Impact of atmospheric turbulence on performance and loads of wind turbines: Knowledge gaps and research challenges

Etienne Cheynet

1 General comment

This manuscript addresses a highly relevant topic. It touches on many critical aspects of turbulence modelling and measurement in wind energy, and the authors should be commended for the breadth of the material reviewed. The feedbacks below are offered in a constructive spirit, with the aim of supporting the authors in refining an already valuable contribution. They are intended as suggestions only, and the authors should of course feel free to address them as they see fit. Loads can refer to either structural loads or electrical loads. Hereinafter, I focus solely on wind loading in the context of structural dynamics, which is where my expertise lies.

Overall, the manuscript may benefit from a more explicit, structured narrative built around the limitations of the stationary, Gaussian, homogeneous, ergodic turbulence (SGHET) framework. This framework still underpins many aspects of wind turbine design. Many of the challenges and knowledge gaps mentioned in the manuscript are valuable but may be more clearly framed as consequences of this prevailing paradigm.

2 Specific comments

Point 1

Definition of turbulence: It may be worthwhile to provide a clear definition of turbulence in this section. In wind turbine design and micrometeorology, turbulence is typically understood as three-dimensional and is clearly delineated by the spectral gap; motions on the low-frequency side of this gap are generally considered “non-turbulent.” However, in mesoscale meteorology, such non-turbulent motions are sometimes described as two-dimensional turbulence. These differing definitions may create confusion for readers from diverse research backgrounds. It could be useful to clarify whether the analysis follows one convention or recognizes both. This could be done by referencing established literature or explaining how each definition applies within the context of the study.

Point 2

Abstract: The sentence “Large-scale atmospheric circulations modulate the boundary layer turbulence, characterized by coherence and intermittence” appears to combine concepts that are not typically treated within the same theoretical framework. For example, coherence ($\neq$ coherent structure), a two-point statistical measure, used in signal processing, is typically applied under assumptions of stationarity and homogeneity. However, "intermittence" refers to transient events with sometimes extreme deviations from stationarity. These concepts do not comfortably coexist, and their combination here appears arbitrary. A clearer and safer formulation might be: “*Large-scale atmospheric circulations and Earth surface characteristics modulate boundary layer turbulence.*”

Point 3

Manuscript organisation: The manuscript might benefit from a clearer structure based on the SGHET framework (stationary, Gaussian, homogeneous, ergodic turbulence), which has been used for wind turbine design. Sections 3.1 to 3.7 largely fall within this framework and could be presented as representing the current paradigm. Sections 3.8 and 3.9 begin to move beyond SGHET assumptions, but the transition is not clearly marked. Section 4 combines both SGHET-compatible and non-SGHET phenomena without always distinguishing between them. Section 4.2 is particularly interesting, as it addresses sub-mesoscale motions that lie outside the SGHET framework and are not accounted for in the idealized picture of a spectral gap separating microscale turbulence from mesoscale flows. This spectral gap is not always observed, suggesting potential coupling between scales. This coupling is often neglected in structural loading, but one that could be important for large offshore wind turbine design. Framing the manuscript around a progression from the SGHET framework to its limitations and potential extensions could improve clarity.

Point 4

Section 3.7: Spectra and coherence Within the SGHET framework, one-point spectra and coherence are the two main turbulence statistics — and arguably the only ones needed for wind load modelling. Indeed, turbulence intensity and integral length scales can be retrieved from the wind spectra. While there has been extensive work on modelling one-point spectra over the past sixty years, the authors have chosen to focus on a limited subset.

I tested the Tchen-Mikkelsen model a few years ago but could not reproduce the published results, despite the model’s simplicity and appeal. Based on a private communication with Prof. Mikkelsen over six years ago, I believe there may have been an issue with the formulation of the equations in their original paper. I am not sure whether this has since been corrected. It is worth noting that Hu et al. (2018) also reported systematic deviations from the Kaimal model in the inertial subrange when using the Tchen-Mikkelsen formulation, which, by design, should not occur.

The final paragraph of Section 3.7 introduces the Davenport coherence model, a foundational contribution that perhaps deserves its own equation number and a proper citation to Davenport (1962). The value of the decay coefficient reported in line 273 (a $\approx$ 60) appears erroneous. Panofsky and Dutton (1984) reported values more typically in the range of a $\approx$ 12–15. The decay coefficient itself depends on the velocity component, atmospheric stability and the type of separation (longitudinal, vertical, or transverse). Bowen et al. (1983) also document a dependency on the separation distance and measurement height, which was also observed at the FINO1 platform (Cheynet, 2018). Solari and Piccardo (2001) provide a helpful overview of this parameter.

