Response to the comments by anonymous referee #1 on manuscript WES-2026-100

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

Lennart Vogt, Julia Gottschall, Jasna B. Jakobsen

Note: Line numbers cited by the referee refer to the WESD preprint, whereas line numbers cited in the response refer to the revised manuscript.

1. General comment

Referee comment

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".

We thank the reviewer and appreciate the feedback and constructive comments on our manuscript. We have addressed the individual comments below and updated the manuscript accordingly.

2. Technical comments

Referee comment

Abstract: Clear and well-written

We appreciate the positive feedback on the abstract.

Referee comment

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.

We agree that the lidar setup in the present study technically does not address the limitation of Patel et al. (2026) due to the differences between offshore and onshore environments. We have therefore removed the claim and have updated the manuscript as follows:

Changes to manuscript

Line 62:
In this study, coherence is characterized at a near-coastal site using a triple-lidar system that enables reconstruction of the full three-component wind field [...].

Referee comment

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.

We agree that the terms "offshore flow" and "offshore sector" are not ideal to describe the wind approaching the test site from the estuary and have rephrased the relevant sections.

Changes to manuscript

Line 89:
The Weser estuary lies northwest of the test site, resulting in a sector of approximately 30° where the flow reaches the site after traversing the sea surface.

Line 109:
A wind rose for this period is shown in Fig. 4, indicating both the estuary (315°...345°) and the grassland southwest of the test site (220°...250°) as the prevailing wind directions.

Line 284:
Unstable conditions account for roughly 20 % overall, but increase to about 50 % within the northwestern sector with flow from over the estuary.

Referee comment

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.

The spectral estimates presented in Fig. 12–17 are averaged over all wind directions after excluding sectors potentially affected by turbine or mast wake interference. Prior to ensemble averaging, the auto-spectra are normalized by their respective variances to account for directional inhomogeneities in turbulence intensity.

We agree with the reviewer that the spectral shape itself is also expected to vary with wind direction. To address this point, we have added a new figure (Fig. 15) showing the normalized auto-spectra under neutral conditions for the four wind-direction sectors previously compared in terms of integral length scales. The results are consistent with the length scale analysis, showing increased low-frequency energy for the western and northwestern sectors.

However, the directional dependence is less pronounced than the variation across atmospheric stability, which is the primary focus of this study and the main reason why ensemble averages over all valid wind directions are presented in the remaining figures. As noted by the reviewer in an earlier comment, even the "offshore sector" is influenced by the development of an internal boundary layer, causing a deviation from pure offshore flow. This likely explains the relatively modest differences observed between the spectra obtained for varying sectors.

We acknowledge that averaging across all wind directions combines flow over different upstream terrain conditions. For this reason, we deliberately use the term "near-coastal" rather than "coastal" to describe the site. Wind turbines operating at such locations are exposed to inflow conditions that vary between sectors; however, based on the present analysis, the influence of atmospheric stability on the turbulence characteristics is found to be more significant than the directional variations associated with the differences in terrain.

Changes to manuscript

Added Fig. 15 plus discussion:

Line 397:

The auto-spectral estimates presented in Fig. 12–14 are ensemble averages over all wind directions that are not affected by turbine or mast wake interference. Prior to ensemble averaging, each spectrum is normalized by its corresponding variance to reduce the influence of directional differences in turbulence intensity. However, as shown in Fig. 10d–f, the integral length scales vary with wind direction, indicating that the spectral shape is also expected to differ based on the upstream terrain. This effect is examined in Fig. 15, which presents normalized spectra under neutral conditions for four wind-direction sectors

Changes to manuscript (continued)

with contrasting terrain characteristics. As the upstream roughness decreases from Fig. 15a (residential area) to Fig. 15d (river estuary), the low-frequency energy increases, causing the along-wind turbulence peak around n = 0.1 to gradually broaden into a spectral plateau. This behaviour is consistent with the corresponding increase in the integral length scales discussed previously. A similar increase in low-frequency energy is observed in the cross-wind spectra, whereas the vertical velocity spectra are comparatively insensitive to the upstream terrain. Overall, the sonic anemometer and SRWS spectra exhibit similar trends across the different sectors. Some estimates, however, such as the sonic spectrum at z = 110 m in Fig. 15c, show increased uncertainty due to the smaller number of available samples.

Referee comment

Equation 1 (Line 115): How is the azimuth defined? As in which direction corresponds to α = 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.

We thank the reviewer for this observation. We have added that the azimuth is defined with respect to the x-direction, not to True North.

Changes to manuscript

Line 117:
Here, the azimuth angle is defined with respect to the x-axis in Fig 3b.

Referee comment

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 30 min?

The choice of a 30-minute averaging period represents a compromise between extending the spectral range towards lower frequencies and maintaining the assumption of stationarity. While longer averaging periods would allow to resolve the spectra at even lower frequencies, they also increase the likelihood of changes in atmospheric conditions within a sample, particularly with respect to atmospheric stability. Since one of the main objectives of this study is to investigate the influence of stability on turbulence characteristics, preserving stationarity within each averaging period is an important consideration. A 30-minute averaging period was found to capture the low-frequency motions of interest while providing a substantially larger number of stationary samples than would be obtained using longer averaging periods such as 1 hour.

