Final Author Responses

"Synthetic Generation of Long Turbulent Wind Time Series Using Hindcast Model Forcing for Offshore Wind Farm Simulation"

WES-2026-58

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1 General Remarks

We thank the reviewers and the editor for their valuable feedback and constructive comments. Below, we address each comment point by point.

2 Anonymous Referee #1 (RC1)

We thank Anonymous Reviewer 1 (2026) for the constructive comments. The following responses address each point raised.

Comment:

> *The authors present a novel methodology to incorporate long-term mesoscale fluctuations into the generation of synthetic turbulent time series for wind farm energy yield and load assessment. The proposed approach is validated against microscale measurements collected at the FINO1 offshore meteorological mast.*
> *The reviewer finds the topic timely and relevant, addressing an important challenge in wind energy science. The proposed methodology is sound, well-structured, and consistently applied throughout the manuscript. The results are presented clearly, and the limitations of the approach are appropriately acknowledged and discussed.*

Author Response: We thank the reviewer for this positive and encouraging assessment of the manuscript. We have nonetheless revised the manuscript throughout in response to the detailed comments below, in particular to better articulate the novelty of the method in the Introduction and to improve the overall clarity and language of the text.

Comment:

> *However, the manuscript would benefit from a stronger emphasis on the novelty and advantages of the proposed method compared to existing approaches, particularly those related to mesoscale spectral modeling. This aspect should be more clearly articulated, especially in the Introduction.*

Author Response: The introduction has been refactored.

Comment:

> *1. Abstract: The description of the methodology is overly technical and does not sufficiently highlight the practical modeling advantages or its improvements over the current state of the art. A clearer, more impact-oriented summary is recommended.*

Author Response: The abstract has been revised to better highlight the practical impact of the method and to improve consistency with the rest of the manuscript.

Comment:

> *2. Section 2: Including a schematic diagram of the proposed workflow would greatly improve clarity. In particular, a visual representation showing how different datasets are combined, along with the key inputs and outputs, would help readers better understand the methodology.*

Author Response: A full methodology schematic, showing how the NORA3 mesoscale forcing and the FLAggTurb microscale simulations are combined through splicing, has been added to Fig. 3.

Comment:

> *3. Section 4: The methodology relies on the NORA3 dataset. It would be valuable to discuss whether the performance of the method is sensitive to the choice of hindcast model, and how results might vary if alternative datasets were used.*

Author Response: This is indeed an important point that had been left out of our discussion. A new section addressing the sensitivity of the method to the choice of hindcast model, and its applicability to other mesoscale models, has been added to the discussion (Sect. 4.4).

Comment:

> *4. Language: The manuscript would benefit from careful proofreading. In particular, the Introduction contains several sentences with grammatical issues, including missing verbs and awkward phrasing, which affect readability.*

Author Response: We thank the reviewer for pointing this out. The manuscript, and the Introduction in particular, has been carefully proofread and revised to fix grammatical issues, missing verbs, and awkward phrasing.

3 Anonymous Referee #2 (RC2)

We thank Anonymous Reviewer 2 (2026) for the constructive comments. The following responses address each point raised.

Comment:

The manuscript presents a method to generate time series that include variability on both meso- and microscale, based on the NORA3 mesoscale model. The method combines time series output from NORA3 with synthetically generated turbulence, the latter based on the model of Cheynet et al. (2018). The relevant turbulence parameters (friction velocity, wind speed, and stability) are derived from the NORA3 output. The method is demonstrated for the FINO1 site, for which a model time series at 100 m is generated. The statistics of turbulence intensity of the resulting synthetic time series are shown to be in good agreement with FINO1 measurements at 82.1 m across different stability and wind speed classes. However, the spectra of the synthetic time series show reduced power spectral density at timescales around one hour compared to the FINO1 measurements. To correct this underestimation, the authors suggest to add mesoscale variability with parameters derived from the measurements.

Author Response: We thank the reviewer for this accurate and thorough summary of the manuscript.

Comment:

The method is novel and relevant for the wind energy community. The manuscript is overall well written, the relevant literature is adequately cited, the datasets are adequately introduced, and the results are well presented. However, further discussion on the representativeness of the excellent agreement in turbulence intensity with the measurements should be included. This includes the applicability to other locations and heights, and, desirably, to other models. In addition, there are some parts of the manuscript which need further clarification, these are listed in the following section.

