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Wind Energy Science

Date: October 29, 2025
Subject: WES-2025-106 Response to Reviewers

Dear Reviewers,

We would like to express our sincere gratitude for the time and effort you devoted to evaluating our manuscript. We are especially thankful to Referee #1 for the additional, constructive feedback provided during this second review round, as well as to both reviewers for their earlier comments, which have greatly contributed to improving the quality and clarity of our work.

In response to the new comments, we have further revised the manuscript, adding clarifications and supplementary details where needed. We believe these additions have strengthened the paper and addressed all remaining concerns.

Below, we provide a detailed, point-by-point response to the comments received, indicating how each has been addressed in the revised version of the manuscript.

On behalf of all authors,
Sincerely,

Alessandro Fontanella

Response to Referee #1 comments

Line numbers in the referee comments refer to the second version of the manuscript.

General comments

R1: I am not sure you can estimate the rotor equivalent velocity under wake conditions using the thrust curve estimated for a uniform freestream incoming wind velocity. The presence of the wake leads to radial shear over the rotor and higher turbulence intensity, which lead to completely different aerodynamic performance of a turbine rotor than for the case with a uniform freestream incoming wind field. This is also mentioned as well, for instance, in the IEC standards for the experimental characterization of power/thrust curves: "wind directions with a potential of wake interaction should be neglected for the characterization of the power curves". Even though measurements of the incoming wind are prohibitive, a direct measurement of the RPM of the rotor should be needed, in my opinion.

A: We thank the Reviewer for this comment. We agree that a turbine operating in a wake experiences a non-uniform inflow, characterized by both radial shear and increased turbulence, and that this differs from the uniform flow used to generate conventional thrust curves. However, our intention in introducing the rotor-equivalent wind speed (U_{RE}) was not to recreate the real inflow conditions, nor to estimate a power/thrust curve in the IEC sense. Instead, U_{RE} is used to prescribe the operating point of the downstream rotor in a consistent manner across configurations.

Specifically, U_{RE} represents the wind speed of a uniform inflow that would generate the same *mean thrust* as the waked inflow. It is therefore a thrust-equivalent velocity, not a physical measurement of the heterogeneous incoming flow. This approach is consistent with common practice in wind-tunnel wake studies, where downstream turbines are operated at a prescribed TSR to isolate wake-induced aerodynamic effects without introducing closed-loop control dynamics.

Regarding the Reviewer's suggestion of a direct RPM measurement, in our experiment the RPM of WT2 is not the unknown to be inferred from inflow conditions, but rather a controlled input, prescribed in open loop to achieve the desired operating point. Since the generator torque is not actively regulated in real time, measuring the RPM would not provide additional knowledge of the inflow. The relevant aerodynamic quantity for our scope is the resulting thrust and torque, which we do measure directly.

We agree that our original text may have been too concise and could lead to the interpretation that U_{RE} is meant as a physical reconstruction of the inflow field, which is not the case. We have revised the manuscript to clarify the role of U_{RE} and to state explicitly that it is used only to define a consistent operating point for WT2, not to characterize wake. This clarification also avoids confusion with the prescription in IEC standards, which concern field-based power curve characterization and are not applicable to the present thrust-equivalence operating procedure.

R1: The second point, which is a kind of puzzling, is the occurrence of speedups (power increase up to ~30% of the freestream unperturbed case) when the turbines are misaligned. The fact that the speedup increases moving the downstream turbine in the transverse direction from 0D, to 0.5D and 1D, and in the streamwise direction from 3D to 5D, in my opinion are all signs that the models within the wind tunnel cross-section create a large blockage factor and large confinement, especially in the vertical direction, leading to this speedup. If my discussion is not correct, then the authors should discuss this in the manuscript and provide experimental evidence.

A: We thank the Reviewer for raising this general point. In our experiments, the increase in WT2 power output is primarily due to its progressive avoidance of the WT1 wake, as the downstream turbine moves laterally, away from the wake core. At a 1R offset the rotor is only partially immersed in the wake, and at 1D it is almost fully exposed to the freestream, which explains the observed power increase without invoking large tunnel blockage effects. This mechanism is now better clarified in Sect. 3.1.2 through small additions to the text.