Referee comment

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

We agree that a wind rose showing only the wind directions included in the analysis would be a valuable addition if the study were restricted to specific sectors. However, as noted in our response to an earlier comment, the analysis includes all wind directions except for the relatively narrow sectors potentially affected by turbine wake or mast interference. Since these excluded sectors differ slightly between the mast and lidar datasets, we believe that the existing wind rose, together with Table 1, provides the necessary information without introducing an additional figure.

Referee comment

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)?

The lateral coherence estimates are indeed corrected for both the phase delay introduced by the scanning sequence and the additional phase delay resulting from the longitudinal separation of the sampling volumes. In Eq. (9), the total time lag τ represents the sum of these two contributions. We understand that this was not sufficiently clear in the original manuscript and have revised the text to explicitly state this.

Changes to manuscript

Line 202:
For lateral coherence, horizontal wind directions non-normal to the bow-tie plane introduce an additional time shift τ_ε due to the longitudinal separation d_x resulting from the yaw misalignment ε. The total time lag τ used for the phase correction is therefore given by the sum of the scanning-induced delay τ_{scan} and the longitudinal delay τ_ε, that is estimated as:

Referee comment

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?

We agree that this observation is not intuitive and suspect that it reflects limitations of Taylor's hypothesis of frozen turbulence under strongly unstable conditions. We have added a short discussion on this to the manuscript:

Changes to manuscript

Line 308:
The dependence of the integral length scales on atmospheric stability and wind direction is illustrated in Fig. 10. Under neutral conditions, the integral length scales are within the ranges of 100 to 250 m for L_u, 30 to 100 m for L_v, and 10 to 50 m for L_w. From neutral to weakly unstable conditions, the length scales increase, consistent with enhanced turbulence generation due to buoyancy. Unexpectedly, this trend does not persist in the strongly convective regime, which may be related to limitations of Taylor's hypothesis of frozen turbulence. The estimation of integral length scales from single-point temporal auto-correlation functions assumes that turbulent structures are advected past the measurement location without undergoing significant evolution. Under unstable stratification, buoyancy-driven turbulence may accelerate the temporal evolution of turbulent eddies, causing the measured auto-correlation functions to decay more rapidly. Consequently, the slightly smaller integral length scales estimated under strongly unstable conditions may not necessarily reflect smaller turbulent structures, but rather indicate departures from Taylor's hypothesis of frozen turbulence.
From neutral to weakly stable conditions, all length scales decrease substantially as buoyancy transitions from enhancing to suppressing turbulence.

Referee comment

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**.

We thank the reviewer for pointing this out. We agree that the definition of the ensemble averages was not sufficiently clear in the original manuscript. As discussed in our response to the previous comment, we have clarified that the ensemble averages are computed from all accepted 30-min samples within the respective atmospheric stability class, excluding only wind directions affected by turbine wake or mast interference. As detailed in the previous response, we have revised the manuscript accordingly and added a discussion of the influence of wind direction, including a new figure showing the directional dependence of the neutral auto-spectra.

Referee comment

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.

The increase in low-frequency energy is indeed observed at all measurement heights, including 25 m and 55 m. Although the effect becomes more pronounced with increasing height, it is not confined to the upper measurement levels. Similar observations were reported by Cheynet et al. (2018) at the FINO1 site, where enhanced low-frequency energy extended down to approximately 40 m. We agree that attributing these motions solely to mesoscale motions is not sufficiently supported by the present measurements. We have therefore revised the relevant sections to use the more general term "low-frequency motions" instead. While mesoscale turbulence is one possible explanation, the observed spectral enhancement may also be associated with other stable boundary layer phenomena, such as buoyancy oscillations near the Brunt–Väisälä frequency.

Changes to manuscript

Line 346:
Simultaneously, low-frequency contributions emerge, likely related to mesoscale motions.

Line 369:
Under neutral conditions, the w spectra follow the basic $f\,S_i(n) \propto f^{1}$ and $f\,S_i(n) \propto f^{-2/3}$ scaling, as neither a plateau nor a low-frequency contribution is present.

Line 391:
The low-frequency contribution captured within the 30-minute averaging window is generally well approximated by the $S_i(n) \propto f^{-5/3}$ scaling.

Line 541:
Under stable stratification, the spectral gap becomes more pronounced as additional low-frequency energy emerges in the horizontal components, potentially related to mesoscale motions.

Referee comment

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.

We agree that the coherence estimates for the 120 m and 160 m separations under unstable and neutral conditions are associated with considerable uncertainty due to the limited number of available samples. To improve the clarity of the figure, we have removed these estimates from the corresponding subfigures and updated the figure caption accordingly.