Author Response: A dedicated discussion section (Sect. 4.4) has been added to address the representativeness of the methodology across different weather models, locations, and heights.

Comment:

SC1. The agreement between the turbulence intensity of the synthetic and measured time series in Fig. 7 is very good. However, the comparison involves different heights (100 m for NORA-based time series vs. 82.1 m for FINO1), and since σ_u generally decreases with height while mean wind speed increases, TI is expected to decrease with height. Please include a discussion on how this height mismatch affects the comparison. In addition, the applicability of surface-layer scaling at 100 m should be discussed. Finally, given the very small bias in wind speed standard deviation, please confirm whether friction velocity and stability used in generating the synthetic time series are taken directly from NORA3 in all figures.

Author Response: A paragraph discussing the effect of the height difference has been added to the TI discussion (Sect. 4.1). To confirm: the friction velocity and atmospheric stability used to generate the microscale part of the spectrum are indeed taken directly from NORA3 in all figures. NORA3 has the advantage of representing the friction velocity at FINO1 very well (Pauchet and Chabaud, 2025).

Comment:

SC2. The title of the manuscript: "Synthetic generation of long turbulent wind time series using hindcast model forcing for offshore wind farm simulation", suggests a *generally applicable methodology for offshore locations, but the study is based specifically on the NORA3 hindcast model and the FINO1 location. It would strengthen the paper to include a brief discussion on the expected generalization of the approach to other locations and mesoscale models. In particular, please comment on whether key turbulence input parameters (e.g., friction velocity and Obukhov length) are expected to be represented with sufficient accuracy across different environments and models. Additionally, it should be acknowledged that the model of Cheynet et al. (2018), including its parameter fitting, is based on FINO1 measurements, and may therefore perform particularly well for this dataset.*

Author Response: A discussion section addressing these points has been added (Sect. 4.4).

Comment:

SC3. The manuscript does not discuss the spectral characteristics of the wind field from the NORA3 hindcast. Since numerical weather models do not necessarily represent wind speed variability accurately at the mesoscale ((e.g. Skamarock, 2004)), it would strengthen the paper to briefly assess this aspect. In particular, please comment on whether NORA3 reproduces the expected -5/3 slope in the mesoscale range. Including the spectra of the spline-interpolated NORA3 time series, for example in Fig. 10, would be a useful addition.

Author Response: NORA3 does properly reproduce the $-5/3$ slope ($-2/3$ in the premultiplied spectrum), which is now highlighted in the results section (Sect. 3.2). A short paragraph has also been added to the discussion (Sect. 4.2) on the good representation of mesoscale features by NORA3, consistent with other models.

Comment:

SC4. Lines 169–172: The description of how the cross-contribution term is omitted was unclear to me and would benefit from clarification. If I understood correctly, multiple time series are generated using different random seeds, and the realization with the smallest cross-contribution term is then selected. Please confirm whether this interpretation is correct and consider rephrasing for clarity.

Author Response: A more detailed explanation of the splicing process, with a focus on statistical independence, has been added (Sect. 2.2.3). To directly answer the second part of the question: no, the realization with the smallest cross-contribution term is not selected. Consistently with the underlying Gaussian process generation, the cross-contribution term is zero only when averaged over realizations. Monte Carlo simulations must therefore be run for each generated realization, and results (e.g. spectra, not the time series themselves, which would average to zero) should be averaged over these realizations, as is standard practice in turbulent wind turbine simulations. This was already stated in the manuscript but has now been made clearer.

Comment:

SC5. Line 109: It remains unclear why the model from Cheynet et al. (2018) is only applied to the u component of the wind, while the v component is modeled using the Kaimal spectrum. Is there a specific reason for using the Kaimal model for the latter?

Author Response: At the time most of this work was carried out, the parameters from Cheynet et al. (2018) for the v and w components were not yet available, and time constraints did not allow us to rerun all simulations once they were; this is now clarified in the manuscript (footnote 1, Sect. 2.2.1). Future versions of FLAggTurb will implement the full Cheynet et al. (2018) model, removing the need for the Kaimal fallback.