We acknowledge that some speed-up of the inflow also occurs due to wind-tunnel blockage. The flow accelerates from the 4 m/s freestream to approximately 4.2 m/s at the first rotor (+5%), and a further acceleration is expected in the section where WT2 is located. Although this second speed-up is not directly measured, the estimated increase of about 0.3 m/s based on U_{RE} is consistent with the expected blockage trend. As discussed in the Discussion, blockage effects influence the absolute power levels and prevent a direct one-to-one extrapolation to full scale. However, the dominant wake mechanisms and their trends remain representative, which is consistent with the broader literature on wind-tunnel wake studies. Ultimately, full-scale data will be required in the future to validate wake–interaction studies under realistic offshore conditions.

2.1 Experimental setup

R1: Sect. 2.1 – Can you specify the distance of the rotor top tip from the ceiling of the test section and comment if you anticipate any effects on the generation and evolution of the tip vortices?

A: We thank the Reviewer for raising this point. The distance between the blade tip and the wind-tunnel ceiling is approximately 0.5 m, corresponding to 0.2D. Previous studies on the same turbine in the same facility (https://doi.org/10.5194/wes-8-465-2023) have shown that this relatively small clearance can slightly inhibit the vertical expansion of the wake, especially in the near wake, and may therefore influence the evolution of tip vortices and local induction. In particular, numerical–experimental comparisons demonstrated that CFD models including the wind-tunnel ceiling reproduce the measured wake expansion more accurately than those neglecting it.

However, these effects were found to be small and did not change the fundamental wake dynamics or the qualitative behavior of the flow structures. Since the focus of the present study is on relative variations induced by platform motion, rather than on the absolute characterization of wake expansion, in our opinion the ceiling proximity does not affect the interpretation of our results. To clarify this, we have now added the rotor–ceiling distance and a brief comment in Sect. 2.1.

R1: L 171 – 173 – I believe it is important to summarize this methodology here for the sake of completeness.

A: We thank the Reviewer for this suggestion. We agree that briefly summarizing the load-correction methodology improves the readability and completeness of the paper, without forcing the reader to consult the cited reference. We have therefore added a concise description of the procedure used to remove inertial and gravitational contributions from the measured loads of WT1, while still keeping the full details in Fontanella et al. (2022a).

2.1 Wind turbine operating settings

R1: L 184 – Please add the resulting Reynolds number.

A: We thank the Reviewer for this suggestion. We agree that indicating the Reynolds number at the operating wind speed improves clarity. In the revised manuscript we now report, at the same location where the freestream velocity $U = 4.0\ \mathrm{m\,s^{-1}}$ is introduced, that this corresponds to a chord-based Reynolds number of approximately 1×10^5 at mid-span. This value is consistent with the Reynolds number range already discussed in Sect. 2.1.

R1: L 196 – You do not need a new paragraph for this sentence.

A: We thank the Reviewer for the suggestion. The sentence has now been merged into the preceding paragraph as recommended.

R1: L 208 – Does this information on wind velocity contradict what already reported in L 184? Please clarify.

A: Thank you for pointing this out. The statement at L184 refers to the wind speed used for *all the tests discussed in the paper*, except for one additional measurement carried out at $2.4\mathrm{ms}^{-1}$, which is mentioned at L208. We have now clarified this in the revised manuscript to avoid any misunderstanding.

R1: Line 210: At 2.4 m/s, tip-speed ratio is 7.78 (not 7.5) using the values mentioned in the study. It may be a good idea to represent the uncertainty or range bound on the tip-speed ratio and wind speed measurements.

A: We thank the Reviewer for this remark. The Reviewer is right in noting that the ideal tip-speed ratio of 7.5 at 2.4 m/s would correspond to a rotor speed of approximately 144 rpm. In the experiment, however, the speed was set to 150 rpm for simplicity of operation of the turbine hardware, resulting in a deviation of about 4% from the nominal TSR. In practice this small deviation from the nominal TSR has a negligible influence on the results. We have clarified this aspect in the revised text.

