Can the Mann model describe the typhoon turbulence?

Reviewer's comments

1 General comment

The manuscript "Can the Mann model describe typhoon turbulence?" by Müller et al. examines the applicability of the uniform shear model (Mann, 1994), also known as Mann's model. The study addresses an important topic in wind energy science that deserves attention. While the paper is of broad international interest and falls within the scope of Wind Energy Science (WES), it could benefit from a clearer explanation of its relevance to the field. The analysis and methodology are rigorous, though some aspects may require improvement. The conclusions are generally well-supported by the results, except, perhaps, for the case of Typhoon Nuri in the back eyewall region (Figure 10 in the paper). I recommend a major revision. I am confident that the authors will be able to address my feedback, comments, and questions adequately. Below, I outline a few key points that I believe deserve further attention.

1. The study does not clearly discuss atmospheric stability, which is an important aspect of typhoon winds. It is likely that the stability in the four case studies is near-neutral or slightly convective. The shape of the turbulence spectra can change susbtantially as soon as the atmosphere become slightly convective. Since the uniform shear (US) model (Mann, 1994) is designed specifically for neutral atmospheric conditions, it may not adequately capture turbulence eddies generated by buoyancy forces. The unexpectedly high energy observed in the low-frequency range may be attributed to turbulence generated by convection rather than mesoscale fluctuations, although both processes could be found jointly. Addressing these aspects will provide a more complete characterization of the turbulence conditions in typhoon winds. As you may already know, the atmospheric stability can be estimated using the Obukhov length via the eddy covariance technique, which requires sonic temperature measurements and all three velocity components from 3D ultrasonic anemometers.

2. The scientific literature indicates that extreme wind loading leading to wind turbine structural collapse is often associated with non-stationary and non-Gaussian typhoon winds, particularly rapid changes in wind direction and speed. The present study focuses on stationary turbulence, which is a reasonable approach but also a choice that limit the scope of the study. Non-stationary typhoon winds have been explored in wind engineering literature, particularly since the 2000s. To better position the study within the wind energy and wind engineering it may be helpful to clearly state and discuss the place of the present study within the framework of stationary, Gaussian, homogeneous turbulence, from which the Mann model is based on.

3. It would be valuable to include an analysis of the skewness and kurtosis of velocity fluctuations to address the following question: Are (stationary) typhoon winds Gaussian? The study by Cao et al. (2009) suggests that the answer is "yes," but it would be valuable to see what findings emerge from the present study. This question is particularly relevant for wind turbine design, as non-Gaussian winds can lead to larger extreme wind loadings compared to Gaussian winds—a key assumption widely used in IEC standards and elsewhere. Such an analysis would be within the scope of the present study.

4. The study presents four key findings. However, finding number 4 appears to have been previously documented in Peña et al. (2010) or de Maré and Mann (2016) for non-typhoon winds, so its inclusion may not add significant new insights. Additionally, finding number 2 may require some revision, particularly in relation to atmospheric stability.

5. The study uses some dichotomous expressions that may be perceived as mutually exclusive by researchers in wind engineering and boundary-layer meteorology—fields that form the theoretical foundation for turbulent loading on wind turbines. To avoid ambiguity and potential misunderstandings, it may be helpful to elaborate on the use of these terms in both mesoscale and microscale meteorology. For example, the term "mesoscale turbulence" is used, but turbulence is often defined as wind fluctuations occurring at scales smaller than the mesoscale. To enhance clarity, a possible approach would be to use the term "mesoscale fluctuations" instead, as by Högström et al. (2002). A brief discussion of terminology could help ensure consistency and improve the study's readability for a broader audience.

