Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-159-RC2, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Turbulent transport of energy across a forest and a semi-arid shrubland" *by* Tirtha Banerjee et al.

Anonymous Referee #2

Received and published: 23 May 2017

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 landuse 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.

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);
- **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);
- **page 4, line 17**: stability parameter ζ should include the displacement height d, so $\zeta = (z - d)/L$;
- **page 4, line 22**: Have you tried estimating ϵ using other indirect methods, for instance the inertial dissipation method?

- **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;

- **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;

- **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;

- **page 6, line 12**: You are simply invoking the constant-flux layer hypothesis without 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 Babić 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, Babić 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 levels 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.

- **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.

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

- **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;

- **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;

- **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.

- **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 uncertainties that you can't directly estimate (e.g. the turbulent transport term).

- **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.

- **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.

- **page 13, line 4**: The integral time scale In_u is typically well correlated with the CBL depth δ. 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).

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

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

- **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.

- **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.

- **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?

- **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.

Minor comments

- **page 1, line 16**: avoid the use of citations in abstracts.
- **page 3, Equation 1**: θ, rather than T, is the traditionally accepted nomenclature for potential temperature(s);
- **page 3, line 25**: replace the too-colloquial *sheer* with *large*;
- **page 4, line 7**: the word *prognostically* should come earlier in this subsentence rather than at its end;
- **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.
- **page 6, line 11**: What are the displacement heights at the forest and the desert sites? See above comment about proper definition of ζ;
- **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;
- **page 6, line 24**: So the lidar at tower 2 was working during these outage periods? Why don't you then report its δ in Fig. 8?
- **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.

- **Figure 3**: The momentum flux should have a minus in front of it. Having it without 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);
- **page 7, line 6**: The sentence *Thicker line indicates desert and thinner line indicates 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.
- **page 9, line 5**: The sentence *Thicker line indicates desert and thinner line indicates 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.
- **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.
- **page 10, line 2**: Replace *on the desert* with *over the desert*.
- **page 10, line 4**: Replace *indicting* with *indicating*.
- **page 10, line 4**: It would be quite instructive to calculate the Pearson correlation 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;
- **Figure 5**: I cannot tell the range extent in the stability parameter in some of the subplots... Please make the x-labelticks more numerous.

- **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);
- **page 12, Eqs 6 and 7**: Is there a reason for not including the $2\epsilon_{uw}$ and $2\epsilon_{wT}$ dissipation terms here?
- **page 12, line 8**: *...opposite in nature...* sounds ambiguous. Consider rephrasing (for instance *...opposite in sign...*).
- **Figure 8**: In the spirit of Figs. 2-7+9, please replace the thin black line with a solid red line.
- **Figure 8**: Why interpolate δ on the 21st for the forest, when you don't do it anywhere else in the figure?
- **Figure 8**: Transform the y-axis into a logarithmic one, given the prevalence of small α.
- **Figure 9**: Please consider scaling the averaged vertical velocity on the x-axis with the average Deardorff convective velocity scale w_*.

References

1. Babić, Nevio, Željko Večenaj, and Stephan FJ De Wekker. "Flux–Variance Similarity in Complex Terrain and Its Sensitivity to Different Methods of Treating Non-stationarity." Boundary-layer meteorology 159.1 (2016): 123-145.
2. Grachev, Andrey A., et al. "Stable boundary-layer scaling regimes: the SHEBA data." Boundary-Layer Meteorology 116.2 (2005): 201-235.
3. Nadeau, Daniel F., et al. "Similarity scaling over a steep alpine slope." Boundary-layer meteorology 147.3 (2013): 401-419.
4. Rotach, Mathias W., et al. "Boundary layer characteristics and turbulent exchange

mechanisms in highly complex terrain." Acta Geophysica 56.1 (2008): 194-219.
5. Wilczak, James M., Steven P. Oncley, and Steven A. Stage. "Sonic anemometer tilt correction algorithms." Boundary-Layer Meteorology 99.1 (2001): 127-150.
6. Wilson, J. D. "Monin-Obukhov functions for standard deviations of velocity." Boundary-layer meteorology 129.3 (2008): 353-369.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-159, 2017.