Changes to manuscript

Updated Fig. 21

Figure 21. Lateral co-coherence of (a) – (e) along-wind, (f) – (j) cross-wind, and (k) – (o) vertical velocity from SRWS for separations ranging from 10 m to 160 m under strongly unstable (left) to strongly stable (right) conditions. The 120 m and 160 m separations are omitted for $\zeta < 0.1$ due to high uncertainty associated with a limited sample size.

Referee comment

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.

We agree that the normalization of the cross-spectra by the corresponding auto-spectra is the primary reason why volume-averaging attenuation has little influence on the coherence estimates. In fact, we have used this argument for both the cup-anemometer (line 417 – 419) and lidar data (line 434 – 436). We acknowledge that it was not stated explicitly here and have revised the manuscript accordingly. Nevertheless, we retain the discussion of the affected frequency range, as it provides an additional justification that any residual influence of volume averaging is expected to be negligible at normalized wavenumbers below 1.

Changes to manuscript

Line 434:
As reported by Cheynet et al. (2016a), a reduction of the lidar-based coherence estimates due to spectral attenuation related to volume-averaging is unlikely because the cross-spectra are normalized by the correspondingly attenuated auto-spectra. Any residual influence is expected to be negligible for normalized wavenumbers below 1, as the attenuation primarily affects frequencies above 0.04 Hz. For a separation of 60 m, these frequencies correspond to $k\,d_z < 1$ only for mean wind speeds exceeding $15\ \mathrm{m\ s^{-1}}$, which are rarely encountered in the present dataset.

Referee comment

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

We thank the reviewer for this observation. This was indeed a typographical error, and we have corrected it in the revised manuscript.

Changes to manuscript

Line 547:
Lateral coherence is systematically higher than vertical coherence, except under neutral conditions where both are of comparable magnitude.

Referee comment

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.

We acknowledge that the conclusions would benefit from a brief outlook on future research. We have therefore added the following paragraph to the end of the conclusions section to highlight potential applications of the present results and directions for future work.

Changes to manuscript

Line 558:
The resulting turbulence and coherence model parameters are provided as functions of atmospheric stability in App. B and App. C, respectively, and may serve as input for the generation of synthetic wind fields under non-neutral atmospheric conditions. Future work could build on these results by investigating how the observed stability-dependent variation in turbulence characteristics influences the dynamic response and loading of wind turbines. In addition, the measured spectra provide a valuable basis for a comparison with the uniform shear model (Syed and Mann, 2024) under different atmospheric stability conditions. Further research could investigate methods for estimating lateral coherence from in-situ measurements of vertical coherence, enabling a more comprehensive characterization of atmospheric turbulence with limited measurement equipment.

Referee comment

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.

We agree that Appendix A is not essential to support the stability classification, which is based on the well-established Monin–Obukhov length estimate from sonic anemometer measurements. As the appendix is not directly used in the turbulence analysis, we have removed it from the revised manuscript.

Changes to manuscript

Removed App. A

Line 81:
The stability-dependent coefficients obtained by fitting empirical models to the turbulence spectra and coherence estimates are provided in App. A and B, respectively.

Line 291:
~~Further validation is provided by comparing mean wind speed profiles derived along the vertical line of the SRWS bow-tie scan (Fig. A1). As expected, stable conditions exhibit the strongest vertical shear, whereas unstable profiles are comparatively uniform.~~

3. Minor comments

Referee comment

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

We agree that atmospheric stability also influences two-point turbulence statistics such as coherence. However, at this point in the introduction the discussion is focused specifically on the development of auto-spectral models under non-neutral atmospheric conditions. Since the influence of atmospheric stability on coherence is introduced shortly afterwards (line 47), we have chosen to retain the current structure to avoid interrupting the flow of the discussion.

We further thank the reviewer for the following comments. We agree with the suggestions and have implemented the corresponding changes in the revised manuscript, as detailed below.

Referee comment

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

Changes to manuscript

Line 85:
[...] during previous measurement campaigns [...]

Referee comment

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

Changes to manuscript

Line 86:
[...] on the east bank of the Weser estuary along the North Sea coast [...]

Referee comment

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

Changes to manuscript

Line 87:
To the north through southeast in clockwise direction, [...]

Referee comment

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

Changes to manuscript

Line 109:
[…] indicating both the estuary (315° to 345°) and the grassland southwest of the test site (220° to 250°) as the prevailing wind directions.

Line 138:
[…] mean wind directions of $\bar{\phi} = 349°$ to $29°$ for the mast sensors and $\bar{\phi} = 309°$ to $69°$ for the SRWS measurements.

Line 144:
[…] the availability is slightly higher (68 % to 71 %) […]

Table 3:
10 to 160

Figure 8 (caption):
Data within 270° to 315° is excluded […]

Line 281:
reaching a maximum of approximately 70 % at $\bar{u} = 10\ \mathrm{m\ s^{-1}}$ to $11\ \mathrm{m\ s^{-1}}$, […]

Figure 23 (caption):
[…] for separations of $d_j = 10\ \mathrm{m}$ to $40\ \mathrm{m}$ and mean heights of $\bar{z} = 120\ \mathrm{m}$ to $130\ \mathrm{m}$.