Comment:

> *SU1. A potential weakness of the proposed method is that the correction applied to the spectra on hourly timescales is based on observed spectra, which requires measurement data. This somewhat limits the general applicability of the approach. It may be worth exploring whether these parameters could instead be derived directly from the spectra of the NORA3 time series, which would strengthen the method.*

Author Response: This is indeed an important limitation, and it is now discussed in Sect. 4.4 together with the applicability of the method to other locations. We show there that the variation in the a_3 parameter between Horns Rev (Larsén et al., 2013) and FINO1 is small enough to justify using a single value, $a_3 = 3 \times 10^{-4}$ [SI units], across sites. The a_4 parameter, which sets the range over which the hourly mesoscale model is fitted, is the one most affected by a change of location according to our findings, and we recommend fitting it directly to the hindcast (e.g. NORA3) spectrum at the site of interest.

Comment:

> *SU2. The time series generation approach using the splicing method combined with the Veers method is relatively complex. For a single wind component, time series could alternatively be generated using an inverse Fourier transform, which is likely computationally cheaper. However, the presented splicing approach has the important advantage of extending naturally to three-dimensional fields with time-varying turbulence. This makes it particularly valuable for applications requiring full 3D input, such as load simulations. If the authors agree, this advantage could be emphasized more clearly.*

Author Response: The introduction has been revised to better convey the overall motivation behind this work and why the Veers method is used, explicitly linking it to aeroelastic turbine and wind farm simulations and highlighting, as the reviewer suggests, the advantage of the splicing approach for three-dimensional, time-varying turbulence fields.

Comment:

> *SU3. Since it is argued that atmospheric stability is not well represented in the NORA3 dataset at several locations, it would be helpful to also include the distribution of stability in Fig. 1.*

Author Response: The distribution of z/L at FINO1, compared between the sonic anemometers and NORA3 over the period 2007–2008, is presented in Pauchet and Chabaud (2025, Fig. 4.1), which evaluates NORA3 surface parameters at this site; a reference to this has been added to the manuscript (Sect. 2.1.2) rather than reproducing the figure.

Comment:

> *TC1. Line 35 and 105: Consider that the formulation of the Kaimal model in the IEC standard is a modified version of the spectrum published in Kaimal et al. (1972).*

Author Response: The manuscript has been updated to acknowledge that the IEC formulation of the Kaimal spectrum is adapted from Kaimal et al. (1972).

Comment:

> *TC2. Line 41: "The latter may be resolved [...]": The word "resolved" may be misleading; "parametrized" is more appropriate in this context.*

Author Response: Agreed; "resolved" has been replaced with "parametrized".

Comment:

> *TC3. Line 48: "[...] and the novel splicing methodology will be introduced [...]": This is the first mention of the splicing methodology, and the reader does not yet know what it is; could it be briefly introduced here (or omitted at this point if preferred)?*

Author Response: Corrected to: "and the novel methodology to ensure a linear spectral transition between two consecutive time series will be introduced".

Comment:

> *TC4. Eq. 4: The variable Lv needs to be defined.*

Author Response: L_v, the latent heat of vaporization, has been explicitly defined where it first appears (Eq. 4).

Comment:

> *TC5. Line 93: Regarding the application of the high-frequency filter, it would be helpful to already motivate its use here.*

Author Response: The high-frequency filter is applied to make the FINO1 observations directly comparable to the highest frequency resolved by the microscale model (0.125 Hz); this motivation is now stated where the filter is first introduced (Sect. 2.1.2, line 93).

Comment:

> *TC6. Line 107: Since atmospheric stability is also relevant for onshore applications, it may be misleading to state "[...] being based on onshore observations and thus not accounting for atmospheric stability."*

Author Response: Agreed; the causal "thus" has been removed, since atmospheric stability is also relevant onshore. The sentence now only states that the referenced spectra are based on onshore observations.

Comment:

> *TC7. Line 117: Note that the mesoscale spectrum in Larsen et al. (2016) is independent of n/u* and differs from the form used in Cheynet et al. (2018). It would be helpful to add a note on this here.*

Author Response: A footnote has been added linking the mesoscale spectrum coefficients of Larsén et al. (2013) to those used by Cheynet et al. (2018).

Comment:

TC8. Line 137: What is meant by "the widest spectrum"?

Author Response: Agreed, "widest" was not the clearest term; it has been replaced by "largest spectral range", since the objective is to resolve as much of the high- and low-frequency content as possible.

