# Turbulent transport of energy across a forest and a semi-arid shrubland

***Banerjee et al***

**round 1 verdict: MAJOR REVISION**

In most cases, the responses of the authors have been acceptable. Specifically with the raising of the mast over the desert on the 23rd, most of the comments and analyses have been modified to reflect this more objectively and correctly. Some of the other weaknesses have also been appropriately addressed, e.g. justifying the constant flux layer hypothesis. The authors also interpret results more cautiously when attributing the observed differences in the imbalance term between the forest and the desert, to a secondary circulation.

However, the manuscript still feels incomplete, particularly because of the heavily underutilized Doppler wind lidars. As it currently stands, these lidars are only mentioned in Appendix B - which is not even referenced in the main text at all. As such, Figure 7 contradicts the time series of $\langle W \rangle$ from the sonics - but this is not explained or even mentioned at all in the present version of the manuscript.

I propose another major revision of the manuscript, by incorporating more analyses involving the two Doppler wind lidars. In doing so, the authors have a unique opportunity to provide another view of the secondary circulations which hasn't been explored so far. I believe that such analyses will substantially increase the value of this study.

# Major comments

- **page 4, line 20**: Although you apply planar fit now, you still don't incorporate the directional shear $\overline{v'w'}$, in the definition of the friction velocity. It's likely it won't alter the current $u_*$ values that much, but it should still be included for completeness;
- **page 5, line 28**: how was $\delta$ obtained?
- **page 7, line 1**: Is the mean vertical velocity that is plotted in Fig. 2, the vertical velocity that is rotated with the planar fit procedure?
- **Fig. 2 and Fig. 7**: This is where the major drawback of this study lies: In addition to these two figures contradicting each other, it remains unclear why that would be so since the authors do not address it at all.

  First, the authors do not compare Fig. 7 with Fig. 4a,b in Eder et al (2015, AFM).

Their LES results and your lidar results are in agreement (at least in terms of the sign of average vertical velocity).

Second, it's obvious from Fig. 7 that at the lowest range gates of both lidars, the vertical velocities converge to zero, perhaps maybe even changing signs very near the surface - which would then be in accordance with Fig. 2. However, this area is under the influnce of the dead zone from the lidars, so only speculation is allowed. Even so, it remains clear from Fig. 7 that, throughout the CBL, it is the desert that experiences a downdraft and the forest is the one associated with an updraft. This is in contradiction with the general conclusion of the study, i.e. with the orientation of such a secondary circulation that the authors suggest exists (based on $Imb$ and triple-order moments analyses).

This raises another question: does the secondary circulation extend all the way to the top of the CBL or just to a certain $z/\delta$? The discrepancy explained in the previous paragraph would suggest the latter. As pointed out by the authors, an answer may lie in the vertical velocity skewness $Sk_w$. A simple analysis to do would be to, for available times that the lidars were working, obtain vertical profiles of $Sk_w$ and inspect their temporal evolution over both the desert and the forest. E.g. plotting a time-height Howmoller diagram for the whole 12 days of the campaign comes to mind. Since the forest experiences negative $Sk_w$, it would be interesting to see how far up this persists. Of course, because a third-order moment is involved, a longer averaging period is required - for instance 1 (maybe even 2) hour (thus skipping the single VAD scan that was performed in between vertical stares).

Another useful avenue that would go along the 'eddy size distribution' line-of-thinking that the authors mention in the beginning of the Conclusion, would be to spectrally decompose the lidar-derived $Sk_w$. This has been done on several occasions (Hogan et al, 2009, their Fig. 7). By doing this for each range gate, a height-frequency Howmoller diagram could be designed (for a single representative case study, not all 12 days). Such a diagram would then elucidate how each scale of motion contributes to overall $Sk_w$. I'm certain some interesting differences between the forest and the desert would become apparent.

- **page 11, line 6**: I'm still not comfortable with bringing ejections and sweeps into the mix here (as I've pointed out in my first response). I agree that they are contributors to third-order moments, but the way you describe it currently makes it seem like ejections and sweeps are related to secondary circulations - which is not true. A word of caution would help here, disclosing the fact that ejections/sweeps are a much finer-scale, smaller-scale motions than secondary circulations.

  In line with the spectral decomposition of lidar-derived $Sk_w$ I mentioned earlier, I wonder if decomposing the mixed third-order moments would reveal some aspects at the low-frequency part of those cospectra? Significant deviations should be expected there, considering the different ranges of signs (between desert and forest) that you obtained in Fig. 6.

Also, the fact that higher-order moments require longer averaging times than means and variances, makes me question how reliable the different signs between the forests and desert actually are. Sensitivity analyses of $\langle w'w'u' \rangle$, $\langle w'w'w' \rangle$ and $\langle w'w'T' \rangle$ as a function of increasing the averaging time with increments of 30 minutes, would make the results more bulletproof.

# Minor comments

- **page 2, line 5**: having both *or* and *etc.* is redundant, keep just one of them;
- **page 4, line 16**: it's usually referenced as Monin-Obukhov Similarity Theory (instead of Stability);
- **page 8, line 7**: 23rd instead of 23th;
- **page 8, line 12**: 23rd instead of 23th;
- **page 9, line 1**: *in* the constant flux layer;
- **page 9, line 12**: replace *summation* with *sum*
- **page 10, line 3**: 23rd instead of 23th;
- **page 10, line 12**: 23rd instead of 23th;
- **page 11, line 9**: I don't find $Sk_w$ being positive over the desert surprising - it is expected to be positive throughout the CBL. It may be surprising because LES typically give negative values of $Sk_w$ near the surface, which is an artificial result usually due to SGS motions. What is surprising is the difference in sign between the forest and desert, but you've already attributed that to the presence of the canopy;
- **page 11, line 10**: State here as well that the increase in $Sk_w$ is likely due to the height change;
- **page 11, line 10**: 23rd instead of 23th;
- **page 12, line 9**: 23rd instead of 23th;
- **page 13, line 4**: 23rd instead of 23th;
- **page 13, line 23**: This last part is a remnant from the previous version - it should also be removed;
- **Appendix C**: Usually it is considered enough to simply cite the relevant literature when mentioning how the coordinate systems were rotated. No need to completely describe the

workings of planar fit, since it just lenghtens the manuscript unnecessarily;

## References

1. Hogan, Robin J., et al. ”Vertical velocity variance and skewness in clear and cloud-topped boundary layers as revealed by Doppler lidar.” Quarterly Journal of the Royal Meteorological Society 135.640 (2009): 635-643.