Line 490:
[…] within the range of $\bar{z} = 120\ \mathrm{m}$ to $130\ \mathrm{m}$ are included.

Line 507:
[…] up to approximately 2 Hz to 6 Hz […]

Line 531:
Under convective conditions, integral length scales of $L_u = 100\ \mathrm{m}$ to $300\ \mathrm{m}$, $L_v = 100\ \mathrm{m}$ to $200\ \mathrm{m}$, and $L_w = 20\ \mathrm{m}$ to $100\ \mathrm{m}$ are observed […]

Line 536:
[…] the integral length scales decrease substantially to approximately $L_u = 40\ \mathrm{m}$ to $110\ \mathrm{m}$, $L_v = 10\ \mathrm{m}$ to $50\ \mathrm{m}$, and $L_w = 4\ \mathrm{m}$ to $30\ \mathrm{m}$, […]

Line 542:
This contribution is also reflected in slightly increased integral length scales of $L_u = 50\ \mathrm{m}$ to $120\ \mathrm{m}$ and $L_v = 15\ \mathrm{m}$ to $60\ \mathrm{m}$.

Referee comment

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

Changes to manuscript

Line 203:
[...] resulting from the wind-direction offset ε.

Line 207:
[...] horizontal wind directions producing a maximum offset of [...]

Referee comment

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

Changes to manuscript

Line 373:
The SRWS spectra appear less smooth due to the smaller sample size. However, they seem to capture the main spectral features, [...]

Referee comment

Line 446: The dataset comprises of* 20 months...

We thank the reviewer for this suggestion. To the best of our knowledge, the original wording ("The dataset comprises 20 months") is grammatically correct, as the verb *comprise* is conventionally used without the preposition *of*.

References

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mikkelsen, T., Sjöholm, M., Mann, J., Hansen, P., Angelou, N., and Svardal, B.: Application of short-range dual-Doppler lidars to evaluate the coherence of turbulence, Exp. Fluids, 57, 184, https://doi.org/10.1007/s00348-016-2275-9, 2016a.

Cheynet, E., Jakobsen, J. B., and Reuder, J.: Velocity Spectra and Coherence Estimates in the Marine Atmospheric Boundary Layer, Bound. Layer Meteorol., 169, 429–460, https://doi.org/10.1007/s10546-018-0382-2, 2018.

Patel, A., Mann, J., Sjöholm, M., Thorsen, G. R., Syed, A. H., Simon, E. I., Hung, L.-Y., and Gottschall, J.: Lidar observations of turbulence for tall offshore wind turbines, J. Fluid Mech., 1028, A9, https://doi.org/10.1017/jfm.2025.11103, 2026.

Syed, A. H. and Mann, J.: A Model for Low-Frequency, Anisotropic Wind Fluctuations and Coherences in the Marine Atmosphere, Bound. Layer Meteorol., 190, 1, https://doi.org/10.1007/s10546-023-00850-w, 2024.

Response to the comments by anonymous referee #2 on manuscript WES-2026-100

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

Lennart Vogt, Julia Gottschall, Jasna B. Jakobsen

Note: Line numbers cited by the referee refer to the WESD preprint, whereas line numbers cited in the response refer to the revised manuscript.

1. General comment

Referee comment

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:

We thank the reviewer and appreciate the positive feedback and constructive comments on our manuscript. We have addressed the individual comments below and updated the manuscript accordingly.

2. Specific comments

Referee comment

Line 164. Please report what fraction of samples were excluded by the "stability-classes" criterion. It would also be interesting to show and compare ζ(25m), ζ(55m), and ζ(110m) 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.

We agree that the fraction of samples excluded by the atmospheric stability criterion is valuable information. We have therefore included the number of remaining samples in Table 1 and expanded the corresponding sections of the manuscript as detailed below.
In addition, we have added Appendix C, which presents the distributions of the stability parameter ζ at the 55 m and 25 m measurement levels. While the distributions exhibit similar trends with respect to mean wind speed, wind direction, month, and time of day, the fraction of near-neutral conditions increases with decreasing height. As discussed in the methodology

section (line 171), the potential influence of this mismatch on the vertical coherence analysis is reduced by excluding samples for which the stability estimates at different heights differ substantially.

Changes to manuscript

Added a row to Table 1, specifying the number of samples after the stability filter.

Updated the values in Fig. 5 and in the last row of Table 1.

Line 66:
In the present work, nearly 500 h of lidar measurements acquired over a five-month period are analyzed [...]

Line 140:
Finally, the datasets are filtered to include only atmospheric stability conditions satisfying $|\zeta| \leq 2$. The estimation of the non-dimensional stability parameter ζ is described in Sect. 3.2.