R1: L 214 – The estimate of the rotor effective velocity from the thrust coefficient sounds a bit unusual considering the typical accuracy in the thrust measurements and the presence of the incoming wake. Did you measure at least the rotational velocity, e.g., with a tachometer?.

A: We thank again the Reviewer for raising this point. The rotational speed of WT2 is indeed measured by a magnetic encoder. However, because the turbine is operated in open-loop and the generator torque is not actively regulated, the rotor speed is prescribed and not an output driven by the aerodynamic inflow. For this reason, the RPM signal does not provide additional information about the incoming flow. Instead, the operating point of WT2 in waked conditions is defined through the rotor-effective wind speed U_{RE}, which is a thrust-equivalent velocity derived from the measured thrust. We emphasize that U_{RE} is not intended as a reconstruction of the heterogeneous inflow, but as a practical and reproducible way to impose a consistent operating point across configurations.

R1: Table 2 – Is it possible to add other 2 columns with the thrust coefficients of the two turbines? This information is crucial for the reproducibility of the experiment.

A: We thank the Reviewer for this suggestion. Following the request, we have updated Table 2 by adding the average thrust force and corresponding thrust coefficient for the downstream turbine (WT2), which vary with the wake conditions and are therefore relevant for reproducibility. We did not add the thrust coefficient of WT1, as its operating point is kept constant across all configurations and its thrust force is already documented in Sect. 3.1.1.

R1: Line 215: TI is higher in the wake and there is a radial (lateral) variation or gradient of the incoming velocity. As a result, the turbine in the wake experience higher inflow at rotor tip and lower inflow in the wake. However, the BEM model used for calibration uses freestream conditions. How do radial variations of wind speed and TI affect accuracy on the estimation of U_{RE} from the thrust measurements for the cases with an upstream wake?.
A: We fully agree that a downstream turbine operating in the wake experiences a non-uniform inflow, with both increased turbulence intensity and a radial velocity gradient. These effects indeed imply that the downstream rotor is not aerodynamically equivalent to a rotor in uniform freestream conditions. However, we stress that U_{RE} is not used as an aerodynamic reconstruction of the inflow, but as a metric to define the operating condition. By definition, U_{RE} is the velocity of a uniform inflow that produces the same mean rotor thrust measured in the wake. As such, it does not attempt to reproduce the detailed inflow distribution, and it is not used for modeling turbine aerodynamics.
Because our objective is to ensure a consistent and comparable operating point across configurations, rather than to infer the local inflow via BEM under waked conditions, the use of the freestream-calibrated thrust map is appropriate. Any radial non-uniformities of inflow and turbulence are inherently embedded in the measured thrust and therefore do not bias the definition of U_{RE} for this purpose. In other words, U_{RE} transfers waked conditions into an equivalent freestream reference only at the level of integral thrust, which is the quantity relevant to our operating-point definition.

R1: Line 221: It is mentioned previously that the blockage (speed up) effects of WT1 makes the wind speed equivalent to 4.2 m/s. URE presented in the table 2 shows value up to 4.5 m/s. Is this the error from the calibration or the blockage of the second turbine? It needs to be justified.
A: We thank the Reviewer for this observation. The difference between the 4.2 m/s measured in the free stream between WT1 and WT2 and the higher values of U_{RE} reported for WT2 is due to the additional blockage induced by the downstream turbine. While the Pitot measurement reflects the local acceleration caused by the presence of WT1 alone, the flow at WT2 can experience a further speed-up when WT2 is placed in the tunnel and operated at its thrust setpoint. This explains why the equivalent velocity can exceed 4.2 m/s and does not indicate a calibration error.

2.3 Platform motion scenarios

R1: L 231 – What do you mean for unidirectional sinusoidal motion? Please clarify.
A: We thank the Reviewer for pointing out this ambiguity. Our intention was to distinguish sinusoidal platform motions from realistic, irregular wave-induced motions. Since in our campaign sinusoidal tests include both single–degree-of-freedom motions and combined surge–sway motions, we have removed the term *“unidirectional”* and clarified the sentence accordingly.