6. In the main results, particularly in Figures 8–10, the vertical spectrum exhibits unusual behaviour in the inertial subrange. In this range, the ratios F_w/F_u and F_v/F_u would typically be expected to converge toward 1.33 under the assumption of local isotropy, or at the very least, remain above 1.2. However, in several measurements, this is not the case, and for Typhoons NURI and HAGUPIT, F_w/F_u is even observed to be less than 1. This suggests the presence of significant flow distortion, the well-documented "w-bug," mast shadowing effects, or a combination of these factors. In contrast, for Typhoon PRAPIROON, the ratio F_w/F_u appears to be close to 1.3 in the inertial subrange (Figure 9), which is an encouraging result. Also, the positive spectral slope observed in the inertial subrange for NURI (Back Inner and Eyewall) is indicative of an unphysical signal. Further investigation of these cases is needed, followed by a reassessment of the US model fit after conducting an in-depth quality check. Addressing these issues could potentially impact some of the study's key findings, including disregarding some of the data from typhoon NURI, which seems of lower quality than the other masts.

2 Specific comments

Point 1

Introduction: I think that a more specific and direct link to the design of wind turbines in typhoon-prone regions could strengthen the introduction. This would help highlight the relevance of the topic. One possible way to enhance this aspect is by referencing recent events, such as the collapse of multiple wind turbines during Typhoon Yagi (Sanderson, 2024).

Point 2

Introduction: The literature review appears somewhat incomplete. The manuscript seems to align well with previous studies advocating for modifications to the IEC standard to account for extreme wind loading from typhoons (Chen and Xu, 2016). Also, it appears to complement nicely the findings of Cao et al. (2009), which suggest that the turbulence characteristics of typhoon winds closely resemble those of non-typhoon winds. There may be other studies of interest. Previous studies on typhoon winds for wind turbine design have not focused extensively on the Mann turbulence model, but Han et al. (2014) mention it briefly in their study. There are probably many more studies on Typhoon wind for wind loading on structures (bridges and tower). Maybe a summary table can be used? For example:

Table 1: Summary of studies on typhoon winds and their characteristics

Study	Stationary/Non-stationary	Gaussian/Non-Gaussian	Structure Type	Turbulence Model
Present study	Stationary	—	Wind Turbine	Mann
author et al (year)	Non-stationary	Non-Gaussian	Bridge	-
author et al (year)	Stationary	Gaussian	Wind Turbine	Mann Model
author et al (year)	Non-stationary	Non-Gaussian	Tower	Kaimal Spectrum
author et al (year)	Non-stationary	Non-Gaussian	Bridge	ESDU Model
author et al (year)	Stationary	Gaussian	Wind Turbine	IEC Kaimal Model

Point 3

Page 2, lines 33–34: The interpretation of sea spray in this context seems somewhat unusual. While it is reasonable to mention it, a positive slope in the inertial subrange of the normalized spectra is, in my experience, typically indicative of noise. This can arise from various sources, such as rain causing artificially high velocity readings in sonic anemometer data, tower shadowing effects, or aliasing. The correct reference might be Li et al. (2015) rather than Li et al. (2012). Overall, the unusual behavior observed in the inertial subrange may not be physical but rather a reflection of instrumental errors. Sonic anemometers are known to perform poorly under heavy rain or when exposed to water spray, which could explain this anomaly.

Point 4

Page 2, line 47: The Kaimal model used in the IEC standard differs significantly from the original model by Kaimal et al. (1972)—so much so that referring to it simply as Kaimal may be misleading. It might be more appropriate to refer to it as IEC-Kaimal to distinguish it from the original formulation. Alternatively, citing the IEC standard directly, rather than the original paper by Kaimal et al. (1972), could provide better clarity.

Point 5

Page 2, line 52: Stating that the Mann model fails to account for mesoscale fluctuations may not be accurate, as it does not attempt to model them in the first place. A more precise phrasing would acknowledge that the Mann model is designed specifically for turbulence and does not incorporate mesoscale fluctuations by definition.

Point 6

Page 2, lines 54–55: The reference to inactive turbulence in Högström et al. (2002) could be misleading. If I remember properly, the authors actually argue against using this term. What they describe as inactive turbulence still falls within the definition of turbulence and should not be conflated with mesoscale motion. It may be useful to clarify this distinction to avoid potential misinterpretations.