It is important to recall that the Davenport model was developed specifically for microscale turbulence, and decay coefficients reported in that context should be interpreted accordingly. While applying the Davenport model to mesoscale motions is acceptable, comparing decay coefficients for mesoscale and microscale motions, as done in lines 274–276 (page 13) does not make much sense ("comparing apples and oranges ").

Point 5

Coherence vs coherent structures: I think it would also be helpful for the authors to clearly distinguish between the terms "coherence" and "coherent structures". Coherence is a correlation function in the frequency space. It is a concept from signal processing used as a statistical measure, for example, to characterize the spatial correlation of wind velocity fluctuations. The coherence is used, among others, to generate spatially correlated turbulent wind fields for aeroelastic codes. Coherent structures: a more abstract term used to describe organized motions in a fluid. These terms refer to fundamentally different concepts, though they are sometimes conflated in the literature.

Point 6

Section 5 Maybe this section (atmospheric turbulence observations) could also be reorganized around the concept of the SGHET framework. This section could address the following question: which sensors allow for observations that go beyond this framework?

This section discusses sonic anemometer measurements and draws a parallel between Taylor's hypothesis and the eddy-covariance method. However, this comparison may risk some misinterpretation. The eddy-covariance method does not inherently rely on Taylor's hypothesis. It computes turbulent fluxes directly from time series and does not convert temporal measurements into spatial ones. In contrast, Taylor's hypothesis is generally used to infer spatial statistics from temporal data.

Section 5 should also make a clearer distinction between profiler lidars and scanning lidars. These are different instruments with different purposes. In my experience, this distinction is often overlooked. I have worked a little with scanning lidars in complex terrain, coastal sites and offshore, with both long-range pulsed and short-range continuous waves for the study of atmospheric turbulence. In my experience, the main takeaway is that long-range scanning lidars allow for qualitative analysis of turbulence. However, such lidars would struggle to quantify turbulence, especially spectral statistics, which are more useful than integral statistics. This is due to large probe volumes and low sampling frequencies. Short-range scanning lidars (e.g., WindScanners) are more promising, but unfortunately less commonly used and their useful scanning range is limited to 150-200 m.

Thus, section 5 could address a few more important knowledge gaps and research challenges: (1) it could explain if and how remote sensing could help move beyond the SGHET framework. (2) It could highlight key limitations of scanning lidars: probe volume averaging that can be large, low sampling rate, limited reliability, high cost, reduced performance if the flow across the probe volume is heterogeneous, and the fact that they measure the along-beam component only, which complicates the analysis of 3D turbulence.

Point 7

On the turbulence intensity and its relevance to wind loading

A common misconception in wind loading on structures (turbines, tower, bridges, etc...) is that the turbulence intensity is based on the standard deviation of the wind speed. For non-yawed turbines, it should be the standard deviation of the longitudinal component. The IEC 61400-1 standard itself is ambiguous on that point as it defines the turbulence intensity first based on the standard deviation of the wind speed and later on, using the longitudinal wind velocity component.

For wind loading on structures, the turbulence intensity is, fortunately, not absolutely necessary. Within the SGHET framework, only three wind statistics are needed to generate a spatially correlated wind field: the mean wind speed, the one-point velocity spectra and the coherence of turbulence. The turbulence intensity can be directly retrieved from the velocity spectra and the mean wind speed. In the Eurocode (EN 1991-1-4: Eurocode 1), the turbulence intensity is defined based on the roughness length, which makes more sense from a modelling viewpoint. However, if I remember properly, the Eurocode is only usable for ultimate limit-state design (strong wind), for which the atmosphere is assumed neutral.

Another weakness of turbulence intensity as a statistic is that it is inversely proportional to wind speed. This property is not desirable for a non-dimensional turbulence metric usable in wind loading. In my opinion, the possible over-reliance on turbulence intensity in wind turbine design is a weakness of the IEC standard. The turbulence intensity is a tricky quantity to use: it is widely used, it is easy to measure and interpret, but has multiple definitions depending on the user's background. Finally, it has limited physical meaning and is less informative than the velocity spectra. A possible alternative could be the use of standard deviation profiles, which would depend on surface roughness, atmospheric boundary layer depth and thermal stratficiation of the atmosphere. However, this would require new measurement techniques for validation, with sensor heights extending beyond those of traditional mast-based observations.

Point 8

On the assumption of stationarity and Gaussianity: a conceptual challenge?