Line 143:
Prior to applying the atmospheric stability filter, approximately 57 % to 63 % of the sonic anemometer data remain available. Restricting the dataset to $|\zeta| \leq 2$ reduces the availability to 40.0 %, 55.5 %, and 61.5 % for the upper, intermediate, and lower measurement levels, respectively. The reduced availability at higher elevations is primarily due to the more frequent occurrence of stability conditions outside the selected range. Closer to the surface, enhanced mechanically generated turbulence contributes to the higher occurrence of near-neutral conditions, resulting in a larger fraction of data satisfying the stability criterion.
For the cup anemometers, the availability is slightly higher (53.6 % to 65.5 %), [...]

Line 152:
For the SRWS measurements, the final data availability is the lowest (39.1 %), [...]

Line 276:
The corresponding distributions at $z = 55$ m and $z = 25$ m are provided in App. C.

Added App. C.

Referee comment

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 $dz \geq 90$ m and $dy \geq 90$ 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.

We thank the reviewer for this comment. We acknowledge that coherence estimates at the largest separations are associated with increased uncertainty due to the limited number of available samples. Following similar comments from Reviewers 1 and 3, we have removed estimates for which the sample size was considered insufficient, thereby improving the robustness of the presented results. For the remaining separations exceeding 90 m, the estimates remain consistent with the overall coherence trends across separation distance and were therefore retained.

With regard to the fitted model parameters, we agree that an uncertainty quantification would be valuable. However, deriving statistically meaningful confidence intervals is not straightforward in the present analysis because the empirical coherence model is fitted to ensemble-averaged coherence estimates within each stability class rather than to independent realizations. Consequently, the uncertainty associated with the fitted coefficients cannot readily be inferred from the available data and we have therefore not included confidence intervals in App. A and B. Instead, we would like to refer to our response to Reviewer 3 regarding the range of normalized separations $d\,z^{-1}$ over which the fitted coherence model is intended to be applied.

Referee comment

Line 401. I am not totally satisfied with the reasoning given for the disparity between $\gamma_{w,\mathrm{sonic}}$ and $\gamma_{w,\mathrm{lidar}}$ 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.

We agree that the discussion of the discrepancy between the sonic- and lidar-based estimates of γ_w^z can be strengthened. As discussed in the manuscript, we do not consider the attenuation of the lidar spectra to be the primary cause of the reduced coherence. The attenuation is expected to largely cancel when the coherence is calculated as the normalized cross-spectrum. Furthermore, the discrepancy persists at low wavenumbers, where the spectral attenuation is negligible.

A more plausible explanation is that the retrieval of the vertical velocity and the subsequent coherence estimation from the SRWS measurements introduce additional decorrelation effects that are specific to the w component. We have incorporated these potential effects into the revised discussion below.

Changes to manuscript

Line 434:

As reported by Cheynet et al. (2016a), a reduction of the lidar-based coherence estimates due to spectral attenuation related to volume-averaging is unlikely because the cross-spectra are normalized by the correspondingly attenuated auto-spectra. [...]

The velocity reconstruction and coherence estimation based on the SRWS scans may, however, introduce additional decorrelation effects that are specific to the w component. Due to its greater spatial variability within the finite measurement volumes, the vertical velocity is more sensitive than the horizontal components to departures from the assumptions underlying the velocity reconstruction. Furthermore, the correction for the time lag between the vertically separated extraction points relies on Taylor's hypothesis of

Changes to manuscript (continued)

frozen turbulence and is thus expected to be less accurate for the vertical velocity component because of the more rapid temporal evolution of vertical motions. Both effects may contribute to reducing the correlation between the SRWS-derived vertical velocity time series at different heights.

Referee comment

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?

These are important considerations and partly overlap with a comment raised by Reviewer 1. Given the distance of approximately 2 km between the site and the coastline, the measurements are expected to be within the internal boundary layer (IBL) for all wind directions originating from the sea. Following the suggestions of Reviewer 1, we have removed the terms "offshore flow" and "offshore sector" from the manuscript, as even the northwestern sector cannot be regarded as representative of undisturbed offshore conditions.

To further address the potential influence of the IBL, we have added Fig. 15, which compares the neutral auto-spectra for four wind direction sectors characterized by different upstream terrain. The western and northwestern sectors exhibit increased low-frequency energy compared with the inland sectors, consistent with the increase in integral length scales shown in Fig. 10. However, these directional differences are limited, suggesting that the flow has already undergone substantial change within the IBL. Consequently, the integral length scales and coherence remain lower than those reported by Cheynet et al. (2018) for the FINO1 site.

The increased uncertainty in the coherence estimates at large vertical separations is, however, more likely to be a result of the limited number of available samples at these separations, rather than being caused by the IBL, as it persists even when excluding data from northwestern direction.

3. Minor comment

Referee comment

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

We thank the reviewer for this observation. This was indeed a typographical error, and we have corrected the figure in the revised manuscript.

References

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mikkelsen, T., Sjöholm, M., Mann, J., Hansen, P., Angelou, N., and Svardal, B.: Application of short-range dual-Doppler lidars to evaluate the coherence of turbulence, Exp. Fluids, 57, 184, https://doi.org/10.1007/s00348-016-2275-9, 2016a.