3 Results

R1: L 310 – Can the authors comment on why they ignored completely in this work the discussions on the power performance of the turbine rotors, in terms of RPM and power coefficient? In literature, it has been shown that Cp, specifically, can be a more realistic

parameter than Ct due to the non-similarity of many system components, e.g., tower and nacelle.
A: In our setup the turbines operate at a fixed prescribed rotor speed, so the power is directly proportional to the measured aerodynamic torque. For this reason, we focus on thrust and torque as primary indicators, and the power performance is already discussed through these quantities (e.g., Fig. 4) and through the WT2/WT1 power ratios reported in Fig. 5. In contrast, an analysis of RPM trends is not relevant here, since the turbine speed is not a response but an imposed operating condition. Regarding the use of C_p, we note that in open-loop fixed-speed operation, C_p and torque carry the same information. Moreover, thrust and C_t are directly connected to the wake deficit and the coupled aero–hydro–servo dynamics that are the focus of this work. For this reason, we adopted C_t and torque as the most meaningful and non-redundant performance metrics in the present context.

R1: Line 345: The shear in the wake usually leads to higher flow in the outer region of the rotor (which is more sensitive to the overall operation of the wind farm). Hence, how a performance of a wind turbine for the wake and freestream (average of 2.4 m/s) cases can be compared?
A: We agree with the Reviewer that a waked inflow with radial shear does not lead to the same aerodynamic behavior as a uniform inflow with the same average velocity. Our intention in this part of the text was not to compare the turbine performance in waked and freestream inflows, but simply to verify that WT2, when placed at 5D in the absence of a wake, operates in a uniform inflow and is not affected by end–of–test-section boundary effects. The 2.4 m/s case was therefore used only as a consistency check for uniform-flow conditions, not as an aerodynamic analogue of the waked inflow. We have clarified this in the revised text to avoid misunderstanding.

R1: L 355 – the title of this section is a bit ambiguous. What do you mean for energy recovery? Please consider providing a clearer title.
A: We thank the Reviewer for this suggestion. The term "energy recovery" could indeed be interpreted ambiguously. To improve clarity, we have renamed the section "Effect of platform motion on the power output of the downstream turbine" which more directly reflects the content of the analysis.

R1: L 363 – I am not sure you described so far how you estimate power capture for this experiment. Please address this point or add required information.
A: We thank the Reviewer for this comment. The procedure to compute the aerodynamic power is already described in Sect. 3.1.2, where we state that, since the rotor speed is prescribed and constant, the power is obtained directly as the product of the measured aerodynamic torque and the rotor speed (i.e. $P = M_x \omega$). To improve clarity, we now reference this point earlier in the text.

R1: Caption of Figure 5 – "maximum time-averaged…", this seems a contradiction, maximum or average? Please clarify.
A: We thank the Reviewer for this remark. In the revised caption we have clarified that the plotted values represent the time-averaged WT2 power, and that for sinusoidal motions the value shown is the maximum among all tested combinations of frequency and amplitude for each motion type. We believe this updated wording resolves the ambiguity between "maximum" and "average" in the previous version of the caption.

R1: L 367 – 379. Can you summarize these results in a figure or a table for better interpretation of the results. You should provide these results for all the configurations investigated, not only for a subset.
A: Agreed. A complete summary of the WT2 power response for all motion cases and farm configurations has now been added in Appendix C. This appendix reports the time-averaged power values (including all frequencies and amplitudes for sinusoidal motions) together with the corresponding WT2 rotor speeds, providing full transparency and reproducibility of the results. In the main text, Fig. 5 continues to show only the most representative cases for clarity.

R1: L367 – 377 – Can you assess/compare these values from previous works or predictions from engineering wake models?
A: A comparison with previous works is already provided in this section. In particular, in lines 370–375 we compare our fixed-turbine results to full-scale values from Shen and Mikkelsen (2011), highlighting that the lower downstream power levels (11–18% at 3D–5D) are due to the combination of very low inflow turbulence and a high thrust-coefficient rotor, which limits wake recovery in our setup. This trend also agrees with the findings of previous wind-tunnel studies on floating turbines (e.g., Figure 17-18 from https://doi.org/10.1002/we.2896) where similarly persistent wake deficits were observed under low-turbulence conditions. For this reason, we believe that probably no further comparison is required, and the values reported here are consistent with the expected wake behavior.