In non-stationary (intermittent) flows, non-Gaussianity often arises as a direct consequence of the lack of stationarity. Skewness and kurtosis are two commonly used metrics to quantify deviations from Gaussian behaviour. They rely on the assumption of stationarity. Therefore, their calculation in intermittent flows has limited physical meaning. In other words, once the flow is non-stationary, traditional statistical moments may no longer be applicable, and alternative analysis tools are needed. This raises two key questions: (1) Under which conditions do we observe stationary, non-Gaussian flows in the atmosphere, and how do they affect wind turbine loading? (2) Which tools can be used to study non-stationary atmospheric flows in the context of wind turbine design?

The second question has started being addressed in wind engineering since the 2010s, where researchers often decompose the flow into stationary and non-stationary components using tools such as empirical mode decomposition. Similar approaches could be valuable for advancing wind turbine load analysis beyond the SGHET framework. Maybe I should clarify that the field of wind engineering and wind energy are overlapping but distinct.

Point 9

On the Integral length scales (section 3.6)

Integral length scales (ILS) are useful in wind tunnel studies. For mast-based measurements, ILS are typically estimated in the streamwise (x) direction. Following Panofsky and Dutton (1984), page 176, the use of ILS in atmospheric studies should be avoided due to their lack of reliability. I tend to agree with them. In my opinion, estimates of ILS are fairly reliable for the vertical velocity component, but much less so for the horizontal components. As the authors correctly point out, this is largely due to the influence of large-scale eddies on the auto-correlation function. Obtaining better estimates would require a longer time series, but this can conflict with the assumption of stationarity, potentially compromising the validity of the ILS estimation itself.

It can be noted that there are multiple methods to estimate the ILS, not limited to the use of the autocorrelation function. These methods can lead to quite different values. For example, in wind engineering, the so-called von Kármán spectrum uses ILS as an input parameter, which can be estimated by least-squares fitting to measured power spectral densities. Overall, I think the section could reflect more critically on the relevance of ILS for wind turbine design. Are they truly useful? If they are not reliable, should we consider alternative length scales? And if so, which ones? These are open questions, and while I don't claim to have clear answers, I believe they are important to raise.

Point 10

Section 8.1 Over the years, I have frequently seen authors refer to Kaimal et al. (1972) as the source of both the so-called IEC-Kaimal spectral model and the exponential decay model for coherence. However, a close reading of Kaimal et al. (1972) shows that coherence was not investigated in that study. The foundational work on turbulence coherence was conducted earlier by Panofsky and co-authors, as well as by Davenport during the 1960s and 1970s. In addition, the one-point spectral model presented in Kaimal et al. (1972) differs significantly from the version adopted in the IEC standard.

Point 11

Lines 882–884: The statement that the IEC Kaimal model becomes height-independent above 60 meters is "not physically realistic" might benefit from a more nuanced phrasing. This height-independence is a reasonable simplification that reflects the properties of the mixing layer. Above the surface layer, spectral characteristics often no longer scale with height, and assuming continued height dependence may be less realistic. Paradoxically, the IEC simplification may offer a more accurate representation than surface-layer spectral models that enforce height scaling throughout.

Point 12

Line 885-888: The logical connection between the two sentences in this paragraph seems flawed. The sentence beginning with "However, wind turbines in the field are subject to atmospheric turbulence…" appears to contrast with the previous sentence describing how turbulence is simulated using stationary, Gaussian wind fields. But there is no contradiction here. Simulations based on the SGHET framework are intended to approximate atmospheric turbulence.

A formulation that corrects that issue and highlights the limits of the SGHET framework would read as "*According to IEC standards, standard industry tools such as TurbSim (Kelley and Jonkman, 2007) and the Mann turbulence generator (e.g., Dimitrov et al., 2024) generate stationary homogeneous Gaussian turbulent wind fields. While these models are widely used for design and simulation, actual atmospheric turbulence experienced by wind turbines can exhibit strong non-stationarity and non-Gaussian characteristics, which may significantly affect power output, structural loading, and fatigue life*."

Point 13

Lines 897-902: The paragraph describing TurbSim may give a misleading impression regarding its relationship to the SGHET framework. While the tool does allow for the superposition of randomized coherent turbulent structures, the underlying turbulence field is still generated within the SGHET paradigm. That is, it remains stationary, Gaussian, and homogeneous, constructed from predefined spectra and coherence functions. It is important to clarify that coherence structures are always present in TurbSim-generated fields due to the use of a coherence function; this is not unique to the added structures. Coherent structures refer to organized, persistent patterns of motion. They can be turbulent or non-turbulent motion. These structures are spatially and temporally correlated regions of the flow, e.g. vortices, shear layers, or streaks, that carry a significant portion of energy and contribute to the transport of momentum, heat, or scalars. Unlike statistical coherence, which describes correlations between signals, coherent structures are physical features within the turbulent flow.