Cheynet, E., Jakobsen, J. B., and Reuder, J.: Velocity Spectra and Coherence Estimates in the Marine Atmospheric Boundary Layer, Bound. Layer Meteorol., 169, 429–460, https://doi.org/10.1007/s10546-018-0382-2, 2018.

Response to the comments by anonymous referee #3 on manuscript WES-2026-100

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

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Note: Line numbers cited by the referee refer to the WESD preprint, whereas line numbers cited in the response refer to the revised manuscript.

1. General comment

Referee comment

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 three 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.

We thank the reviewer and appreciate the feedback and constructive comments on our manuscript. We have addressed the individual comments below and updated the manuscript accordingly.

2. Specific comments

Referee comment

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.

We thank the reviewer for this comment. We agree that it is important to distinguish between streamwise, lateral, and vertical integral length scales. In the present work, all integral length scales are derived from single-point time series using Taylor's hypothesis of frozen turbulence and therefore represent streamwise integral length scales. As no lateral or vertical integral length scales are considered, we prefer to retain the simpler notation L_i for improved readability. To avoid any ambiguity, we have added an explanatory sentence around Eq. (16) explicitly stating that L_i refers to the streamwise integral length scale throughout the manuscript.

Changes to manuscript

Line 222:
Under the assumption of frozen turbulence, the streamwise integral length scale can be estimated from a single-point velocity time series as [...]

Line 228:
Throughout this work, L_i denotes the streamwise integral length scale of velocity component i.

Referee comment

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 $i = u, v, w$.

We agree that the notation of the coherence needs clarification regarding the direction of separation. As we are showing both vertical and lateral coherence, we have updated the manuscript introducing the notation γ_{ij}, with $j = \{x, y, z\}$ denoting the separation direction.

Changes to manuscript

Line 194:
In this study, unless stated otherwise, *coherence* refers specifically to the co-coherence γ_{ij}, i.e., the real part of the complex coherence [...] with f denoting the frequency in Hz and $j = \{x, y, z\}$ the separation direction.

Updated notation in Eqs. (20) – (22), (25), and (B1).

Updated axis label in Figs. (16) – (18) and (21).

Referee comment

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.

Figure 9 compares the integral length scales derived from the auto-correlation functions with those obtained from the IEC standard and from fitting the Kaimal spectrum to the measured auto-spectra. Both the IEC standard and the Kaimal spectral model are only applicable to neutral atmospheric conditions and therefore do not provide a basis for estimating length scales consistently across different stability regimes, as required for the subsequent analysis. In contrast, the auto-correlation approach can be applied independently of atmospheric stability, which is why we retain this method only.

Referee comment

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?)

We thank the reviewer for this comment and agree that this observation is not intuitive. As discussed in our response to Reviewer 1, we suspect that this behavior may reflect limitations of Taylor's hypothesis of frozen turbulence under strongly unstable conditions. We have expanded the discussion in the revised manuscript to address this potential effect and to clarify that the observed reduction is not necessarily to be interpreted as evidence of smaller turbulent structures.

Changes to manuscript

Line 308:
The dependence of the integral length scales on atmospheric stability and wind direction is illustrated in Fig. 10. Under neutral conditions, the integral length scales are within the ranges of 100 to 250 m for L_u, 30 to 100 m for L_v, and 10 to 50 m for L_w. From neutral to weakly unstable conditions, the length scales increase, consistent with enhanced turbulence generation due to buoyancy. Unexpectedly, this trend does to not persist in the strongly convective regime, which may be related to limitations of Taylor's hypothesis of frozen turbulence. The estimation of integral length scales from single-point temporal auto-correlation functions assumes that turbulent structures are advected past the measurement location without undergoing significant evolution. Under unstable stratification, buoyancy-driven turbulence may accelerate the temporal evolution of turbulent eddies, causing the measured auto-correlation functions to decay more rapidly. Consequently, the slightly smaller integral length scales estimated under strongly unstable conditions may not necessarily reflect smaller turbulent structures, but rather indicate departures from Taylor's hypothesis of frozen turbulence.
From neutral to weakly stable conditions, all length scales decrease substantially as buoyancy transitions from enhancing to suppressing turbulence.

Referee comment

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.

We agree that the SRWS measurements could in principle be used to estimate integral length scales from lateral and vertical separations. However, in this work we characterize the correlation across these separations in terms of spectral coherence rather than integral length scales. This approach allows the scale-dependent correlation to be assessed across different frequencies and is also directly relevant to the parameterization of coherence for synthetic wind field generation, which is one of the intended applications of the results.

The streamwise integral length scales presented in this study serve primarily to characterize the extent and deformation of turbulent structures and their dependence on atmospheric stability. The ratio of the streamwise integral length scales of the cross-wind and the along-wind turbulence, L_v/L_u, is around 0.75 to 1 under unstable conditions, and decreases to 0.25 to 0.5 in neutral and stable atmopshere, which appear to be reasonable ranges.