Comment:

TC9. Line 140–141: "A sampling interval of 1s is chosen with a highest simulated frequency of 0.125 Hz yielding minimum 8 points per period." The meaning of the sampling interval, simulated frequency, and period is not clear; could this be clarified here, or a reference to documentation be provided?

Author Response: A reference to the latest FLAggTurb release and its user manual has been added at this point for clarification.

Comment:

TC10. Lines 238–239: A 30° calibration offset is rather large; it seems more likely that the modeled wind direction is not perfect.

Author Response: Since the wind direction used as forcing comes directly from NORA3, a systematic offset of this magnitude would be surprising over open sea, far from any topographic features that could deflect the flow. We therefore concluded that the offset originates from a calibration issue in the sonic anemometer heading, whose settings were changed during 2007–2008.

Comment:

TC11. Line 305: This statement is rather strong and may be misinterpreted. Please clarify that it applies specifically to turbulence intensity estimates from lidar measurements, and briefly include details of the measurements in St. P ́e et al. (2021). The effect of the applied low-pass filtering on the error statistics should also be acknowledged.

Author Response: The statement has been reworded to clarify that it refers specifically to turbulence intensity estimates from lidar measurements. We also now specify that St. Pé et al. (2021) is based on raw lidar-to-cup-anemometer comparisons at multiple sites, without any post-processing or adjustment methods, which is an important distinction from our filtered, processed dataset.

Comment:

TC12. Figures 5 and 6: The axis label is given as u [m/s], while I understand the figure to show friction velocity u.*

Author Response: The axis labels are correctly $\overline{u}$ and u_* in the source figures; the apparent plain "u" is a rendering artifact of the PDF viewer, which we will double-check before final submission.

Comment:

TC13. Figure 5: Clarify whether stability, wind direction, and friction velocity are taken from FINO1 data or from NORA3. Further, σ in "red arrows indicate values exceeding 5σ [...]" may be confused with σ_u, σ_v, and σ_w.

Author Response: The wind direction, atmospheric stability, and friction velocity shown on the secondary (right) axis are taken directly from the FINO1 81 m sonic anemometer measurements, as now stated explicitly in the caption of Fig. 6 (figure numbering has shifted slightly during revision; this is the time-series figure referred to in this comment). The caption has also been reworded to avoid confusion between the exceedance threshold and the velocity-component standard deviations, now reading "5 times the standard deviation of the displayed series" instead of "5σ".

Comment:

> *TC14. Line 113: f is defined as the normalized frequency $f = nz/u$, following the notation in Cheynet et al. (2018); Kaimal et al. (1972). This differs from Fig. 4 and Fig. 10, where f is used as frequency in units of s-1 , as in ??. Please check whether the notation is consistent throughout the manuscript.*

Author Response: n is the frequency measured in Hz, and the reduced frequency $f = nz/\overline{u}$ is dimensionless. Figure 10 indeed used the wrong notation, which has been corrected.

Comment:

> *TC15. Figure 12: The discussion of Figure 12 is relevant to the manuscript; however, in my view the figure itself does not add substantial value and could be omitted. The final decision is of course up to the authors. If it is included, a colorbar should be added.*

Author Response: We agree that the figure does not, by itself, add to the discussion, but it remains the basis for the numerical values quoted in the main text. We have therefore moved it to Appendix A (Fig. A2) to keep the discussion section lighter. We chose not to add a colorbar there, as the color scale only encodes the density of points in the distribution and does not carry additional information relevant to the discussion.

Comment:

> *TC16. In the in-text citations, the publication year is often omitted (e.g. Lines 98 and 214), which makes them harder to read*

Author Response: The manuscript has been updated to include the year in in-text citations throughout. As previously indicated, we kept author-only citations (without a year) only for the Kaimal et al. spectrum and the Cheynet et al. model, to keep the manuscript a little lighter when referring to these two models repeatedly.

Comment:

> *TC17. Fig. 9: The caption does not describe all terms; in particular, what does "FT + N3" refer to? The citation of Jeans (2024) could also be added to the caption.*

Author Response: The citation is indeed important and has been added to the caption. "FT + N3" corresponds to FLAggTurb + NORA3, i.e. the modeled data; this explanation has also been added to the caption of Fig. 9.

4 Anonymous Referee #3 (RC3)

We thank Anonymous Reviewer 3 (2026) for the constructive comments. The following responses address each point raised.

