This manuscript utilizes a combination of high-frequency, eddy covariance measurements coupled with two Doppler wind lidars, conducted during a 12-day summer period over a desert/forest interface, with the aim of assessing the extent of the secondary circulations previously observed at this site. Additionally, the simplified TKE budget is used to explain the discrepancies between the individual budget terms over the desert and the forest. The observed discrepancies are assigned to the presence of mesoscale secondary circulations caused by the marked heterogeneity between the two opposing land use types. The authors analyze time series and scatterplots of relevant quantities (first, second and third order statistical moments), as well as some derived quantities (integral length scales $In_{u,w}$, CBL depth δ, bulk parameter α). The authors conclude that the TKE budget terms (especially the imbalance term Imb) contain signatures of the aforementioned secondary circulations.

The manuscript provides a genuine view of the secondary circulations over a heterogeneity interface, which are currently held responsible for the surface energy balance non-closure. Hence, the study provides an important contribution to the understanding of a long-standing issue in boundary layer meteorology. The methodology implemented by the authors is well founded and the instrumental setup is sufficient for this purpose. However, the current version of the manuscript suffers from a number of critical drawbacks that the authors have not addressed, or have addressed very poorly. The manuscript requires major revisions prior to its acceptance for publication.

We thank the reviewer for the constructive comments and suggestions.

Major comments

page 4, line 10: You should cite some relevant work done on TKE budgeting, in particular pertaining to how turbulent transport terms, advection terms and the pressure correlation terms may contribute individually over the desert vs. over the forest. Be aware of what may influence the imbalance terms on which your study heavily relies (especially since your Imb also inherently contains the errors from the production and dissipation terms, as stated on line 11 on this page)

Not many instances were found in the literature where the nature of turbulent transport were studied across large scale surface roughness heterogeneities, except for Nadeau 2011 and Yue 2015. These references are now added.

page 4, line 16: The fact that you are conducting a field experiment over gently sloping terrain, immediately calls into place the need for more advanced rotation techniques, and

the inclusion of the directional shear term $\overline{v'w'}$ into the definition of friction velocity u_* (Rotach et al, 2008; Wilson, 2008);

$u*$ now contains $\overline{v'w'}$ in its calculation.

page 4, line 17: stability parameter ζ should include the displacement height d, so ζ =(z-d)/L;

It was already calculated using the displacement length d, the text is now corrected.

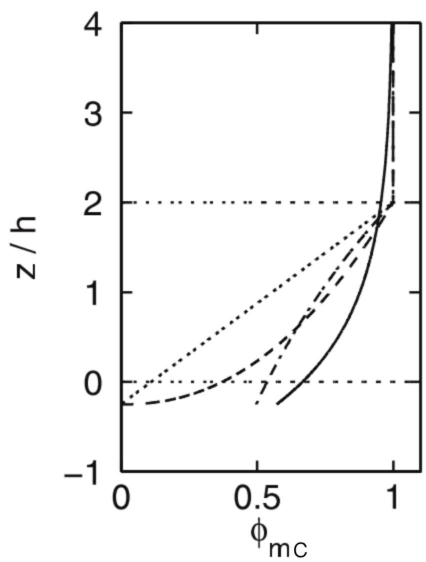
page 4, line 22: Have you tried estimating ϵ using other indirect methods, for instance the inertial dissipation method?

No, the structure function is used as it usually shows a more robust scaling relation compared to the spectral (inertial dissipation) method since it is calculated in the real space. Moreover, the scaling relation (2/3) in the structure function method can be translated into the scaling relation (-5/3) in spectral space - so ultimately there is not much difference between the two.

page 6, line 10: A mobile mast? Does this mean that the mast was moving around during the 12-day period? If it was not, then please omit mobile because it just distracts;

Removed.

page 6, line 11: Are you confident enough that with being just 9 meters above the canopy top you are above the roughness sublayer? There is no mention of the roughness sublayer here, and there should be one - particularly because you are applying the flux-gradient version of Monin-Obukhov similarity theory to estimate an important TKE budget term, which becomes invalid if you are within the roughness sublayer;