Point 7

Pages 2–3, lines 53–63: The terminology used in this paragraph appears to conflate mesoscale fluctuations with turbulence, which may lead to conceptual ambiguities. The distinction between mesoscale and turbulent motions is well-established in atmospheric science. For instance, Högström et al. (2002) describe mesoscale fluctuations as "unsteady quasi-two-dimensional motion," emphasizing that they are non-turbulent. Typically, mesoscale fluctuations lie on the left side of the spectral gap, while turbulence is on the right. The spectral gap, which separates these two scales, is a key feature of atmospheric turbulence spectra. Under convective conditions, this gap may become less distinct or even undetectable due to buoyancy-generated turbulence overlapping with mesoscale motions. However, referring to these large-scale fluctuations as mesoscale turbulence may be misleading. It would be beneficial to clarify this distinction to ensure the terminology aligns with established turbulence theory. Specifically, rather than mesoscale turbulence, a more precise term might be mesoscale fluctuations or mesocale motion.

Point 8

Table 1: Many Gill WindMaster Pro anemometers produced between 2006 and 2015 were affected by a known issue that led to an underestimation of the vertical wind component. See, for example, `https://www.licor.com/support/EddyPro/topics/w-boost-correction.html`. Would it be possible to verify whether this issue affected the instruments used in this study? If so, the bias can be corrected (to some extent) using a straightforward data processing method, as described in the linked resource.

Point 9

Line 81: Could a brief explanation be provided for the choice of the 6 km area? If this selection is related to internal boundary layers, would it be possible to use a simple analytical model to estimate the internal boundary layer thickness? Garratt (1990) presents several relevant models that might be useful. If such an approach is considered, specifying the roughness length for the sectors of interest would further clarify the reasoning behind the choice.

Point 10

Spike Filtering: It may be beneficial to first apply a flat threshold, such as 65 m/s, which is the upper measurement limit of the Gill sonic anemometer. The reason for this is that spikes can exceed this value, potentially masking other outliers. A possible approach could be: (1) Apply a flat threshold to remove physically unrealistic values. (2) Perform outlier detection using a moving median filter. It is important to use the absolute median deviation (MAD) rather than the absolute mean deviation, as recommended by Leys et al. (2013). It is currently unclear whether the study employs the median or arithmetic mean for outlier detection. The wording "average" suggests the latter, but clarification would be helpful.

Point 11

Data Processing and Data Filling: The use of linear interpolation for datasets with 15% missing values (NaNs) raises some concerns, particularly for turbulence studies, where preserving statistical properties is crucial. In atmospheric science, a common threshold for acceptable missing data is around 5%. How would the findings be affected if a stricter threshold were applied, such as 10% or 5% NaNs? Exploring the sensitivity of the results to different thresholds could help assess the robustness of the analysis.

Point 12

Section 2.1.2 – Power Spectra Calculation: The approach described in this section closely resembles Welch's method (Welch, 2003), which is a well-established technique for power spectral estimation. To avoid unnecessary reinvention, it may be beneficial to explicitly state that Welch's method is being used and to reference the appropriate implementation, such as the scipy.signal.welch function in Python or the pwelch function in MATLAB. In this study suggest using around 3 segments with 50% overlapping to reduce uncertainties. Reformulating this section to reflect this could improve clarity and align the methodology with standard signal processing practices..

Point 13

Section 2.1.2 – Stationarity Test: Using wind direction change as a criterion for stationarity is a good idea. However, if the goal is to analyze stationary fluctuations specifically, it may also be useful to check stationarity in mean wind speed (first-order stationarity) and variance (second-order stationarity). While this might not be strictly necessary given that the results appear reasonable, performing these additional tests could provide a more rigorous assessment for future studies.

Point 14

Line 154–155: A clearer formulation might be to state that a key advantage of the Mann model is that the second-order structure of homogeneous atmospheric turbulence in a neutral atmosphere is incorporated using only three parameters. The limited number of parameters is a significant advantage, particularly for wind energy applications, where simplicity and computational efficiency are crucial.

Point 15

Line 180–181: The authors raise an important point: the study focuses on stationary turbulence. This distinction should be explicitly mentioned in the abstract, as many readers might initially expect the paper to address non-stationary turbulence for Typhoon winds.