Comment:

> *The paper describes the stochastic generation of ABL turbulence time series at specified points from microscale spectra and NORA3 data using methods previously published by one of the authors (Chabaud 2024a and 2024b), the focus of this work is on the experimental validation of the methods using data from the FINO1 weather mast. The extension of the turbulence spectrum towards lower frequencies is very relevant and necessary for the prediction of fatigue loads using aero-elastic simulations.*

Author Response: We thank the reviewer for this accurate summary and for recognizing the relevance of extending the turbulence spectrum towards lower frequencies for fatigue load prediction.

Comment:

> *Perhaps a general criticism of the applied methods from the perspective of the industrial application, which should not affect the decision of publication of this work: the described method does not preserve the spatial structure of an incompressible flow; it generates one-point time series, but no solenoidal 3D velocity fields (this compromise is also mentioned by the authors, e.g. . We as a wind turbine manufacturer have experimental evidence from instrumented turbines indicating that using divergence-free velocity fields model has a significant effect on certain fatigue loads, with the DEL predictions of the divergence-free turbulence (e.g. Mann (1994)) being higher than that of stochastic spectral models without that property (Kaimal et al. (1972), very similar spectra and identical TI otherwise). Similar observations can be made when the components of an initially divergence-free velocity field are scaled (resulting in $div\vec{u} \neq 0$) to model inhomogeneous turbulence. For wind turbine loads predictions, it would be very interesting to see if the e.g. the Mann (1994) or Syed and Mann (2024) models can be adjusted/constrained using NORA3 data. The authors do provide a very short discussion on that in Sect. 4.3 to support their choices made in the paper. Again, this is a general comment and outside the scope of this work, and should therefore not affect the decision for publication.*

Author Response: We agree that this point deserved clearer framing. The introduction has been revised to better position this work within the broader context of 3D wind fields and wind farm/turbine simulations. We would like to stress that the focus of our toolset is computational efficiency at farm scale, whereas Syed and Mann (2024) targets higher fidelity at turbine scale, at a considerably higher computational cost; the two approaches are complementary rather than competing.

Comment:

> *A comment on the writing style: there is a certain level of textual "bloat" (for instance, the description of the spectral splicing method is fully from Chabaud 2024b and covers almost two pages in this work) and a rather high dose of marketing vocabulary ('explore this "terra incognita"', 'The power of FLAggTurb', 'The absence of a clear spectral gap underscores the importance of the presented splicing methodology'). The latter might be due to the use of Generative AI for writing the paper, as stated*

by the authors. These are the main reasons for my evaluation of the presentation quality as "fair" only.

Author Response: The manuscript have been proofread and the phrasing has been improved.

Comment:

The description of the statistical and spectral validation methods in 2.3 are very good, and from what I can see the main contribution of this work over Valentin Chabaud (2024); Chabaud (2024). I agree with the choice of unnormalized spectra for comparing FINO1 time series to those predicted by the presented stochastic method, but perhaps the argument argument against using Cheynet 2018 normalization could be worked out a bit better: as far as I can see, Cheynet et al. (2018) only discusses the relative normalization of the component spectra S_w/S_u, not the weighting of different parts of the spectrum (i-iv) relative to each other. I guess that is meant by "lacks meaning outside the microscale range".

Author Response: The normalization used by Cheynet et al. (2018) follows the Monin–Obukhov similarity theory, which is only applicable within the microscale range. Since we consider periods well beyond what can be considered "microscale", this normalization loses its physical meaning outside that range, which is what we intended to convey; the wording has been clarified accordingly.

Comment:

In Section 3.1, I agree with the conclusions of the authors that the method reproduces the histogram of the longitudinal velocity component u (Fig 5A) and the friction velocity (Fig 5F). I do not fully agree with the "excellent agreement" for the standard deviations for the three velocity components, though. The authors correctly point out the "battlement" (jagged or peaky histogram). The authors attribute this to the binning in the friction velocity (estimated by equation (2) from NORA3 data) and atmospheric stability (estimated by equation (1) from NORA3). It would be nice to have more details on how the NORA3 data resolution affects the binning here. Sect 4.1 discusses this point a bit further (influence of refining the binning), but battlement also remains for the fine binning. Being on a "hill top" or in a "valley" due to some binning in the data upstream of the parameter estimation can significantly impact the usability of the proposed method for the estimation of turbine loads that are highly sensitive to the standard deviation of the velocity components.

