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

Overall, the article provides a unique analysis of a rich dataset containing sonic, cup anemometers, and a triple short-range lidar system. It's an excellent contribution to the wind energy community. The article is scientifically sound, easy to follow and comprehensive.

I believe the article is suitable for publication in WES after some minor additions/revisions. Following are some of my observations/comments which can improve the present quality of the article even further:

Specific Comments

1. Line 164. Please report what fraction of samples were excluded by the "stability-classes" criterion. It would also be interesting to show and compare $\zeta(25\,\mathrm{m})$, $\zeta(55\,\mathrm{m})$, and $\zeta(110\,\mathrm{m})$ statistics. Estimate the mismatch between the stability classes at these heights and discuss how this mismatch could affect the vertical coherence results, especially for the largest separations.

2. One of the reasons mentioned by the authors to perform this analysis is to characterize wind turbulence for modern multi-megawatt wind turbines. A modified coherence model is also fitted to the measured vertical and lateral coherence values at multiple separations, including some beyond 100 m. However, there is substantial uncertainty in the coherence measurements for $d_z \geq 90$ m and $d_y \geq 90$ m, which are important separation distances for large wind turbines. Generating synthetic wind fields from this data may yield unrealistic results. Including confidence intervals or uncertainty quantification alongside the fitted model values in the tables in Appendix C can be valuable.

3. Line 401. I am not totally satisfied with the reasoning given for the disparity between $\gamma_{w_{sonic}}$ and $\gamma_{w_{lidar}}$ in Figure 16. Furthermore, the authors dismiss the possibility that this could be due to lidar volume-averaging effects. Please provide more sound, quantitative reasoning for this mismatch.

4. Since the test site is coastal and offshore winds are dominant, some or all of the measurements must be within the internal or transitional boundary layer. The authors have not discussed any impact of the development of the internal boundary layer on the turbulence statistics measured at the site. Please discuss how this affects the turbulence statistics, especially the vertical coherence values. Can the large uncertainty in measured coherences at large separations be explained by the development of an internal boundary layer? Specifically, how does the transition from the marine boundary layer to the inland site modify the integral length scales and the vertical coherence decay coefficients compared to pure offshore sites like FINO1?

Minor issues

1. Figure 3 (b). I am assuming the coordinates should be N pointing up and E pointing right, rather than W. Please correct them.