Investigations of Correlation and Coherence in Turbulence from a Large-Eddy Simulation

Community comment by Etienne Cheynet

The manuscript "Investigations of Correlation and Coherence in Turbulence from a Large-Eddy Simulation" by Thedin et al. addresses major contemporary challenges for the development of wind energy. I found the preprint informative and interesting to read. Nevertheless, the manuscript has some shortcomings. A number of points should be clarified, partly because the scientific review of the coherence and its connexion to wind loading is perfectible.

1 Specific comments

Point 1

Abstract: I feel the abstract is somewhat self-contradictory. The authors write first that turbulence statistics and spectral analysis give limited information to describe a turbulent flow. Then the authors state that they focus on the coherence and spatial correlation functions. But the coherence is also a (two-point) spectral characteristic of turbulence. The integral time scale and integral length scale are also turbulence characteristics[a]. So I do not really understand the logical reasoning here.

[a]The turbulence length scales are integral characteristics and contain less information than the coherence or auto- and cross-covariance functions

Point 2

Line 23-25: It should be noted that the coherence does not necessarily compare the phase relationship between two time series or spatial series. In eq. 3, the authors use the magnitude-squared coherence (mscoh) which does not give any information on the phase. This is something they judiciously comment. A more appropriate definition of coherence is given by Ropelewski et al. (1973), which states that the coherence is a correlation function in the frequency space.

Point 3

Line 31-32: I understand what the authors mean with "implicitely", but this sentence is actually incorrect. In the Davenport model (Davenport, 1962), the separation distance is explicitly needed (and modelled) in the coherence function:

$$\gamma_u = \exp\left(\frac{-C_z^u d_z f}{\overline{u}}\right) \tag{1}$$

where d_z is the vertical separation distance, f is the frequency, C_z^u is an empirical decay coefficient and $\overline{u}$ is the mean wind speed. Interestingly, in the Davenport model, the dependency of C_z^u on the separation distance is not modelled. This dependency was found to be, sometimes, necessary for vertical separations (Bowen et al., 1983; Cheynet, 2019) and lateral spatial separations (Sacré and Delaunay, 1992; Cheynet et al., 2017).

Point 4

Line 33: Contrary to a popular belief, Kaimal et al. (1972) did not study the coherence of turbulence. Therefore, writing "Kaimal's coherence" is incorrect. A more appropriate wording for this coherence model would be "exponential decay model" based on the Davenport model. It can also be noted that the empirical velocity spectra model used in IEC 61400-1 (2005) is not Kaimal's model either, although the work by Kaimal et al. (1972) was used as a basis for this model.

Point 5

Line 41: We should note that in the marine atmospheric boundary layer (MABL), non-neutral conditions are also commonly observed at wind speed above the rated wind speed of wind turbines (Barthelmie, 1999; Sathe et al., 2011; Cheynet et al., 2018).

Point 6

Line 53-54: The sentence "In general, these studies found that coherence levels based on observations at an offshore environment are higher than those computed by the spectral model" may be contradicted by the scientific literature. In Cheynet et al. (2018), the IEC exponential coherence model is found to model fairly well to the vertical coherence under near-neutral conditions at FINO1. However, as for the Davenport model, the IEC exponential coherence model has two main drawbacks: (1) it does not account for the thermal stratification of the atmosphere and (2) its main decay coefficient, which is equal to 12, is independent on the separation distance. In Mann (1994) and Cheynet (2019), the uniform-shear (US) model tends to slightly overestimate the vertical coherence of turbulence. It can be noted that, following Mann (1994), the US model captures remarkably well the lateral coherence of near-neutral turbulence.

Point 7

Page 2: I agree with the authors that studying the lateral coherence with sonic anemometers is challenging. However, anemometers mounted on bridges in coastal areas (e.g. Sacré and Delaunay, 1992; Kristensen and Jensen, 1979) have been used in the past with encouraging results. For the past few years, scanning Doppler wind lidars have also been used to study the lateral coherence of turbulence (with relative success). Sonic anemometer data from bridges, while not offshore, are still valuable to validate spectral turbulence models. Also, the US model Mann (1994) was validated for lateral separation in the MABL. This may be worth mentioning in the manuscript.

Point 8

Line 162-163: A more robust way to test Taylor's assumption of frozen turbulence is to study the coherence of turbulence with longitudinal separations. If the turbulent field is frozen, the longitudinal coherence will be equal to 1 at every frequency. In reality, it will not be the case. This method can also be used to show that smaller eddies will not satisfy the assumption of frozen turbulence at separation greater than a few dozen of meters while bigger eddies may be considered as "frozen" over a distance greater than e.g. 100 m (Cheynet et al., 2017, Fig. 11). The approach adopted in Fig 6 is more limited as it only focuses on large eddies.

