Investigations of Correlation and Coherence in Turbulence from a Large-Eddy Simulation - Version 2

Reviewer's comments

General comment

The revision of the manuscript "Investigations of Correlation and Coherence in Turbulence from a Large-Eddy Simulation" by Thedin et al. has substantially improved since the previous version. The nice explanation of the authors in their replies to the reviewers has also helped me a lot to understand the objective of the paper. Most of my comments are specific and, fortunately, can be addressed through **minor revisions**.

For the general comment, I recommend the authors split section 4 "Methodology & results" into two independent sections: the first one would be named "Methods" and the second one would be named "Results". In the current version, section 4 is too unstructured to allow the reader to easily grasp the logical pattern of the results. The new section "Methods" should contain the necessary information on (1) the numerical setup, (2) the method to derive the spatial, temporal correlation and spectral characteristics and (3) the background information on the previous turbulence models.

Specific comments

Point 1

Line 1 and line 21: The contradiction that was noted in the previous version (Point 1.4, version 1) is still present in the abstract and one line 21. To remove the contradiction, I suggest simply removing "and/or spectral analysis" in line 2 and line 21. The reason is that coherence analysis is part of spectral analysis. Also, auto-correlation analysis is directly connected to a spectral analysis by the Wiener–Khinchin theorem. The latter states that for a stationary random process, the power spectrum of the process is the Fourier transform of the autocorrelation function. More generally, lines 1 and 2 could be formulated as "Microscale flow descriptions are often given in terms of integral flow characteristics. Those metrics, while valuable, give limited information about the spatial and temporal structure of turbulent eddies."

Point 2

Line 34: The equation for the Davenport coherence model can be given in a new line with an equation number. This is a little more elegant than an in-line equation.

Point 3

Line 49: We should remember that "Kaimal's exponential decay model" is inaccurate since the exponential decay model is actually from Davenport. In the technical report by Thresher et al. (1981) the authors use the expression "Davenport-Kaimal model", which is much fairer since it indicates the combination of the Kaimal spectral model with the Davenport coherence model. Alternatively, the term "Thresher's model" could be used too.

Point 4

Line 72: The reference to Wise and Bachynski (2019,2020) and Shaler et al. (2019) should be written as a parenthetical citation rather than an in-text citation.

Point 5

Line 76: I do not understand the sentence "As mentioned, standard only specifies in the streamwise direction". Do you mean "As mentioned, the IEC standard only specifies the coherence of the along-wind component in the cross-wind direction"?

Point 6

Line 70-77: I was unaware of this information. This is quite useful! I am a little puzzled by the choice of some authors to have a fully correlated wind field for the v and w velocity components if the standard does not explicitly state which coherence values should be used. In wind engineering, the coherence of the three velocity components (u, v and w) is usually modelled, even though there exist a lot of uncertainties. The review by Solari and Piccardo (2001) is quite enriching in this regard.

Point 7

Line 87: For the sake of clarity, I suggest reformulating the last sentence as "In the IEC standard, the thermal stratification of the atmosphere is not explicitly accounted for by either the Mann or Davenport-Kaimal model". In practice, it is possible to (partly) account for the stability in the Mann model by fitting this model to in-situ data representative of unstable or stable conditions as done by Sathe et al. (2013). The same idea applies to the Davenport model, see e.g. Soucy et al. (1982); Cheynet et al. (2018), where the Davenport decay coefficient become stability-dependant for the three velocity components.

Point 8

Lines 88-93: This paragraph is really nice and, I believe, crucial to the understanding of the paper. I suggest moving it to the beginning of the introduction, typically after the first or second paragraph. In general, the objectives of the study should be announced early. Also, a new paragraph announcing the structure of the paper can be added at the end of the section “Introduction”.

Point 9

Lines 105: I suggest removing “and will only be as accurate as the mesoscale”. This part is contradicted by the sentence coming immediately after, since the microscale gives information on turbulence but not the mesoscale.