Point 16

Line 180–181 and lines 224: The claim that significant wind direction changes make the Fourier transform "invalid" is not accurate in my opinion. The Fourier transform remains valid for both stationary and non-stationary signals because it preserves phase information. However, the power spectral density (PSD), which discards phase information, is primarily suited for stationary signals. In strongly non-stationary conditions, such as in the eye and rainband regions of a tropical cyclone, the PSD may not accurately represent the underlying turbulence characteristics. A more precise formulation could be to state that the spectral analysis is not performed in the eye and rainband regions due to the strong non-stationarity of the wind field, which complicates the interpretation of the power spectra.

Point 17

Figures 4-7: The figures are well-presented. However, why are the Mann parameters shown only in Figures 4-5 and not in Figures 6-7? If there is a reason for this omission, it would be helpful to clarify it.

Point 18

Figures 4-5: As noted by the authors, some fits have reached the upper boundary for Γ and L. This suggests that the fitting procedure may not have converged. While such cases can occur, it might be beneficial to disregard those samples (if this has not already been done) or explore whether a better initial guess could improve the fitting.

Point 19

Mast shadowing: During the passage of a typhoon, wind direction can change dramatically, which may lead to periods where the sonic anemometer data are affected by mast shadowing. To ensure a meaningful analysis, it would be useful to provide the boom orientation and indicate whether and when the data are impacted by mast shadowing. If data are found to be affected, they should be excluded from the analysis.

Point 20

Lines 267-268: This is a good observation. As noted in the manuscript, the length scale parameter (L) tends to increase with height above the surface. This trend has been documented in previous studies. Also, it is generally expected that L will increase substantially under convective atmospheric conditions, as discussed in Sathe et al. (2013).

Point 21

Lines 272-273: The variation of $\alpha\varepsilon^{2/3}$ is influenced not only by the mean wind speed but also by the variance of the velocity components. The current statement suggests a dependence solely on the mean wind speed, but it would be more accurate to acknowledge both factors.

Point 22

Figure 10: Could the secondary peak at $k > 1 \times 10^{-1}$ m^{-1} be related to the first eigenfrequency of the mast? If so, it may be useful to investigate whether structural resonance effects influence the spectral shape.

Point 23

Line 355: Good observation. It is possible that many turbulence models used for wind loading underrepresent low-wavenumber variability in the v-component. However, I ignore what could be the physical interpretation of this variability. Boundary layer rolls, as proposed in this study seems to be a reasonable interpretation.

Point 24

Line 342-344: This statement could be rephrased with more caution. Some of the observed spectral peaks do appear to have a physical origin—for instance, the secondary peak in the F_v-spectrum for the Back Outer region is consistent with trends previously observed in the literature. However, other peaks, such as those for the Front Outer (FO) region, may be influenced by large uncertainties in the lowest-frequency bins due to the use of the modified periodogram method.

It is important to remember that power spectral densities (PSDs) are statistical estimates and inherently contain uncertainties that increase as the wavenumber decreases. The spectral peaks observed at $k < 3 \times 10^{-3}$ m^{-1} may be particularly affected by these uncertainties. Ideally, a larger sample size would help determine whether these peaks have a physical basis, but obtaining additional typhoon wind data is understandably challenging. An alternative approach could be to use a greater number of overlapping segments (with at least 50% overlap) in the Welch algorithm to reduce spectral uncertainties and smooth out noise in the lowest frequencies.

Point 25

Spectrum shape: Hagupit's spectrum shows a flattened spectral peak, which raises the question of whether ground blockage effects contribute to this flattening. A potential way to assess the impact of ground blockage is to analyze the imaginary part of the cross-spectrum between u and w, as suggested in Mann (1994).

Point 26

Lines 346-348: The presence of nonzero covariance $\overline{v'w'}$ in offshore conditions has been documented since the 1980s (see Geernaert (1988)), so this phenomenon is now well established. Maybe these observations should not be described as non-classical turbulence behaviour. Given that the measurements in this study are from coastal or offshore regions, observing significant values in the F_{vw} co-spectrum is expected. It may be useful to mention this as a reminder for readers interested in offshore wind turbine design.

