Review of “Turbulence characterization in near-coastal environment using triple short-range lidars and mast anemometry” by Vogt et al.

This paper presents measurements of turbulence spectra and coherence at a site in North Germany using a met-mast equipped with cup and sonic anemometers as well as with three synchronized ground-based lidars. The authors find that the spectra and coherence from the different instruments agree well with each other. More importantly, they are able to reproduce and verify observations presented in previous publications. They also present an empirical model to capture vertical and lateral coherences that could be useful for generating wind fields along with the IEC Kaimal model.

I think their methods and analysis are scientifically sound, and I would like to praise the quality of presentation of the results. However, I believe a lot more work is needed before this article is ready for publication. My main concern is about the environment, which the measurements are intended to characterize. I have substantiated this below, along with other technical criticisms. I also have some comments regarding the writing, please find them after the “technical comments”.

Technical comments

Abstract: Clear and well-written

Line 61: I do not agree with the claim that this study addresses the limitation of Patel et al. 2026 which lacked information about the vertical component. This is because the measurements in this study are taken onshore (although near the coast) whereas the limitation of Patel et al. stemmed from the difficulty of measuring the w-component offshore using a lidar. Indeed, there is no technically feasible solution for this problem as of yet, so the sentence in Line 61 can be a little misleading.

Line 89, 108 and Figure 1, 2: It is claimed that the sector between 315 to 345° experiences “offshore” flow. But looking at the map of the site in the two figures, I’m not sure if this is the case. It looks like the site is about 2km

inland. Using the rule of thumb that the internal boundary layer (IBL) depth grows as x/10 m where x is the downstream distance from the roughness change. So, in this case, the measurements (which are taken at z = 25 to 125m) would be within the IBL and not really offshore.

I would like to raise another point here which is that I do not understand what wind direction sectors are used to calculate the spectra in Figures 12, 13, 14 and the coherence in Figures 15, 16, 17. Do you use all wind directions apart from the sectors where the measurements are influenced by the turbine or the mast wakes (as I can infer from Table 1)? Then this is not quite correct as I would expect the flow from north-west to have different characteristics than the flow from the eastern sector. Moreover, since the article is intended to characterize "near-coastal" flow, more care should be taken when selecting which wind directions to include in the analysis of spectra and coherence because flow from many directions at this site is probably not coastal. If the required processing of wind directions has already been done, then please explain it in the manuscript as it is not clear in the current form.

Equation 1 (Line 115): How is the azimuth defined? As in which direction corresponds to $\alpha = 0$? In my experience, the azimuth of the instruments is defined with respect to True North and then Equation 1 would contain the east-west, north-south and vertical wind components on the LHS. But you have defined it with the x, y and z components. Since the x-direction in Figure 3(b) is not aligned with True North, it seems to me Equation 1 is incorrect, unless you have defined the azimuth with respect to the x-direction.

Line 124: Why was the 30 min period chosen? I imagine you wanted to analyze the spectra below the commonly used 1/10min frequency, but why did you not go even lower than 30min?

Figure 4: I would suggest making another wind rose showing the directions that you have analyzed in this study.

Line 194-195: Do the authors not correct for the phase delay that comes from the longitudinal separation (as they have done so for the time lag in the scanning pattern in Equation 9)?

Figure 10: I find it surprising that in (a) the length scales are larger during neutral conditions than under very unstable conditions. Can the authors comment on why this might occur?

Line 313: It is not clear what is meant by an ensemble and what samples the ensemble-average is taken over. This is related to my previous comment about **Line 89, 108 and Figure 1, 2**.

Line 372 to 376 and Figure 14: Please mention from which height mesoscale turbulence is visible in the spectra. From (d) and (i) it looks as if the spectra rise at the lowest frequencies even for z = 55m which is surprising.

Figure 19: The measured coherence for the largest separations (120 and 160 m) appears quite noisy for unstable and neutral conditions. I would suggest the authors remove those measurements from subfigures (a), (b), (c), (f), (g), (h), (k), (l) and (m) as it is difficult to conclude anything from them.

Line 402 – 403 and 465 – 466: The primary reason one does not see attenuation is that it cancels out when calculating the coherence as a ratio of the cross-spectra to the auto-spectra. For the smaller separations, the coherence figures show appreciable coherence at normalized wavenumber below 1. So, I believe your argumentation in Lines 402-403 is not completely correct.

Line 494: I believe there is a typo here because this line seems to contradict the statement in line 444.

Line 500: Please mention something about the future research that can be pursued to build on this study. Currently, the conclusions section is devoid of any outlook on future research.

Appendix A: I think this part can be removed because it does not add anything to the analysis. If its purpose is to support the stability classifications, I think that is unnecessary as using sonic anemometers and the Monin-Obukhov length is a well-validated method.

<u>Minor comments</u>

Line 21: One could also mention that stability or non-neutral stratification affects the 2-point statistics such as the coherence.

Line 84: during former measurement campaign → during previous measurement campaigns

Line 85: Weser estuary **along** the North Sea coast

Line 86: "To the north through southeast", please mention in the clockwise or anticlockwise direction

Line 136: 349 to 29°. I see that you define ranges in many places as x … y, please change that to x to y.

Line 195: Please use another terminology for ε as "yaw misalignment" can be confusing to many readers.

Line 354: smaller sample size. However, they seem…

Line 446: The dataset comprises of* 20 months…