Point 9

The paper seems to dedicate a significant part to the coherence at longitudinal separation. From the viewpoint of wind turbine design, this may be a minor challenge compared to the need to reduce the uncertainties associated with the co-coherence at lateral and vertical separations.

Point 10

Line 240: The relationship between wind loading and wind coherence was developed and modelled in the 1960s by pioneers such as Davenport (Davenport, 1962), through the so-called "buffeting theory". This theory was originally applied to high-rise buildings and bridges. Maybe a lesser-known fact is that the response of a wind turbine can be quantitatively described without an elaborated numerical model by the buffeting theory. I recommend reformulating line 162 by stating that the theory related to the wind-induced response of wind turbines can be traced back to the 1960s with the development of the buffeting theory, where both the coherence and one-point velocity spectra were used to predict the dynamic response of wind-sensitive slender structures.

Point 11

Line 263-265 and Lines 270-271: It should be kept in mind that one fundamental assumption to study and model the coherence of turbulence is that the flow is fairly homogeneous and stationary. Downstream of a wind turbine and inside a wind farm, the flow can be strongly heterogeneous. Therefore, the paragraph on lines 263-265 and lines 270-271 may be criticized for violating the assumption of flow homogeneity. I think these lines can be removed without problem.

Point 12

Line 268-270: I do not understand the logical reasoning behind the sentence "The coherence values for laterally separated points [...] suggest that it may be important to properly account for the longitudinal separation, in addition to the lateral and vertical separations included in the

IEC standard". The fact that the lateral coherence coh_y is smaller than the longitudinal coherence coh_x does not justify the need to model coh_x for wind loading on a wind turbine. If turbulence is considered frozen, which is commonly done, then for a given separation, $coh_x \geq coh_y$ at every frequency.

Point 13

Line 272-273: The fact that the coherence is not always equal to 1 at zero frequency was documented by Kristensen and Jensen (1979) and in many studies since then. This should be mentioned in the manuscript

Point 14

Line 274-276: The fact that the mscoh does not converge toward zero is not due to numerical noise but because the mscoh is a biased estimator. This bias is documented in e.g. Kristensen and Kirkegaard (1986) and commented in Mann (1994, p. 156). To avoid (or limit) this bias, I recommend focusing on the co-coherence.

Point 15

It should be noted that experimental studies of the coherence at separation distance greater than 100 m are associated with significant uncertainties because only a few data points are different from zero or are hidden in the ambient noise. To reduce such uncertainties, a large number of samples should be used. The coherence estimates will be then ensemble averaged. Ideally, both short and large separation distances should also be considered to capture the full extent of the coherence. If such conditions are not met, it can be challenging to reach meaningful conclusions.

Point 16

Page 14: I do not recommend using negative separations because these have no physical meaning and could trigger numerical errors with exponential coherence models. Instead, I suggest focusing on pairs of measurement heights or lateral positions as formulated in Putri et al. (2022, p. 1697).

Point 17

Line 280-288: From my understanding, the asymmetry effect mentioned by the authors has been studied in detail in the MABL by Cheynet (2019) at FINO1 and by Putri et al. (2022) near Vindeby wind farm. The asymmetry can be modelled quite well using the approach adopted by Bowen et al. (1983) and reflects the presence of the ground, which blocks the flow. Some interesting questions are (1) how this asymmetry changes with the thermal stratification of the atmosphere and (2) how it affects the loading on a wind turbine. These challenges can be discussed by the authors or be the topic of further study.

Point 18

Line 288-289: The justification with reference to Naito (1983) seems incorrect to me. The coherence is lower than unity at zero frequency because the separation distance is not negligible compared to a typical length scale of turbulence (Kristensen and Jensen, 1979). If there exist long-period fluctuations, then the time series may be non-stationary and prevent the study of turbulence characteristics. Fortunately, a quick look at Fig 2 suggests that the time series are reasonably stationary.

Point 19

Line 332-334: The suggestion "Future work should include a parametric study with varying atmospheric conditions, which will produce a set of coherence curves for each component and separation direction" has been done for vertical separations in Cheynet et al. (2018) at FINO1 using two years of continuous measurements. More generally, the dependency of the coherence on the atmospheric stability is also documented for an onshore location in Soucy et al. (1982) and in the MABL by Putri et al. (2022).

Point 20

Line 339-344: I agree with the authors. However, this paragraph is partly covered by the buffeting theory that was developed in 1960s and 1970s. So I am not sure if it is still an up-to-date material for discussion. Maybe this can be elaborated or removed?

Point 21

Section 5: One of the limiting aspects of the coherence for stochastic flow simulation is the need for a fairly homogeneous flow, which is not the case inside a wind farm. This limit can be partly overcome by LES simulation. maybe this aspect can be discussed in more detail here?

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