This is a great point and it was already discussed in the first round of revision. As observed from the figure taken from of the roughness sublayer correction function from the paper (figure 2a): Harman, I. N., and J. J. Finnigan, 2007, A simple unified theory for flow in the canopy and roughness sublayer. Boundary-Layer Meteorol., 123, 339–363, its value is about 1 at z/h=2 which is the case in this campaign. This correction function ϕ_{mc} is multiplicative to the original stability correction function ϕ_m. So its value being 1, this is not included. Moreover, it also justifies the fact that we are above the roughness sub layer for both heights 9 m and 18m. We agree that this was not articulated well in the text before. Now it is mentioned.

page 6, line 11: In my opinion, here lies the biggest weakness of this manuscript. First, the raising of the mast occurred on the 23rd, and a lot of subsequent analyses describe the different behavior that suddenly began to occur from the 24th onwards, due to a passage of a large scale mesoscale system. Can you show that there indeed was a large scale system present, for example by showing any before/after upper-level charts? To add to this, you briefly describe the synoptic conditions in the 4th bullet point of the Conclusion (page 16, line 16) - however that information should be moved out of the Conclusion and expanded upon with supporting figures and charts much earlier in the manuscript;

Following the suggestion of reviewer 1, we have removed this sentence. We realized that changing the mast height was indeed responsible for the changes observed after 24th. We have also removed section 3.5 and all discussions of the passage of the large scale structures from the conclusion as well. Changes in individual statistics have been explained in conjunction with the raising of the mast as reviewer 1 suggested. Please see response to reviewer 1.

page 6, line 12: You are simply invoking the constant-flux layer hypothesis with- out citing relevant literature which actually looked at its validity. As it happens, this hypothesis is more often violated than met. Grachev et al (2005), Nadeau et al (2013) and Babic´ et al (2016) are some of the studies that have done this, and found the hypothesis to be true only for certain fluxes and during limited stability conditions. In particular, Babic´ et al (2016) have shown that the sensible heat flux is indeed constant within the daytime surface layer, however this was not true for the momentum flux. Since their study was also conducted over a shrubland, I expect similar to hold in your case (over the desert). My concern is that the raising of the mast by 6 meters may have partially invalidated your conclusions pertaining to the evolution of the friction velocity and consequently mechanical production term after the 24th. Since you don't have at least two lev- els of measurements to estimate the flux divergence, I would highly recommend to cite the relevant literature and insert your view on the potential invalidation of the constant-flux layer hypothesis, especially ways in which your results may be sensitive to assuming that this hypothesis is true.

It is a valid suggestion. To avoid the confusion, we have removed the sentence altogether. As pointed out by reviewer 1, certain changes can indeed be attributed to the change in mast height. As we have noted later: "*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* $\overline{w'w'}$.

also,

"*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.*"

page 6, line 16: You do not mention how you have pre- and post-processed the eddy covariance data. This also goes for the lidar - you list all these technical specifications (even the serial numbers!), yet you only use the lidar data to calculate the CBL depth. It should be the other way around - the eddy covariance data should be given much larger emphasis in terms of technical specs: What type of sonic anemometers were used? What rotation procedure was applied? How did you detrend the time series? How do you justify the choice of the 30-min averaging time? If all of these are the same as in Fabian Eder's AFM paper, then at least mention this.

We have simply mentioned that the details of the EC method are similar to Eder 2015 paper. More details are added on the rotation technique.

page 7, line 6: But the lower amount of friction over the desert could simply be responsible for higher wind speeds?

Noted and added.

page 7, line 10: On the contrary, this is the perfect opportunity here to discuss the synoptic conditions before and after the 24th. Please include before/after upper-level charts to clearly elucidate the structure of synoptic influence;

Following the argument from reviewer 1, This is changed to : "*This can be attributed to the raising of the tower height*".

page 7, line 13: gentle topography and strong vertical updrafts - something that is particularly sensitive to coordinate rotation. Please specify earlier what rotation technique was applied. Wilczak et al (2001) come to mind, who have shown that even a subtle misalignment of the coordinate system may lead to large errors in momentum flux estimates;

Discussion on rotation technique added.