The superposition of optional deterministic coherent structures can indeed help represent flows beyond the scope of traditional models like the IEC Normal Turbulence Model. In that sense, TurbSim offers a useful extension. Maybe the paragraph could better distinguish between the base SGHET-generated field and the additional non-SGHET structures. This clarification would help avoid confusion regarding what constitutes a true move beyond the SGHET assumptions.

Point 14

Lines 912-923 The discussion on quad-coherence and its impact on wind loading is interesting but appears somewhat speculative. As discussed in Cheynet et al. (2022), quad-coherence typically may have little to no influence on wind loading when linearized load models are used. Its relevance for nonlinear loading remains less clear. A recent study by Wang et al. (2025) investigated the role of out-of-phase fluctuations on wind-induced forces on floating bridges, which might offer some insight here.

It is also worth noting that specific modelling of quad-coherence is not strictly required for wind field simulation using the IEC Kaimal spectrum with exponential decay. As shown in (eqs. 28-29 Cheynet et al., 2022), quad-coherence can arise implicitly through the use of a complex exponential phase term in the coherence function. This produces vertical quad-coherence but not lateral quad-coherence unless yaw misalignment is introduced. Similarly, the uniform shear model without blockage (Mann, 1994) also exhibits vertical but not lateral quad-coherence.

Given this, it might be helpful to refine the paragraph to reflect that while quad-coherence can be present and included in some models (like the Mann model), its practical significance for wind turbine loading, particularly in standard design methodologies, remains uncertain.

Point 15

Lines 928-934: An important aspect to consider when combining mesoscale and microscale motions is that such models generally assume the two scales are uncoupled, relying on the presence of a clear spectral gap. If I remember well, the model by Syed and Mann (2024) follows this assumption and was developed specifically for neutral conditions; it may also be valid under stable stratification. However, under convective (unstable) conditions, the separation between scales often breaks down, and interactions between mesoscale and microscale motions become apparent. In such cases, the assumption of scale independence may no longer hold, and the applicability of this type of model becomes uncertain. I believe this limitation should be more clearly highlighted in the discussion, especially given the relevance of convective boundary layers in wind energy research.

Point 16

Section 8.3: I believe this is one of the most important sections of the manuscript and may deserve further elaboration. A significant research gap in wind loading, particularly from a structural dynamics perspective, is the continued emphasis on undisturbed, upstream flow conditions. In practice, many wind turbines — especially in large farms — operate in the wake of other turbines, where the flow may not be stationary, Gaussian, or homogeneous. This calls for a move beyond the SGHET framework commonly used in wind turbine design.

Most turbulence generators assume at least statistical stationarity or spatial homogeneity. However, in wind farm wakes, these assumptions are often violated, raising the question of whether our current tools are still appropriate. There is a clear need for new turbulence characterization methods that can account for turbulence in the wake of turbines, particularly in the context of wind loading and fatigue. While I am not an expert on FAST.Farm (I could misunderstand it), it appears to offer a promising middle ground by capturing key wake dynamics and turbulence advection in a computationally efficient framework that may be suitable for load analysis.

Point 17

Section 8.6 I think this section should begin by clearly restating the prevailing paradigm under which turbulence is used in wind energy, namely, the assumption of stationary, Gaussian, and homogeneous turbulence. Framing the conclusion around this paradigm would help emphasize the need to move beyond it. Specific pathways for doing so, whether through new models, observational techniques, or analytical tools, would make the call to action more concrete. As it stands, the list of recommendations feels somewhat disjointed, and organizing it around a clear logical structure would significantly improve its clarity.

Point 18

Line 1185: The phrase “mesoscale-generated turbulence associated with low-level jets, convective cells, convective rolls, and gravity waves” seems to conflate turbulence with larger-scale organized motions. In wind engineering and micrometeorology, such features are not considered turbulence themselves but rather mesoscale flow structures that can interact with or trigger turbulence under certain conditions. It may be helpful to clarify this distinction to avoid confusion between mesoscale motions and (microscale) turbulent fluctuations.

Point 19

Line 1190: this line attempts to address multiple concepts: atmospheric stability, turbulence characteristics, and turbine performance, in a single sentence, which results in ambiguity. It also groups “turbulence,” “coherent structures,” and “length scales” together in a way that conflates concepts at different levels. Turbulence is a flow regime, coherent structures are organized motions that may or may not be part of turbulent flow, and length scales are statistical measures used to characterize turbulence. These should be more clearly distinguished, as they represent different aspects of atmospheric dynamics.

Overall, this manuscript is a valuable contribution to the field. My comments are intended to help sharpen the focus and strengthen the clarity of the arguments. In particular, I believe that organizing the manuscript, especially the conclusion, around the idea of a paradigm shift beyond SGHET assumptions would enhance its impact. I hope the authors find these suggestions useful.

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