Response to reviewer 1

We thank Prof. Bohrer for the constructive comments and suggestions.

P4L7 - Prognostic models predict the result in a future timestep (relative to the times- tamp of observations they ingest). I think you mean here "diagnostically".

Corrected.

P4L9 – I think you somewhat misrepresent the meaning of prognostic and diagnostic models. The difference between the two is that diagnostic model does not include a time evolution. Neither of your terms requires time evolution. Please remove the terms "prognostic" and "diagnostic". I think the best rems to use here with be "directly" and "indirectly". Also see https://earthscience.stackexchange.com/questions/924/model-types- robust-diagnostic-versus-prognostic for a good explanation.

Thanks for pointing this out. We have removed the terms prognostic and diagnostic. The section now reads like this: "*Under these constraints, a strategy is needed to evaluate the TKE budget. The dominant mechanical production term, the buoyant production/destruction term and the dissipation term will be evaluated directly from the data. The residual of the TKE budget will be described as the imbalance as per equation 3 which would contain the effects of advection and transport terms*."

P4 where did eq. 4 come from (it is not in Banerjee et al 2016)? And how come it does not include the roughness length?

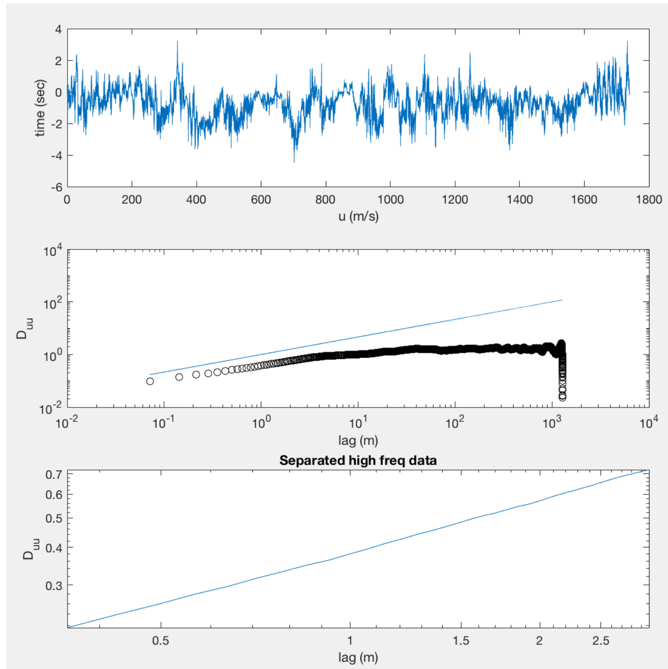
The equation is defined inline in Banerjee et al., 2016 after equation 3. However, two new references are added, where they are defined more explicitly (Li et al., 2016 and Kaimal and Finnigan, 1994). The roughness length comes in the equation for the profile of the mean longitudinal velocity, which can be derived by integrating equation 4. The roughness length comes as the lower integration constant. The gradient of velocity should be independent of the surface boundary condition.

P4L20 I recommend making this a numbered equation (the new eq 5), as this is a key component of your calculation, and you don't want to make the reader fish it out of the inline.

Agreed and changed to numbered equation.

P4L26 Can you show the results of this regression (perhaps in an appendix)? What was its R^2? As you can use a whole range or r values to calculate epsilon, how did you actually do

it? Picked a particular r? using the average with all possible r values (given your observation timestep and wind speed) within the 0.2-2 m range? Please add an equation stating the exact and complete formulation of epsilon the way you actually calculated it.



Since it has been a standard technique, it is not repeated in the main text and just the references are added. It is only discussed in the letter following Salesky 2013.. As mentioned in the text, the scaling relation used is $D_{uu}(r) = C_u \epsilon^{2/3} r^{2/3}$,

where $c_2 \approx 4.017 c_k$, $c_k = 18 c_e / 55$, and $c_e = 1.5$ is the Kolmogorov constant for the inertial range of the TKE spectrum E(k). Our estimate of ϵ was calculated by a linear regression to the compensated second-order structure function $r_1^{-2/3} D_{11}(r_1)$, i.e., $r_1^{-2/3} D_{11}(r_1) = c_2 \epsilon^{2/3} = a r_1 + b$,

using values of r1 in the range 0.2 ≤ r1 ≤ 2.0 m. The lower limit imposed on r1 ensures that distortions from the sonic anemometer finite path length are negligible. The upper limit on r1 is selected so as to ensure at least one decade of scales is available in the determination of ε. The regression coefficient b was used to obtain an estimate of the dissipation rate (i.e., $\varepsilon = (b/c_2)^{3/2}$); the coefficient a must be nearly zero if the data follow inertial-range scaling. The top panel of the attached figure shows a sample half hour high frequency time series. The middle panel shows the 2/3 scaling fit to the structure function and the third panel shows the extracted dataset between the r range 0.2-2.