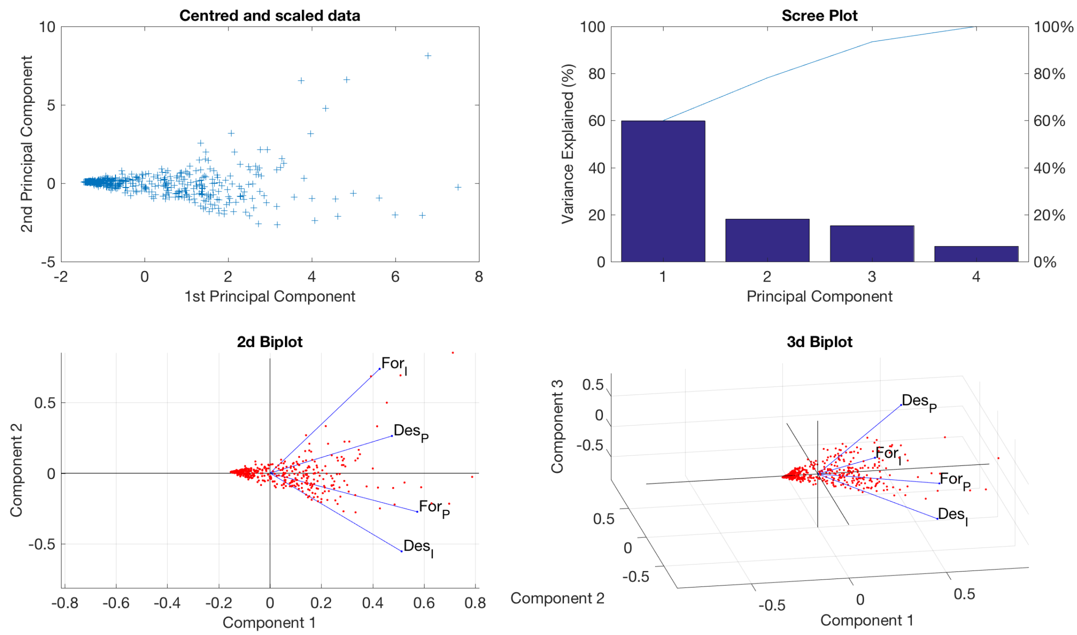
page 10, line 9: Why do you report results for stable stratification all of a sudden? The goal of the manuscript is to try and gain deeper insight into the secondary circulation that causes the DAYTIME energy underclosure, not the NIGHTTIME energy overclosure. Besides, you do not talk about stable conditions hereafter all that much anyway.

We realize that figure 5 is not conveying more information, so it is removed altogether. The same information can be extracted from figure 6.

page 11, line 7: There is a tendency in this paper of very easily assigning the patterns in the Imbalance term to secondary circulations, with oftentimes very far-fetched statements such as this one. Please keep speculations to a minimum when you comment about terms which contain too many variables and uncertain- ties that you can't directly estimate (e.g. the turbulent transport term).

This is a great point and was also raised by reviewer 1 in the previous review. To clarify this issue, we resort to principal component analysis (PCA) which shows the relationships between different variables in a multidimensional data space (not shown in the paper). The first panel shows the centered and scaled data. The second panel shows the total amount of data variability explained by the principal components. As observed, top 2 principal components explain about 80% of the data variability. The 3rd panel shows the 2d biplot and the 4th panel shows the 3d biplot. The angles between the vectors indicate the degree of correlation between the variables. An angle of zero degrees or 180 degrees indicates perfect correlation and orthogonality between the vectors indicate zero correlation.