Point 27

Lines 350-351: This sentence may need to be rephrased after conducting a more in-depth literature review. While there have been numerous relevant studies, many of them belong to the field of wind engineering, which, somewhat surprisingly, is often overlooked by the wind energy community. This study presents an opportunity to highlight the overlap between these two fields and possibly propose a path toward a unified description of turbulence in tropical cyclones for wind loading on structures.

Point 28

Section 4, paragraph 1: The first paragraph of this section reads more like a conclusion. It could either be removed or merged into Section 5 (Conclusion).

Point 29

Line 359; The Mann model has, to my knowledge, been tested against actual measurements in typhoon winds (see Han et al. (2014)), but only in a superficial manner. In contrast, this study provides a much more detailed analysis, making it both valuable and complementary to previous work.

Point 30

Discussion section: It may be useful to discuss how this study fits within the broader framework of stationary vs. non-stationary typhoon wind models. The absence of the Mann model in studies of non-stationary turbulence is reasonable, as it was not designed for such conditions. However, most wind-induced damage to wind turbines during typhoons is likely caused by extreme, non-stationary gusts. Future studies might explore ways to combine the Mann model with non-stationary models, for example, by incorporating a fluctuating mean wind speed and direction. This could be briefly outlined in the discussion as a potential avenue for further research.

Point 31

Table 3: This is an informative table, but the comparison of spectral slopes may be misleading, as some slopes in the inertial subrange appear to be spurious. This could mislead readers and practitioners into developing inappropriate wind loading models. Instead of presenting the slope as a result, it may be more appropriate to treat it as a quality check for the data. The last column, which presents the logarithm of spectral peaks, is particularly interesting. It could be beneficial to expand on this aspect.

Point 32

The term "excessive energy" may need to be changed into something like "energy larger than predicted by turbulence models designed for a neutral atmosphere". The energy in the u and v components is only excessive if it results from an error. Offshore measurements consistently show that these components exhibit higher energy at lower wavenumbers than predicted by the Kaimal model or Mann model. This effect appears to be even more pronounced for typhoon winds, though it may also be influenced by convective conditions. The literature documents a substantial increase in energy of the low-frequency F_u and F_v spectra when the atmosphere becomes slightly convective. This suggests that the observed energy enhancement in the low-wavenumber range is more likely linked to buoyancy effects rather than purely mesoscale fluctuations.

Point 33

Line 374-376: The interpretation that the increased energy at lower wavenumbers is due to mesoscale fluctuations may be criticized. The time and spatial scales used in this study—30-minute averaging with PSD estimation using overlapping segments (potentially reducing it to 10-minute windows)—indicate that the observed structures still fall within the turbulence regime. In fact, in multiple instances within this study, the spectral gap is only just becoming visible, reinforcing the idea that the large eddies observed here are still turbulence—though likely strongly anisotropic or influenced by buoyancy effects. Also, the atmosphere might be slightly convective, which would amplify turbulence through buoyancy-generated eddies, an effect not accounted for in the Mann model. Since the Mann model was developed specifically for neutral conditions, deviations from the model in the low-frequency range under stable or unstable stratification are expected.

In cases where the spectral gap is not visible within a 30 min window (e.g. NURI, Front Outer), the observed spectral behavior is more likely to be convective turbulence than mesoscale fluctuations. Under convective conditions, large turbulence eddies generated by buoyancy can fill the spectral gap, creating a transition zone where turbulence and mesoscale fluctuations may overlap. However, identifying this overlap reliably requires longer averaging times—likely closer to 1 hour or more rather than the 30-minute windows used in this study. If the spectral gap remains invisible even with 1+ hour of data, the observed variability may represent a mixture of large turbulence eddies and mesoscale fluctuations. In such cases, distinguishing between the two might require a turbulence model that explicitly accounts for unstable atmospheric stratification.