Referee comment

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.

We agree that the term "validation" was not appropriate in this context, as the mean wind profiles can only provide supporting evidence for the reliability of the stability classification. Moreover, following the suggestion of Reviewer 1, we have removed the appendix containing the mean wind profiles, as it does not contribute directly to the turbulence analysis and the stability classification based on Monin–Obukhov length estimates from sonic anemometer measurements is a well-established approach.

Referee comment

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

We appreciate this suggestion and have added the average measurement heights to the legend.

Changes to manuscript

Updated Fig. 16.

Referee comment

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".

We agree with the reviewer and have replaced the term throughout the manuscript.

Changes to manuscript

Line 448:
From unstable to stable conditions, coherence decreases, reflecting the reduction in turbulent length scales.

Line 474:
The reduction of turbulent length scales from unstable to stable conditions is reflected in the lateral coherence.

Referee comment

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 z is the same for all these measurements (may be worth stating that in the figure caption).

We thank the reviewer for these comments. Coherence estimates are subject to statistical uncertainty and inherently very noisy. The smoothness observed at smaller separations results from ensemble averaging over a relatively large number of samples. As the separation increases, the number of available realizations reduces substantially, leading to increased scatter.

We agree that the measurement height should be specified when comparing coherence estimates at different separation distances. In the original analysis, the estimates shown in Fig. 18 were obtained by averaging across all heights sampled by the bow-tie scan. As noted in the reviewer's subsequent Comment (#11), this is an oversimplification because coherence may vary substantially with height. We have therefore revised the analysis presented in Figs. 18 and 21 by restricting the ensemble averaging to estimates obtained at comparable heights. The chosen height ranges have also been added to the figure captions.
In addition, following Comment #11, we have added Figs. 19 and 22, which show the vertical and lateral coherence as a function of measurement height for a given separation distance. These figures provide further insight on the dependence of the coherence estimates on height.

Changes to manuscript

Updated Fig. 18 and 21.

Figure 18. Vertical co-coherence of (a) – (e) along-wind, (f) – (j) cross-wind, and (k) – (o) vertical velocity from SRWS for separations ranging from 10 m to 160 m under strongly unstable (left) to strongly stable (right) conditions and average heights of 110 m ≤ z ≤ 140 m.

Changes to manuscript (continued)

Line 453:

To obtain meaningful averages as a function of stability and separation distance, the ensembles are restricted to estimates obtained at comparable measurement heights of 110 m ≤ z ≤ 140 m.

Figure 21. Lateral co-coherence of (a) – (e) along-wind, (f) – (j) cross-wind, and (k) – (o) vertical velocity from SRWS for separations ranging from 10 m to 160 m under strongly unstable (left) to strongly stable (right) conditions and a measurement height of 125 m.

Line 481:

To obtain meaningful ensemble averages, the lateral coherence estimates shown in Fig. 21 are restricted to a measurement height of 125 m, corresponding to estimates obtained along the horizontal segment of the bow-tie pattern.

Referee comment

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 160 m 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.

We agree that some of the curve fits in Fig. 18 are strongly influenced by substantial scatter in the coherence estimates and do not consistently follow the trends observed at smaller separations. As discussed in our response to the previous comment, we attribute this increased scatter primarily to the reduced number of available samples at the largest separations.
While there is no artificial constraint forcing $\gamma_w = 1$, the fit to the limited and highly scattered data produces an unreliable coherence estimate. Following the suggestions of Reviewers 1 and 3, we have therefore removed the 160 m separations for $|\zeta| > 0.6$.

Changes to manuscript

Updated Fig. 18.

Figure 18. Vertical co-coherence of (a) – (e) along-wind, (f) – (j) cross-wind, and (k) – (o) vertical velocity from SRWS for separations ranging from 10 m to 160 m under strongly unstable (left) to strongly stable (right) conditions and average heights of 110 m ≤ z ≤ 140 m. The 160 m separation is omitted for $|\zeta| > 0.6$ due to high uncertainty associated with a limited sample size.

Referee comment

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 z value as well, but this sentence explains why this is not the case. How does the coherence of same dz but varying 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.

We agree that averaging coherence estimates with the same separation distance across substantially different measurement heights is an oversimplification, and that the height dependence should be explicitly examined. As described in our response to Comment #9, we have therefore revised the analysis and restricted the range of measurement heights when ensemble-averaging the coherence estimates presented in Figs. 18 and 21.
To further investigate the dependence of coherence on measurement height, we have added Figs. 19 and 22, which show the vertical and lateral coherence estimates, respectively, for a fixed separation distance as a function of the average measurement height.
We further agree that the deviations from Eq. (24) in Fig. 20 require further discussion on the validity range of the coherence model. We have elaborated on this in our response to Comment #12.

