found to be related to a large pressure transport of turbulence into the surface layer which also led to an unusually large normalized dissipation of 1.24 (Högström 1990)."

C25: *"Page 29770 Line 6. Why is the length scale used in the relationship $E^{3/2}/z$ not taken to be a function of stability which might explain the variation with height in Fig. 10a. The results in Fig. 10a suggest that each height could be fitted by a line through the origin. The authors should comment."*

R25: We do not agree that a good fit of the data would be to fit a line through the origin in Fig. 10a for each measurement height as suggested by the reviewer. We consider that the effect of stability enters implicitly through the turbulence kinetic energy.

C26: *"Section 4.3 This is an interesting section and the final results in Fig. 12 are reasonable in suggesting that including the boundary layer depth in the parametrization of the dissipation rate works. The authors should discuss whether averaging over the afternoons is reasonable given the variations in the TKE budgets shown in Figs 5. Could they obtain this by averaging over a shorter period when the buoyancy term is large ? Do the authors consider that this result is related to the failure of MO similarity for the horizontal velocity components in unstable conditions."*

R26: Please note that averaging over afternoons was used in the exploratory work summarized in Figures 10 and 11 but for Figure 12 and the final evaluation we show that the result works at a shorter averaging time scale of one hour. The paper as such does not explore the relative contribution of vertical and horizontal wind but the comment from the reviewer is very insightful and we agree that this is a plausible interpretation.

M26: Please see our response R3 and modifications suggested in M3.