Review of "Turbulence characterization in near-coastal environment using triple short-range lidars and mast anemometry"

The authors obtained wind velocity measurements over a large period and by multiple instruments. Of particular interest, is the bowtie-shaped lidar-based velocity measurements. They allow analysis of the theree components of the wind over vertical and lateral separations, which is uncommon. The authors explore single- and two-point statistics and introduce a coherence formulation that includes extra terms to account for well-known limitation of the simple Kaimal spectrum with Davenport's exponential.

The authors do a great job at showing the spectra and coherence estimates of the different instrument and discuss the relevant differences. Overall the manuscript is an important contribution that will hopefully be used as basis for other coherence-related work.

Find some comments below.

Comments

1. The manuscript needs additional clarification in terms of notation used for integral scales. For example, one can use the notation L_w^z to mean the vertical integral length scale, which characterizes how w is correlated over vertical separations. Or, the notation L_w^x, a measure of the w fluctuations along the mean wind direction. This distinction is especially relevant as the authors use frozen turbulence to convert between L_i and T_i using the mean horizontal wind speed. Using Eq 16 as is means one is obtaining the streamwise integral length scale of w, or, L_w^x. I make this comment fully understanding the IEC standards do not make this distinction but I believe the manuscript could benefit from additional clarity.

2. Also on notation, the coherence could use additional symbols to indicate the direction. For example, Figs 15-17 show vertical co-coherence and Fig 19 shows lateral co-coherence, but they all use the same symbol γ_i, with $i = u, v, w$.

3. Figure 9 shows how sensitive the computation of integral length scales L_i^z is to the method chosen. Despite that, the authors continue their analysis using one of those methods – auto-correlation function – without further justification. It is unclear if Figure 10 (for example) and supporting conclusions would remain the same if a different method were to be used.

4. Fig 10(a) shows the integral scales generally increaseing as ζ approaches zero from a negative number – going from a strongly unstable scenario to a weakly unstable towards neutral. That is opposite of what I was expecting, as the more boyancy in the flow, the higher the turbulence production and the larger the integral scales. The authors should mention and offer explanations as to why that was the case. (Is this an artifact of the auto-correlation method chosen?)

5. Fig 10(b): values of L_v are about the same order of magnitude as the values for L_u. Does that hold true for L_u^x and L_v^y? Seems like the SRWS dataset can help answer this question.

6. Line 278: providing vertical profiles obtained with the SRWS scan is not "validation". Using terms like "further validation" also implies the author mean that some sort of validation is provided by recovering typical and expected seasonal and diurnal patterns on the data. As accurately described, recovering these patterns indeed "support[s] the reliability of the stability classification", but does not provide any validation.

7. Fig 15: Just a suggestion: I understand d_z for the sonics and cups completely define the points per earlier description, but it would be nice to also have the $\overline{z}$ value right on the caption here, similar to Fig 16.

8. Line 408: an "increasing stability" is not a well defined term. Consider using "as ζ increases ...". This comment applies to every instance of "increasing stability".

9. Fig 17: why does the scatter get worse as separation distance increase? I understand that N is lower, but is that enough to justify increased scatter? Also - similarly to my comment above, would be helpful to know here if $\overline{z}$ is the same for all these measurements (may be worth stating that in the figure caption).

10. Fig 17: panel (o) looks like the 160 m curve fit has $\gamma_w = 1$ somehow being enforced at $k = 0$. Please check that curve fit. Seems unlikely that the single point at around $\gamma_w = 0.42$ would affect the fit that much (or at least it shouldn't). Overall some fits for 160m are departures from the trend for other separations. I know the sample size is much smaller – if the differences can all be attributed to N, then the authors should consider dropping 160m altogether from the coherence analysis. One could argue it does not provide enough data for reliable estimates.

11. Line 418: The fact that pair of points located at (e.g.) z=55,75 and z=195,215 can be averaged together requires some additional jusitifications. As I was reading through, I noticed Fig 17 could benefit from having the $\overline{z}$ value as well, but this sentence explains why this is not the case. How does the coherence of same d_z but varying $\overline{z}$ compare? I understand Fig 19 tackles part of this question. Fig 19, however, shows several points where the Eq 24 isn't a good fit. Do the points that don't follow the fit have a common pattern/trend to them in terms of location? It's hard to look at Fig 19 and conclude that these pair of points can be averaged over. I think the question of how the vertical coherence for the same separation changes over different mean heights is an important one and this dataset can help answer. Having this discussion would not only enrich the manuscript but also provide (or not provide) the needed evidence for the assumption given on line 418.

12. Fig 18 a-c: As mentioned in the text, a greater scatter is present for the horizontal components. For the sake of argument, let's say that for $d_z/\overline{z} > 0.5$, the curve fit provided by Eq 24 is no longer valid. The points used here range from z=55 to z=215, with a mean of 135m. If we use this mean value, it means that pair of points with $d_z > 67.5$m fall on a region where the model does not perform well. If we get pairs of points that have a lower $\overline{z}$ there is a more significant restriction on the d_z values. I ask the authors consider this and recommend a discussion on the validity of the model and if validity bounds should be given (especially for horizontal wind components).

13. section 4.3.2 and Fig 20: Can this be used to estimate lateral coherence from known vertical coherence? How good would that estimate be? Ultimately, we tend to obtain measureaments on a met-mast type of setup. The current setup investigated in this work is ideal to draw conclusions on how to estimate lateral from vertical.

Minor omments

1. Throughout the manuscript the notation "..." to indicate a range is used (e.g. u = 10...11 m/s). The ellipsis is not a common notation for ranges and I suggest the authors consider using a

different notation.

2. Line 38: Please add a sentence clarifying what is meant by "horizontal asymmetry associated with reduced lateral coherence"
3. Line 46: numerical studies have shown that as well.