The biplot confirms that the desert production is highly correlated with the forest production as they should have the same forcing. The desert production is also correlated with the TKE imbalance over the forest. However, the desert production has weaker correlation with desert imbalance. Moreover, the desert imbalance has high correlation with the forest production. The imbalance over desert and forest have almost zero correlation. This indicates that the large scale mostly thermal structures are transported from the desert to the forest. Not all the transport over the desert is generated by the desert production and the large scale nonlocal structures contribute



to the transport over the desert, which creates additional forcing over the forest. Thus the high correlation between the desert imbalance and forest production is retained. However, the additional correlations between the desert production and desert imbalance, as well as desert production and forest production are added and acknowledged in the text.

page 11, line 30: Using large scale in conjunction with mesoscale is counterintuitive - did you mean large scale macroscale? If yes, then look at my point earlier above about the need to show upper-level synoptic charts.

As discussed earlier, we have removed all discussions on mesoscale motions.

page 12, line 5: Why suddenly involve sweeps/ejections? The time scale of these coherent structures (hairpin vortices, streaky structures, ramplike convective plumes) is much smaller (20-180 s) than those of secondary circulations (several hours). Besides, secondary circulations do not sweep and eject momentum (in the Theodorsen horseshoe sense) since they are fixed to the heterogeneity interface.

Triple moments have been shown to be connected with sweep-ejection motions (Nakagawa and Nezu, 1977; Raupach et al., 1986; Cava et al., 2006; Katul et al., 2013; Banerjee et al., 2017a). We don't mean to say that the secondary circulations are causing these motions, but the net transport of turbulent energy is causing them.

page 13, line 4: The integral time scale In_u is typically well correlated with the CBL depth . But here you get the opposite: even though the CBL depths over the forest and desert are roughly equal (Fig. 8), the In_u scales show the opposite behavior. Furthermore, I do not see a significant bulk difference in magnitude of the integral scales before and after the 24th, i.e. based on integral scales I would not be confident saying that there was a secondary circulation after the 24th and there was not one before the 24th. I do not see the connection you made with secondary circulations appropriately justifying this discrepancy from a physical standpoint. It looks like you are incorrectly assigning the turnover time of CBL-scale convective thermals (on the order of less than 200 s judging from Fig. 7) to turnover time of secondary circulations (which may last for several hours). If this were true, your autocorrelation function would experience a zero-crossing at much longer time lags (which obviously it does not).

Agreed. This sentence is removed and replaced by what was suggested by reviewer 1: "*More interesting is the observation that the integral time scales for the eddies above the desert are larger than the forest- both of which increase after 24th. This is another indicator of buoyant production of turbulence, which generates larger eddies than shear production*".

page 13, line 8: From Fig. 8, it is obvious that the CBL depths over the forest and desert are almost the same, especially after the 24th. The forest is only slightly larger only on the 18th and the 19th...

This section is now removed.

page 14, line 9: Ill conditioned in what regard?

This section is now removed.

page 14, line 9: Not entirely obvious to me how the forest α would be representative for the desert: When I look at Eq. 8, σ_u (Fig. 3) and δ (Fig. 8) are similar between the forest and desert, but u_* is very different (Fig. 2). This seems to invalidate the justification to extend the forest α to the desert.

This section is now removed.

page 14, line 11: There are only two instances of large α after the 24th, while there are four instances prior to the 24th. Hence this statement is invalid.

This section is now removed.

Figure 8: You don't comment on the apparent tendency for large α (on the 18th, 22nd, 23rd, 24th, 27th) to occur when the CBL is still growing (mostly during morning and early afternoon hours)... Any thoughts on this?

This figure is now removed.

page 14, line 14: This would imply that you would see a low-frequency bump in the vertical velocity spectra, both before and after the 24th. I would like to see a plot of the temporal evolution of the vertical velocity variance profiles from both lidars (perhaps in the form of a time-height Hovmöller diagram?). Maybe something in there would correspond well with the large α instances? I'm aware that Fabian Eder already did something similar in his AFM paper, however he did it only for the 25th-27th period, so after the apparent large scale system passage on the 24th - not before it.

This section is now removed.

Minor comments

page 1, line 16: avoid the use of citations in abstracts.