R1: L 378 – 380 – How do you justify this speedup leading to a 20% extra power at 3D and becomes even larger at 5D?
A: The Reviewer's comment is interesting. The increase in WT2 power with lateral offset is not solely due to local flow acceleration, but primarily to the progressive exposure of the downstream rotor to the freestream as it moves out of the wake. At an offset of 1R, only part of the rotor is still within the wake, while at 1D the rotor is essentially outside the wake and sees freestream conditions, which explains why the WT2 power can locally exceed that of WT1. This physical mechanism is already described in the manuscript (Sect. 3.1.2), and we have clarified that the observed increase is related to this partial and then total wake avoidance, rather than to speed-up alone.

R1: L 385 -387 – "power increase of 26%…24%". I believe that presenting the results in this way is misleading. The percentage difference for the case 3D is large just because the initial value for fixed conditions is very low compared to the other cases. In my opinion, a more rigorous way to present these results is to calculate the respective power coefficients, which is a universal meaningful parameter. Then, the results should be presented as difference from the Cp of WT1, and for the dynamic cases as difference with the fixed case. At the end, it is obvious from the data that the effects due to the WT1 motion are negligible with respect to the fixed wake interaction for all cases. Therefore, mentioning "26%" is only misleading.
A: We thank again the Reviewer for this observation. We acknowledge that the 26% relative gain occurs in a configuration where the baseline downstream power is very low due to the highly persistent wake under low-turbulence and high-Ct conditions. For this reason, the percentage increase should be interpreted together with the absolute power levels.
Presenting the results in terms of Cp would not change the interpretation (since Cp2/Cp1 = P2/P1 when referenced to the same undisturbed wind speed U), and a Cp definition based on the waked inflow is not practical, as the rotor-averaged wind speed is not directly measured. To avoid possible misinterpretation, we have clarified in the manuscript (abstract, discussion and

conclusions) that the 26% gain is associated with a very low initial power and that the absolute gains remain modest.

R1: Line 489: How do the thrust and torque amplitudes for the fixed case are extracted from the load spectra evaluated at a reduced frequency of 0.6?
A: We thank the reviewer for the question. The amplitudes for both the fixed and moving cases are obtained from the magnitude of the complex FFT of the thrust and torque signals. In all configurations, we compute the FFT of the time series, identify the spectral peak at the reduced frequency of 0.6, and extract its magnitude. Prompted by the Reviewer's comment, we have clarified this in the text.

R1: Line 507: Usually, meandering is observed in the far wake. However, the downstream turbine is kept in the near wake. How the effect of meandering in the near wake is justified?
A: The Reviewer's comment is pertinent. We agree that natural wake meandering of a bottom-fixed turbine typically becomes dominant in the far wake. However, in the present work the meandering under discussion is of a different nature: it is the forced and coherent lateral motion of the wake induced by prescribed platform sway or yaw motions. Several experimental studies have shown that such motion-induced meandering can develop much closer to the rotor (e.g. at 3–5D), as also demonstrated in the works of Messmer et al. (2024a), Fontanella et al. (2025b) and Messmer et al. (2025). Therefore, although WT2 is located in what would normally be considered the near-wake region, the imposed crosswind or yaw motion produces a coherent lateral displacement of the wake that can affect the downstream turbine. We have clarified this relevant point in the revised manuscript.

Others

R1: Line 758: Is the blade-element-model of the rotor is experimentally validated?
A: We thank the Reviewer for this question. Yes, the blade-element-momentum (BEM) model used in Appendix B has been experimentally validated. The BEM implementation used to predict the thrust and power of this rotor, were extensively validated against wind-tunnel measurements in Bergua et al. (2023), where multiple numerical tools were compared with the experimental data of the same model turbine. We now clarify this in the revised Appendix B and explicitly cite the validation study.