Changes to manuscript

Added Fig. 19

Line 457:
Additionally, coherence is affected by the measurement height, which is not accounted for by the Davenport model. The height dependence of the vertical coherence is examined in Fig. 19 for a fixed separation distance of $d_z = 160\ \mathrm{m}$ and neutral conditions. A reduction in coherence with decreasing height due to surface effects is observed for all wind components, while being most pronounced for the vertical velocity. The coherence of the along-wind turbulence appears to be least sensitive to $\bar{z}$ and exhibits a noticeable dependence only at normalized wavenumbers $k\, d_z > 0.25$.
To account for the combined influence of measurement height and separation distance, the model coefficients A and C in Eq. (22) are fitted for a wide range of combinations across the bow-tie pattern. The resulting coefficients are shown in Fig. 20 as a function of the normalized separation $d_z\, \bar{z}^{-1}$.

Added Fig. 22

Line 482:
For lateral separations between 10 m and 90 m, one additional estimate is available both above and below this height from the diagonal segments connecting to the vertical line, as

Changes to manuscript (continued)

illustrated in Fig. 6. For $d_y = 60\ \mathrm{m}$, these estimates are located at $\bar{z} = 70\ \mathrm{m}$ and $\bar{z} = 192\ \mathrm{m}$, and are compared to the $\bar{z} = 125\ \mathrm{m}$ estimate in Fig. 22.
Although only three measurement heights are available and the estimates at the lowest and highest elevations are associated with relatively large uncertainty, a reduction in lateral coherence with decreasing measurement height is evident. Consistent with the vertical coherence results (Fig. 19), the height sensitivity is most pronounced for γ_{wy}. At $\bar{z} = 70\ \mathrm{m}$, the vertical velocity is quasi incoherent across the $d_y = 60\ \mathrm{m}$ separation.

Referee comment

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 dz/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 dz > 67.5m fall on a region where the model does not perform well. If we get pairs of points that have a lower z there is a more significant restriction on the dz 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).

We agree that the increased scatter at large normalized separations observed for the horizontal components, warrants a clearer discussion of the validity range of the proposed coherence model. Following the reviewer's suggestion, we have expanded the discussion in the revised manuscript to explicitly state the proposed validity bound of $d_j\,\bar{z}^{-1} \leq 0.5$.

Changes to manuscript

Line 467:
The exponential formulation in Eq. (24) provides an accurate fit to A_w. Greater scatter is observed for the horizontal components, particularly for $d_z\,\bar{z}^{-1} > 0.5$.

Line 550:
By introducing exponential relationships between the model coefficients and the normalized separation $d_j\,\bar{z}^{-1}$, a three-parameter coherence model is established.
While these exponential relationships provide accurate fits to the model parameters for the vertical velocity, increased scatter and deviations are observed for the horizontal components at $d_j\,\bar{z}^{-1} > 0.5$. The reduced accuracy at large normalized separations can primarily be attributed to the low data availability resulting from the limited size of the SRWS bow-tie pattern. The available measurements therefore support an application of the proposed coherence model for normalized separations of $d_j\,\bar{z}^{-1} \leq 0.5$. Additional measurements at larger separations are recommended for assessing and improving the model accuracy for the horizontal velocity components beyond this range.

Referee comment

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.

We appreciate this comment and agree that estimating lateral coherence from vertical coherence would be a valuable application of the present dataset, particularly because vertical coherence can be obtained from conventional met-mast measurements more readily than lateral coherence. The comparison of the coherence model parameters for vertical and lateral separations in Fig. 23 provides a first indication of the relationship between the two and could potentially be used as a basis for such an approach. However, a thorough study of this relationship would require including additional separation distances and measurement heights than the ones shown in Fig. 23. Such a comprehensive study is beyond the scope of the present analysis but represents a very promising direction for future work. We have therefore added this aspect to the outlook of the manuscript.

Changes to manuscript

Line 562:
Further research could investigate methods for estimating lateral coherence from in-situ measurements of vertical coherence, enabling a more comprehensive characterization of atmospheric turbulence with limited measurement equipment.

3. Minor comments

Referee comment

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.

We thank the reviewer for pointing this out. We have replaced the ellipsis ("…") throughout the manuscript by the word "to". For a detailed list of the resulting changes, we refer to our response to Reviewer #1, who raised the same issue.

Referee comment

2. **Line 38:** Please add a sentence clarifying what is meant by "horizontal asymmetry associated with reduced lateral coherence"

In this context, the term "horizontal asymmetry" refers to spatial differences in wind speed in lateral direction across the rotor associated with reduced coherence. We have clarified this in the revised manuscript.

Changes to manuscript

Line 38:
The horizontal asymmetry in wind speed across the rotor associated with reduced lateral coherence in the uniform shear model increases tower torsion, [...]

Referee comment

3. **Line 46:** numerical studies have shown that as well.

We agree with the reviewer and have added the suggestion to the manuscript.

Changes to manuscript

Line 47:
Field measurements and numerical studies indicate that spectral coherence varies across wind components, separation distance and direction, measurement height, and atmospheric stability.