Removed.

page 3, Equation 1: θ, rather than T , is the traditionally accepted nomenclature for potential temperature(s);

Since we have used T consistently in the paper and other previous papers, it is retained.

page 3, line 25: replace the too-colloquial sheer with large;

Replaced.

page 4, line 7: the word prognostically should come earlier in this subsentence rather than at its end;

As pointed out by reviewer 1, we have removed the terms prognostically and diagnostically.

page 6, line 2: You should emphasize that the Tower 1 location is different from the one analyzed in the cited Eder et al paper.

Mentioned.

page 6, line 11: What are the displacement heights at the forest and the desert sites? See above comment about proper definition of ζ

There is no displacement length considered for the desert. For the forest, it is taken as 2/3rd canopy height.

Figure 1: Please include a map scale and a terrain elevation contour line. As for the north-pointing arrow, please move it to e.g. the top left corner since I barely noticed it in its current position;

Done.

page 6, line 24: So the lidar at tower 2 was working during these outage periods? Why don't you then report its in Fig. 8?

Figure 8 is now removed.

Figure 2 and the following figures: Overbars, rather than brackets, are traditionally used for denoting temporal averages. Brackets are usually used for spatial averaging. There are some inconsistencies: you use brackets around the (co)variances, while in the text you use overbars. Please correct the relevant y- labels. Additionally, specify the x-axis as time in UTC. Finally, I would recommend putting letters to the top corner of each subplot and then accordingly modifying the text to mirror this change.

MATLAB has been used to generate this figures, and a glitch does not allow having over bars in the labels. It has been mentioned that they convey the same thing. time in UTC is mentioned. Letters are not used since there is only one column and each has a specific ylabel.

Figure 3: The momentum flux should have a minus in front of it. Having it with- out one implies that there is a momentum source and mechanical destruction of turbulence (assuming a log law) - which is not in line with the rest of the analyses (where you do indeed have a momentum sink and a corresponding mechanical production of turbulence);

Corrected.

page 7, line 6: The sentence Thicker line indicates desert and thinner line in- dicates forest is a remnant from a prior version of the manuscript before you replaced the thin line with a red line. Remove or modify this sentence.

Corrected.

page 9, line 5: The sentence Thicker line indicates desert and thinner line in- dicates forest is a remnant from a prior version of the manuscript before you replaced the thin line with a red line. Remove or modify this sentence.

Corrected.

page 9, line 8: The start of the sentence Buoyant TKE production over the forest is slightly larger over the forest... is unclear. Please rephrase.

Corrected.

page 10, line 2: Replace on the desert with over the desert.

Done.

page 10, line 4: Replace indicting with indicating.

Corrected.

page 10, line 4: It would be quite instructive to calculate the Pearson correla- tion coefficient between the two Imbalance terms. Also adding a Imb/forest vs. Imb/desert scatterplot to Fig. 6 would be another way of expressing this;

The physical significance of that correlation is not well understood, so we are not adding this. Moreover, from the pca analysis shown before, these two imbalances have almost zero correlation (orthogonal to each other).

Figure 5: I cannot tell the range extent in the stability parameter in some of the subplots... Please make the x-label ticks more numerous.

This figure is now removed.

page 11, line 6: Be careful with wording and speculations here - sounds like you are aiming at studying the turbulent transport term (which naturally you cannot estimate in your case);

Rephrased to : figure 6 is used to better understand the nature of turbulent transport between the desert and the forest.

page 12, Eqs 6 and 7: Is there a reason for not including the ϵ_{uw} and ϵ_{wT} dissipation terms here?

We just wanted to be consistent with the other references provided.

page 12, line 8: ...opposite in nature... sounds ambiguous. Consider rephrasing (for instance ...opposite in sign...).

Corrected.

Figure 8: In the spirit of Figs. 2-7+9, please replace the thin black line with a solid red line.

Figure now removed.

Figure 8: Why interpolate δ on the 21st for the forest, when you don't do it any- where else in the figure?

Figure now removed.

Figure 8: Transform the y-axis into a logarithmic one, given the prevalence of small α.

Figure now removed.

Figure 9: Please consider scaling the averaged vertical velocity on the x-axis with the average Deardorff convective velocity scale w_*;

We wanted to show the strength of the recirculation, so the unit is retained.