Reviewer comments on "Synthetic generation of long turbulent wind time series using hindcast model forcing for offshore wind farm simulation"

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1 General comments

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.

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.

2 Specific comments

1. 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.

2. 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.

3. 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.

4. 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.

5. 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?

2.1 Suggestions

In the following are some well-meant suggestions for the authors to consider:

1. 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.

2. 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.

3. 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.

3 Technical corrections

1. 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).

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

3. 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)?

4. Eq. 4: The variable L_v needs to be defined.

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

6. 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."

7. Line 117: Note that the mesoscale spectrum in Larsén 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.

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

9. 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?

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

11. 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. Pé et al. (2021). The effect of the applied low-pass filtering on the error statistics should also be acknowledged.

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

13. 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.

14. 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.

15. 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.

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

17. 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.

References

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