WES 2022–71: Author's Response to the Reviewers (Version 2)

The authors thank both reviewers for their further comments. A marked-up version of the manuscript highlighting all the changes is available. Note that large chunks highlighted in the marked-up version are related to re-organization of the manuscript.

Reviewer 1, Etienne Cheynet

The authors thank Dr. Cheynet for his time in reviewing our revised manuscript. We appreciate the further comments provided and have addressed them below.

Reviewer Point P 1.1 — 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.

Reply: Thank you for the comments. We agree that the organization of the paper could be improved by separating methods and results. We have moved the sections around and re-structured it into two separate sections, also including the "Scenarios investigated" section into the new "Methodology".

Reviewer Point P 1.2 — 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."

Reply: Thank you for the suggestion. We meant to say that simple spectral analysis is usually done just to ensure the cascade follows $-5/3$. In any case, your point is valid and we have removed that mention from the abstract and the text.

Reviewer Point P 1.3 — 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.

Reply: Done.

Reviewer Point P 1.4 — 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.

Reply: We have made small updates throughout the document to address this issue. Thanks for highlighting it once again.

Reviewer Point P 1.5 — 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.

Reply: Yes. That was a mistake on our part. Fixed it.

Reviewer Point P 1.6 — 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"?

Reply: We do mean that the IEC standard only specifies the coherence of the streamwise component, in the vertical and crosswind direction. We made the sentence more clear. We revised the entire manuscript so that streamwise and cross-stream refer to the components and along-wind and crosswind refer to directionality.

Reviewer Point P 1.7 — 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.

Reply: We agree with your comment about some author's choice of coherence model in v and w. Some authors decide to use *a* equation to model the coherence in v and w, but no further work is done to assess the performance and accuracy. Unfortunately the latest version of the standard is quite confusing with the terms L_c and L_k which has resulted in inaccurate interpretations and thus inaccurate use of the suggestions from the standard.

Reviewer Point P 1.8 — 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.

Reply: We have rephrased the sentence being more explicit about the "stability" aspect. We understand that such models can have their constants tuned/curve-fit using data representative of whatever stability state one wishes to represent. Our comment was more related to their original formulations– we made that more clear.

Reviewer Point P 1.9 — 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".

Reply: We are glad this paragraph is now clear and useful to the reader. We understand the value of having the objectives stated early on, but we have decided to keep it as is, since it functions a closing comment for the introduction, tying all the points discussed and clearly stating the objectives and what will come next.

Reviewer Point P 1.10 — 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.

Reply: When we say the microscale will only be as accurate as the mesoscale, we are referring to the mean quantities. Yes, the microscale will develop the lower scale turbulence, but it will not change the mean in any significant way. The point of the sentence is to say that the microscale may not match the observations, but will rather match the mesoscale—that is illustrated in Fig 2. We have added "mean quantities" to the sentence to make that more clear.

Reviewer Point P 1.11 — 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".

Reply: Thanks for the suggestion. We have added some comments about it when discussing the figure.

Reviewer Point P 1.12 — 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?

Reply: We have noted that on the spatial correlation subsection, in the context of using time averages as an ensemble average. But we agree that is not a bad idea to mention that again when referring to the spatial average. We added a comment about it in the beginning of the new "Results" section.

Reviewer Point P 1.13 — 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.

Reply: We computed the integral scales by numerical integration of the correlation curve we obtained (Fig. 7 in the latest version of the manuscript). We had mentioned that on line 219-221 of the first revision of the manuscript.

Reviewer Point P 1.14 — 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).

Reply: That is a fair point. We have tried to make it clear which direction and velocity components we used. We do not give any equations, but we have added symbols to make it easier to understand what quantity we are referring to in the larger context of integral scales and across different papers. We appreciate the suggestion.

Reviewer Point P 1.15 — 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.

Reply: Thanks for pointing that out. We did not mean to imply that Nibø et al coined the term, it was simply given as reference to the reader. We have removed the reference to Nibø et al's work.

Reviewer Point P 1.16 — Line 251: I think "Kaimal exponential coherence model" can be replaced by "IEC exponential decay model".

Reply: As per the point P 1.4, we have modified these mentions throughout the manuscript to better reflect the authors of the models.

Reviewer Point P 1.17 — Line 260-261: "representing second-order statistics" may be removed since the Mann model also describes second-order statistics only.

Reply: Done. Thanks for catching this.

Reviewer Point P 1.18 — 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.

Reply: We added the suggested "wind energy applications", but left "complex tools". Our reasoning is that some reasonably complex tools do make use of the models suggested by IEC (e.g. FAST.Farm).

Reviewer Point P 1.19 — 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?

Reply: See response to next point.

Reviewer Point P 1.20 — 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.

Reply: We were vague on purpose about this figure. It is important because it shows the coherence in the longitudinal separation which isn't usually given, as well as the v and w components being non-negligible. We decided to include it because it does give more "curves" than Figs. 9–11. We kept a contour map but have removed the thin lines between the levels– thanks for the suggestion.

Reviewer Point P 1.21 — 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.

Reply: The cut-off frequency mentioned in the discussions was left in terms of frequencies because we wanted to refer to the specific setup we had and especially the plots shown. We understand that it depends on the mean wind speed, but it also depends on the grid resolution used. We believe the discussions are more consistent with the plots shown earlier if we talk in terms of the same quantities as before, as opposed to talk about in terms of wavenumbers for the first time in the paper.

Reviewer Point P 1.22 — 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).

Reply: That's an interesting point. We added a few sentences relating it to the size of eddies. Thanks for the suggestion.

Reviewer Point P 1.23 — Line 452: "better inform turbulence models" is a little unclear to me. Maybe "improve turbulence models" is better a better formulation?

Reply: We have changed the sentence to the suggested wording.

Reviewer 2

Thanks again to reviewer 2 for his/her comments. We have addressed all comments in the updated manuscript.

Reviewer Point P 2.1 — Line 30: check subject-verb agreement

Reply: Fixed. Thanks for catching it.

Reviewer Point P 2.2 — Line 61: the structures studied by Li et al. were lattice frames - not necessarily for wind turbines

Reply: We appreciate you catching that mistake. We have removed the incorrect reference.

Reviewer Point P 2.3 — Line 265: "a sufficiently turbulent condition" - maybe "a sufficient description of the turbulence for load estimation purposes"?

Reply: We modified the sentence. Thanks for the suggestion.

Reviewer Point P 2.4 — Line 282: lowest rather than highest? On a log scale, it's hard to say which frequencies are really correctly captured. For a 15 minute time series, the Nyquist frequency is around 2×10^{-3} Hz. I think that the bounds in Fig. 9 are probably more relevant than those in Fig. 7.

Reply: You're correct. The entire paragraph has been re-written. And yes, you could say that the drop in resolved energy drops after 10^{-1}, which is the reason we did not show the coherence curves much past that frequency.