Author Response: Indeed, the observed battlements are directly related to the binning applied to reduce the number of simulations required from FLAggTurb. An evaluation of how this affects the accuracy of the results is given in Sect. 3.1, using the 2-hour mean error to derive a confidence interval for the generated time series: for the turbulence intensity of the u component, the default (coarse) binning gives a 95% CI of $[-0.032, 0.031]$, which illustrates in practice the impact of being on a "hilltop" or in a "valley" of the binned parameter space raised by the reviewer. This is further explored in Sect. 4.1 (Fig. 11), where refining the friction-velocity binning is shown to reduce, though not eliminate, the battlement effect.

Comment:

It is interesting to see in Fig 6D that the vertical velocity fluctuations are underestimated vs. the two velocity components parallel to the ground. The vertical velocity *component is usually computed from the 3D continuity equation, and corresponds to the deviation of the 2D divergence of the flow in a plane from zero. It seems that the intrinsic divergence of the flow field generated by the Veers method takes its toll here, otherwise the low-frequency fluctuations of u,v and w would be coupled.*

Author Response: This is indeed one of the flaws of our generation process: the vertical wind velocity is not derived from NORA3, so the low-frequency vertical wind component is completely absent. This likely explains the underestimation, as the vertical wind spectrum for frequencies below 1/3600 Hz is entirely ignored.

Comment:

The second part of Sect. 3.1 (MBE and RMSE) is very good. Besides for the wind speed bins ¡ 5 m/s, the proposed method has a low bias and an acceptable level of RMSE, the latter about twice the limit for wind speed measurements defined by IEC 61400-50. That is very good for such a stochastic method, and would make it suitable for site-specific fatigue life time estimates. If possible, I would appreciate a discussion for the deviations at bins ¡ 5 m/s; this velocity range is increasingly important for larger rotors due to the accumulated fatigue damage during idling.

Author Response: We added a short discussion on the increased RMSE for wind speeds below 5 m/s. We think that the larger deviations are mainly attributed to the higher sensitivity of turbulence intensity at low mean wind speeds, the slight low-wind-speed bias of the forcing data, and discretization effects in the microscale model. We agree that this wind speed range is important for modern large-rotor turbines due to fatigue accumulation during idling.

Comment:

The spectral parameters of the Cheynet 2018 model are obtained from a fit to the FINO1 measurements, the same test site is used to obtain time series for the validation of the model. The presented validation should therefore be seen as a best-case scenario, where the spectral parameters are known from the experiments and are used together with the NORA3 hindcast data. Additional spectral parameter estimates are needed for a3 and a4, which were obtained by fitting the Larsen 2016 model to the FINO1 data. In practice, using NORA3 data alone will require an estimation of these parameters at a specified location. Perhaps the authors can comment on either the generality of their parameter estimates, and/or on how these can be obtained from NORA3.

Author Response: This is indeed an important point that had been overlooked in our discussion. It is now addressed in Sect. 4.4, together with the related comments on generalization to other locations and models (RC2-SC2, RC2-SU1).

Comment:

Sect 4.3 discusses the use of the Veers method vs. Syed and Mann (2024), again using rather bold rhetoric. I would challenge the statement that lifting Taylor's frozen turbulence hypothesis is more important than discarding the divergence-free structure of a 3D incompressible flow. At least from the perspective of predicting wind turbine loads, the contrary is the case. The authors also use a 1m rotor size to validate their model against a point spectrum in FLAggTurb, removing the low-pass filtering of large rotors (Sect. 2.2.2). Notwithstandingly, the authors provide a valid argument about the difficult parameter estimation for the Syed and Mann (2024) method.

Author Response: As discussed in our response to the second comment, the apparent disagreement stems from the different core applications and validation targets of the two approaches. Sect. 4.3 has been revised to better reflect this distinction and to temper the comparative language flagged by the reviewer. We thank the reviewer for acknowledging the practical difficulty of parameter estimation for the Syed and Mann (2024) method.