Figure 2 – I assume you mean the half-hourly means (or is it the hourly? Daily?) Please state it in the caption.

Half hourly, mentioned in caption now.

P7L6 (and in the description of all other figures) in “thicker” and “thinner” lines, I assume you mean “black” and “red” lines?

Yes, corrected. Thanks for pointing this out. We also corrected the same mistake on P9L5. These two instances remained after we changed the thick - thin scheme which was used in the earlier version of the manuscript.

P7L3-6 this entire section (and similar sections that follow each of your figures) be- longs in the figure caption and not in the text. You should move this to the captions of figs 2 and 3, and start section 3.1 stating: "Our observations show that the desert in associated with higher wind speed . . .(Fig. 2). . .". I have the exact same problem with the first few line of section 3.2. Also, P10L7-11 should be removed (it is already in the caption). These are just examples, the same problem exist in many in other places.

We have faced instances where reviewers had felt uncomfortable without figure descriptions in the main text body although they were in the caption - since this is a subjective editorial issue, we are not changing this style at this time.

P7L9 I totally do not agree that the increase of u_* over the desert after 24th August "can be attributed to mesoscale motions appearing over the region". I think that this is a very simple and direct result of the change in tower height. I do not accept your claim (P6L11-12) that "However, the raising of the mast should not have affected the measurement of turbulent fluxes since it was done within the constant flux layer" - Obviously, and as clearly expressed in your observations - it did.

Accepted. Changed to "*This can be attributed to the raising of the tower height*". Also deleted the sentence : *"However, the raising of the mast should not have affected the measurement of turbulent fluxes since it was done within the constant flux layer"*.

P8L6 "however, after 24th August, the levels of w'w'. . ." Similarly, it is rather easy to claim that it is due to changing the tower height. As the vertical profiles of w'w' are different between the desert and forest (due to roughness length differences), the observed differences between w'w' are a function of observation height. Apparently at 15 m above the desert and 19 m above the forest are high enough to be at the "constant flux layer", the vertical profiles of TKE (u'u' + w'w') converge. However, when you observed at lower elevation, and apparently below the constant flux layer, your data show clear differences in w'w'. As currently stated, without explicitly reminding the reader about the elevation change at that exact date, this statement is highly misleading, especially as it is immediately followed by "Thus. . ." (next sentence, L7).

Accepted. The sentences describing the effect of large scale structures for $\overline{u'u'}$ *and* $\overline{u'w'}$ *are removed as well*. The section is replaced by:

The vertical velocity variance $\overline{w'w'}$ *over the forest is higher than its desert counterpart, however, after 24th August, the levels of* $\overline{w'w'}$ *over desert increases as well and become similar to the forest. It is due to changing the tower height. As the vertical profiles of* $\overline{w'w'}$ *are different between the desert and forest (due to roughness length differences), the observed differences* *between $\overline{w'w'}$ are a function of observation height. At 15 m above the desert and 19 m above the forest are high enough to be at the "constant flux layer", the vertical profiles of TKE ($\overline{u'u'}$+$\overline{w'w'}$) converge. However, when observed at a lower elevation, and below the constant flux layer, the data show clear differences in $\overline{w'w'}$.*

Further in the same point: P9L14 "Although the effect of the large scale structure after 24th August seems to dampen the [dissipation] over the desert while its effects on the [dissipation] over the forest are not very conspicuous." Here, again, it is rather clear to me that you record less TKE dissipation when you are further from the ground and above the roughness sub-layer. One strong argument for observed changes after Aug 24 being tower-height effects rather than change of forcing is that you only observe changes in the desert after the 24th, while the forest observation keep a rather consistent dynamics. You only changed the height of the desert tower, however, a change of forcing should be apparent over both forest and desert.