Point 34

Lines 432–440: The statements in this section may be controversial. Aeroelastic codes do not necessarily require spatial series. Since the dynamic response is computed in the time domain by solving the equation of motion, the required input consists of time series, not spatial series. Additionally, this paragraph seems to paraphrase the assumption of stationarity, and I am unsure if it adds substantial value to the paper. A more straightforward approach would be to state that if the assumption of stationarity holds, the methods used in IEC standards can be applied. If it does not hold, a different framework is required. More generally, IEC standards are based on the assumption of stationary, homogeneous, Gaussian, and ergodic turbulence. However, when this assumption is violated—as is often the case for typhoon winds—a different framework must be adopted. This alternative framework is commonly referred to as non-stationary turbulence. Furthermore, non-stationary turbulence frequently exhibits non-Gaussian and non-homogeneous characteristics, as turbulence that deviates from stationarity often displays non-Gaussian properties. This is particularly relevant because non-Gaussian wind loading is known to produce much higher extreme loads than Gaussian wind loading (Gong and Chen, 2014). To my knowledge, this alternative framework has not yet been clearly established in wind energy research. However, it has been extensively studied in wind engineering for bridge design and high-rise building design since the early 2000s.

Point 35

Lines 447–451: The critical role of coherence in wind loading has been well recognized since the 1960s (see Davenport (1961)). Therefore, citing additional foundational studies rather than relying solely on Dimitrov et al. as a reference may be a good idea. From the results in Mann (1994) and Cheynet (2019), it appears that Mann's model tends to overestimate the coherence of the u-component at vertical separations. This behavior may also hold for stationary typhoon wind conditions, but further investigation would be needed to confirm this. Previous studies indicate that Mann's model may provide reasonable estimates of turbulence coherence at short lateral separations, but there is a knowledge gap for the case of large lateral separations.

Point 36

Conclusions: The conclusion may need to be reformulated and be more closely related to wind energy science.

For points 1, it could be a good idea to further elaborate on two key aspects. First, there are large uncertainties at low wavenumbers in the spectra, which should be acknowledged more explicitly. Second, the possible unstable stratification of the atmosphere implies that turbulence eddies generated by buoyancy may be contributing to the observed spectral features. This is not accounted for in the Mann model, as it was developed specifically for neutral atmospheric conditions. The approach using mesoscale simulations could help assess to what extent mesoscale fluctuations and turbulence interact within a 30-minute window, which could be an interesting topic for future research.

Maybe point 2 can be removed from the findings. Instead, the following points could be added: (i) There are regions within a typhoon where the wind can be described as stationary. In these cases, the Mann model provides a reasonable approximation of the turbulence structure, which is a valuable result. (ii) The spectral peaks of the *u*- and *v*-spectra occur at much closer frequencies in this study than predicted by the Mann model. This raises the question of whether this discrepancy is related to a non-neutral atmospheric state or typhoon wind characteristics. This could be explored further.

Point 4 might be removed from the key findings. Similar findings have already been reported in non-typhoon conditions. Since this behavior is already well documented, it may not be considered as a "new finding". For point 5, the behavior described has been well known since the 1980s for offshore wind and is expected to apply to typhoon winds in coastal regions as well. Given its well-documented nature, it may not need to be emphasized as a key new finding.

3 Technical comments

Point 1

On the use of $10^{-2.5}$ m^{-1}: writing 3×10^{-3} m^{-1} is easier to read than $10^{-2.5}$ m^{-1}.

Point 2

Spelling convention: Since Copernicus Publications is a European publisher, it may be preferable to use British English rather than American English for consistency with their style.

Point 3

Page 1, line 9 – Use of "excessive energy": The term "excessive energy" may not be the most appropriate choice, as the model underestimates the observations. A more precise phrasing, such as "more energy than predicted," could better reflect the context.

Point 4

Section 3.3: The title of the subsection may be renamed or adjusted using an question mark "?"

Point 5

The units of the spectra kF_i, where $i = u, v, w$, should be either in m s^{-1} or m s^{-2} Hz^{-1}, given that F is expressed in m^2 s^{-1} and k is in m^{-1}. However, Table 3 (and possibly other sections) lists the units as m^2 s^{-2}, which appears inconsistent. It would be useful to verify and correct the units where necessary.

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