Comment:

> *Overall, I would suggest publication conditional to a minor review. The authors recycle a large part of their methods section from their previous work (Valentin Chabaud (2024); Chabaud (2024)), new is the validation against FINO1 time series and spectra. As some spectral parameters of the Cheynet et al. (2018) model were obtained from FINO1 fits, the work represents to some degree a circular closure and the generality of the approach for an arbitrarily selected location in the NORA3 domain is left in doubt. This is also a practical limitation for the implementation of the proposed method in an industrial tool chain. I would suggest that the authors address this point in a revised manuscript, as well as the comments above. I furthermore expressed some disagreement with modelling choices and statements made by the authors, but I am certainly biased there as well and would not consider that as prohibitive for publication. If there is time for some textual improvements, I would strongly suggest a revision of the AI-generated text in some parts of the paper.*

Author Response: We thank the reviewer for the constructive assessment and for recommending publication subject to minor revisions. We have revised the manuscript throughout to address the points raised above, including a new discussion (Sect. 4.4) on the generalization of the fitted spectral parameters and the applicability of the method beyond FINO1 and NORA3, which also speaks to the circularity concern raised in this comment. The manuscript has also been proofread and revised to address the language issues noted; the use of generative AI tools for language and phrasing assistance is disclosed in the Acknowledgements, and all content has been reviewed and validated by the authors.

5 Xiaoli Larsén (EC1)

We thank Larsén (2026) for the constructive comments. The following responses address each point raised.

Comment:

> *1- The title mentions "for offshore wind farm simulation" - is the concept of wind farm included in the analysis, and if yes, what aspects? For instance, is wake effect included in the NORA3 data?*

Author Response: The introduction has been revised to present the overall picture more clearly. Wake effects are not directly included in NORA3 or in the present analysis; rather, the long mesoscale spectra characterized in this work are an important input for wind farm simulations that do resolve wake effects, including wake meandering driven by low-frequency direction changes.

Comment:

> *2. Please include the year in some citations e.g. Cheynet et al. and Larsén et al.*

Author Response: We fixed the citations that were missing the year. The only exception is when referring to the Kaimal spectrum, where we use an author-only citation (without the year) to keep the manuscript a little lighter while retaining the hyperlink to the reference.

Comment:

> *3. How was the along-wind component obtained from the time series? How long is the window/segmentation for calculating the spectra in e.g. Fig. 10? How many segmentations are there in total? This is related to the reliability of the u-spectra from measurements presented in Fig 10. Please compare the u-spectra and wind speed spectra - this can serve as a quality check of the success of wind vector decomposition into u and v.*

Author Response: The along-wind component is obtained by maximizing u from the high-frequency wind speed and direction output by FLAggTurb; this has been clarified in the manuscript. As indicated in the methodology, each individual spectrum is calculated using a single Hamming window of 24 h, with zero-padding to five times the number of points (nfft = 5) to ensure sufficient resolution at low frequencies; the spectra are then averaged using log-smoothing. The number of individual 24 h spectra averaged has been added to Fig. 10. We compared the wind speed spectrum from the 10-min observation dataset with the spectrum of the u component (both raw and filtered at the sampling interval used in the modeling). The two spectra are coherent with each other; we do not think including this comparison would add value to the manuscript, but we are happy to share it with the editor if useful.

Comment:

> *4. Fig 10: is there a reason we have spectra from measurements at 81 m, but spectra from models at 100 m? Please unify it.*

Author Response: We use data at 100 m from NORA3 as forcing for the model, and unfortunately no observations are available at 100 m in the North Sea. We therefore chose to keep the two different heights, as the spectral variation between them is limited, as shown by Larsén et al. (2016).

Comment:

> *5. How were the spectra from Larsén et al. (2016) obtained? Larsén et al. (2016) merges the mesoscale spectra and the microscale spectra, and the demonstration from their study used measurements, which accordingly suggests for f¿0.001 Hz, the black dashed curve should match measurements. Unless the authors are referring to Larsén et al (2021) (https://doi.org/10.1007/s10546-020-00575-0), where microscale turbulence spectral models are proposed, which should be relevant reference to the current paper.*

Author Response: The black dashed line shown in Fig. 10, labeled "Larsén et al. (2016)", is taken from the observational wind speed spectrum at 100 m presented in Fig. 3 (panel d) of Larsén et al. (2016). The separation between stable, neutral, and unstable cases, together with differences in spectral calculation methodology, are the main sources of mismatch between the spectra.

References

Anonymous Reviewer 1. Comment on wes-2026-58. April 2026. URL https://wes.copernicus.org/preprints/wes-2026-58#RC1.

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