Agreed. This section is rewritten as : "*A smaller TKE dissipation is recorded when the measurement location is further from the ground and above the roughness sub-layer. One strong argument for observed changes after Aug 24 being tower-height effects rather than change of any large scale forcing is that changes in the desert are observed only after the 24th, while the forest observations maintain a rather consistent dynamics.*"

Fig 4 – what is "full TKE production"? You did not define such term, and if it is the e from eq 1, your data does not allow calculating it. I guess it is the sum of the mechanical and shear production terms. Please state it explicitly and do not call it "full TKE".

It is defined as the summation of mechanical and buoyant TKE production.

ALL figures - Please list in the caption the exact same symbols you used on the figures' y axes, so it is easier to understand what they are, and which is which. Currently you either ignore the symbols (e.g. fig 4), or provide a different version of the symbols on the caption than what is listed on the axes (e.g. fig 7 top 3 panels).

Please note that all terms are listed on the section of the text describing the figure. This way, the caption and the figure description are not exactly the same, referring to Your earlier point.

P9L7 remove "also". You already say "and"

Removed.

P9L12 “huge” is a very subjective term. Perhaps “significant” (if you tested it) or “large” or simply “a” difference (can you calculate and state the % difference?)

Agreed and removed huge. The % difference changes with time, following the exact same trend as the actual terms. replaced by: “*It also indicates that mechanical forcing, and not buoyancy makes a difference (mechanical production is higher by approximately an order of magnitude than buoyant production) in the turbulence generation over the desert and the forest*”.

Fig 5 – Explain what are the blue lines, and in the caption or on the figures (as in fig 6) provide the regression statistics (R^2, significance P) for the trend lines (blue?) that you are plotting.

This figure is now removed as we realized that it is not conveying much more information other than what is already there in figure 6.

Fig 6 Provide also the significance P.

p : 0.05.

P13L4 I do not understand why a larger integral eddy time scale over the desert is an indicator of “the transport by secondary circulations above the desert.” I think it is indicative of buoyant production of turbulence, which generates larger eddies than shear production.

Agreed and corrected.

P14 – Please combine eq 8-10 to a single equation that relates sigma_u/u* to alpha. It is easy to see that eq 10 is totally redundant (you are re-assigning a fixed number) , and neither eq 8 or 9 are too complicated to allow direct substitution (B1 is a simple additive term in eq 8).

This section is now removed. We agree with Your argument.

P14L11 How do you determine that “The data over the desert is found to be ill conditioned to compute alpha”? I think it’ll be more accurate to say that this empirical formulation was originally derived for forests (using data from forest flux towers) and therefore, the values of A_1 and C”_k for the desert are unknown.

This section is now removed.

Fig 8 – draw a dashed line for alpha=1 (but, as you can see below, I rather you removed this figure altogether) Section 3.5 – I totally do not understand what you learn from the VLSM analysis (shown in bottom panel fig 8). During the entire section, you explain how to calculate alpha, and provide excuses for not calculating it over the desert, and not being unable to use it to show sea breezes and other obvious large scale circulation patterns. The only actual informative stamen you make about VLSM is that "there are a number of large peaks of ↲ > 1 after 24th August which confirms the presence of VLSM and supports the interpretations of previous findings in this manuscript". I need to point out that there is presence of large peaks also before 8/24. In fact, larger (Aug 15 is the largest peak) and more (especially if you bundle up the adjacent peaks on the morning of Aug 27) peaks are present before you changed the tower height. Later, in the conclusions section (bullet point 4) you state that "The VLSMs are found to enhance turbulence fluxes and the nonlocal motions for both the forest and the desert. Although its main effect is to enhance the secondary circulations already existing over the desert transporting energy towards the forest." How do you reach this conclusion? Did you measure the correlation between alpha and turbulent fluxes? Can you prove that it enhances the mesoscale circulation already existing? This is purely speculative. If the reason for section 3.5 and conclusion point 4 is to provide justification for all the false claims about the effect of changing the tower height – than it doesn't work. It totally doesn't make a strong case to convince me that there was not effect of tower height. However, I do not understand the insistence on this entire point. Your conclusions do not rely in any way on the tower height and all the things you show about imbalance are valid before and after Aug 24, so why get yourself into this problem. Simply point out the places where the tower height may have influenced the observations, and further point out that the imbalance and other observations from which you draw conclusions about mesoscale circulations and TKE advection are showing similar patters regardless of the tower height. I will be happy if you remove this section and the 4th point of the conclusions.

Agreed. We have now removed the section and the 4th point of the calculation. Earlier discussions have now also pointed out the changes after 24th occurs due to tower height change.