Point 10

Figure 2: This is a nice and clear figure. Maybe one sentence can be added to explain how the wind shear exponent is calculated. This would be useful to the reader. This sentence could be placed in the section “Methods”.

Point 11

Section 4.3: For the sake of pedagogy, it could be briefly mentioned that since the flow is assumed homogeneous, spatial averaging is equivalent to ensemble averaging. Or maybe has it been already mentioned?

Point 12

Section 4.3 bis: I really like the idea to assess Taylor’s hypothesis of frozen turbulence by using the integral time scale and integral length scale. For the sake of clarity, I suggest writing the equations demonstrating how these quantities are calculated. In particular, the estimation of the integral length scale (or time scale) can be obtained either by (1) integration of the auto-correlation down to the first zero crossing or (2) by modelling the autocorrelation with an exponential decay as

$$R_u(d_x) = \exp\left(\frac{-d_x}{L_u^x}\right) \tag{1}$$

where $R_u(d_x)$ is the auto-correlation function; d_x is the streamwise separation distance and L_u^x is the integral length scale of the u-component in the x-direction.

Point 13

Figure 8: For the sake of clarity, replacing "integral length scale" with a symbol may be preferable. For example, the previous point uses L_u^x which specified both the direction and velocity component. The integral length scales of the along-wind component could be L_u^x (streamwise separation) L_u^y (lateral separation) or L_u^z (vertical separations).

Point 14

Line 248: It is true that Nybø et al. use the term “co-coherence” and “quad-coherence”. However, their paper is not a primary source. The possible primary source is the thesis by Watson (1975). The thesis is openly available at this link. The term “co-coherence” was further used in the 1980s by Barnard (1981) among others.

Point 15

Line 251: I think “Kaimal exponential coherence model” can be replaced by “IEC exponential decay model”.

Point 16

Line 260-261: “representing second-order statistics” may be removed since the Mann model also describes second-order statistics only.

Point 17

Line 275: I suggest reformulating “Nowadays, more complex simulation tools” into “Nowadays, simulation tools for wind energy application”. Indeed, engineering tools relying on the Davenport model can be considerably more complex than the IEC standard, for example, the ESDU standards for the Wind Engineering Series.

Point 18

Line 367: This line read as “Fig 14 summarizes the importance of modelling all three components of the turbulence”. Is it the importance for wind loading or wake meandering?

Point 19

Fig 14: Is this figure necessary to the paper? If the authors decide to keep it, I suggest not using a contour map but a pseudocolour plot instead. The contour lines introduce artefacts that can lead to misinterpretations.

Point 20

Line 422: The discussion is interesting, but I suggest not discussing the cut-off frequency in terms of frequencies (Hz) but in terms of wavenumbers (m^{-1}). Otherwise, the cut-off frequency will depend on the mean wind speed.

Point 21

Line 448-449: the part “suggesting that frozen turbulence may not be applicable under other conditions” may be reformulated more clearly. If the hypothesis of frozen turbulence is discussed in terms of coherence, it should be related to the size of eddies. For example, at a specific spatial separation, the turbulence can be considered frozen for large eddies (high coherence) but not for small eddies (low coherence).

Point 22

Line 452: “better inform turbulence models” is a little unclear to me. Maybe “improve turbulence models” is better a better formulation?

References

Thresher, R., Holley, W., Smith, C., Jafarey, N., Lin, S.. Modeling the response of wind turbines to atmospheric turbulence. Tech. Rep.; Oregon State Univ., Corvallis (USA). Dept. of Mechanical Engineering; 1981.

Solari, G., Piccardo, G.. Probabilistic 3-D turbulence modeling for gust buffeting of structures. Probabilistic Engineering Mechanics 2001;16(1):73–86.

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Soucy, R., Woodward, R., Panofsky, H.. Vertical cross-spectra of horizontal velocity components at the Boulder observatory. Boundary-Layer Meteorology 1982;24(1):57–66.

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