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Wind Energy Science

Date: September 04, 2025
Subject: WES-2025-106 Response to Reviewers

Dear Referees,

We would like to express our sincere gratitude for the time and effort you devoted to reviewing our manuscript and for providing such constructive feedback. Your comments have highlighted important aspects that were not fully addressed in the original submission, and by incorporating your suggestions we believe the manuscript has been significantly strengthened in both clarity and impact.

We have carefully considered each of your comments and revised the manuscript accordingly. In the following, we provide a point-by-point response detailing how each remark has been addressed.

On behalf of all Authors,
sincerely,

Alessandro Fontanella

Response to Referees comments

We have organized the comments by section and, where possible, grouped similar comments together, providing a single response to avoid redundancy. In this document, **R1** refers to Referee 1, **R2** to Referee 2, and **A** to our response.

Line numbers in the referee comments refer to the first version of the manuscript, while line numbers in the authors' responses refer to the updated clean version.

General comments

R1: Justification of the observed results on power and loadings are mostly provided in reference to the previous experimental results on the wake. However, some discussions on the effect of the induction of the downstream turbine in the wake and its interactions with the waked inflow under different motion conditions of the upstream turbine would make the study more robust.
A: We agree with the reviewer. In the revised manuscript, we expanded the discussion to explicitly include the role of the downstream turbine induction and its interaction with the wake inflow. To this end, we added an additional panel in Fig. 6 and structured the interpretation using a causal chain: WT1 loads → WT1 wake → WT2 loads. This highlights how the induction of WT2 interacts with the unsteady wake generated by WT1 and clarifies the physical link between motion of the upstream turbine, wake dynamics, and loading of the downstream turbine. The new discussion is provided in Section 3.2 (pp. 17–18, Fig. 6) of the revised manuscript.

R2: The paper reads well, though is not the most concise. If made more to the point and concise it could help future readers.
A: We appreciate the suggestion. In revising the manuscript, we prioritized clarity and reproducibility while addressing the technical points raised by the reviewers. We streamlined wording where possible and moved detailed material to the appendices to keep the main narrative focused. Because several clarifications were requested (measurement definitions, normalization, TSR variation, etc.), the manuscript is slightly longer than before, but we believe it is now clearer and easier to follow without sacrificing transparency.

R2: The paper presents results for many different conditions, and hypotheses are proposed for the observed behavior, but are mostly not proven or verified by a deeper analyses of the underlying physics.
A: We thank the reviewer for this observation. In response, we have revised the manuscript by expanding the discussion of results in Section 3 to include interpretations regarding wake pulsing versus wake meandering, and the influence of reduced frequency, turbulence, and spacing. These additions offer a more comprehensive physical explanation for the observed trends. At the same time, we note that the scope of the present paper is to report controlled wind-tunnel experiments and to identify robust trends across configurations. A full validation of the proposed mechanisms through high-fidelity numerical simulations or additional flow measurements would go beyond the present dataset.

R2: Most importantly, the paper should describe more clearly which wind measurement data is used, how it is measured, or where the rotor effective wind speed is reconstructed through thrust measurements.

More details about how this is calculated is also needed in the paper: formula and used coefficients. I want to put attention to the fact that the rotor reconstructed velocity has its limitations, and if the entire analysis is based on the rotor reconstructed velocity, a second review should be performed to make sure the analysis is correct.

A: We thank the reviewer for this important observation. In the revised manuscript, we have clarified the use of wind measurement data throughout the paper. Specifically:

- We added Appendix B, which describes in detail how the rotor effective wind speed is reconstructed from thrust measurements, including the estimator formulation.
- We also revised the Methodology section (Section 2.1.2, p. 7) and other relevant parts of the text to explicitly state that results concerning wake velocities are obtained from hot-wire measurements, not from reconstructed rotor-effective velocities.
- The reconstructed velocity is only used to define the operating conditions of the downstream turbine, while all wake-related analyses are based on direct hot-wire data.

Abstract

R2: L12 'a key driver' - sentence is missing for what it is a key driver? For wake recovery? unsteady loading?

A: We thank the reviewer for pointing out this ambiguity. The sentence has been revised to specify that platform-induced wake dynamics are a key driver of wake recovery, downstream turbine performance, and dynamic loading.

Introduction

R2: L21 'substantial power losses, increased fatigue loads' --> The platform motions could increase or decrease wake losses and fatigue loads, as also shown in this paper. It is more unbiased and correct to word it as 'change', 'affect; or 'impact' those performance characteristics, without suggesting it will increase or decrease.

A: We thank the reviewer for this suggestion. The sentence has been revised to adopt neutral wording, now stating that platform motions alter power generation, fatigue loading, and control requirements. The updated text appears in the Introduction (p. 1, line 22) of the revised manuscript.

R2: L59: What is not discussed in the introduction is the different time scales at which platform and wave motions can occur (Strouhal number of motions). A difference should be made between very slow motions with larger amplitude and very fast motions usually with a much smaller amplitude. The impact on the wake can be very different, and could impact the summarized conclusions in this section.

A: We appreciate this valuable suggestion. In the revised manuscript, we expanded the Introduction to include a dedicated discussion of the frequency ranges at which floating wind turbine motions occur. A new paragraph explains how platform motions are characterized by two distinct frequency bands (wave-frequency range and lower-frequency range associated with platform eigenmodes). In the sentence at lines 48-49 we explained that most wind tunnel experiments investigated motions with relatively large amplitudes and low frequencies reaching reduced frequencies up to 1.5.

In addition, we added a paragraph clarifying the frequency and amplitude ranges investigated in our wind tunnel experiments, and their correspondence to full-scale reduced-frequencies numbers (pp. 10–11, Section 2.3.1).

R2: L75: 'more realistic turbulence levels.' --> It is relevant to add for which conditions/ Strouhal numbers and amplitudes these findings were made.

A: We thank the reviewer for this remark. The revised manuscript now specifies the conditions under which the findings of Li et al. (2025) were obtained, including the tested motion frequency, motion amplitude, and turbulence intensity. These details have been added to the Introduction (p. 3, lines 88–92).

R2: L87: The two key research questions are a bit misleading. We know the answer is yes to both questions. The real question is: how much and for which conditions. I think the real answers from this paper are about the quantification and characterization for specific test conditions.

A: We agree that the way the research questions were originally phrased made them sound like simple yes/no questions with an obvious "yes." Our real contribution lies in the quantification and characterization of these effects under controlled experimental conditions. We have therefore rephrased the research questions to better reflect this focus.

2.1 Experimental setup

R2: L131: It would be good to add to this sentence how the authors expect that the results in this paper should be interpreted to make conclusions for wind turbines with larger spacings. Likely the motion signatures in the unsteady loading will reduce for a larger spacing? Can the authors make predictions based on the tests at different locations?

A: We thank the reviewer for this helpful comment, which indeed contributes to framing the conclusions of our study. In the revised manuscript, we added a discussion in Section 2.1 (pp. 5–6, lines 159–164) clarifying how our results should be interpreted for larger turbine spacings. Specifically, we note that at distances beyond 5D, the coherent velocity structures generated by the upstream turbine are expected to progressively dissipate due to turbulence and wake mixing with the free stream, leading to a reduction of motion-induced signatures in the unsteady loading of downstream turbines.

R2: Is the design of the rotor blades changed for a lower Reynolds number? What is the Reynolds number during the tests?

A: In the revised manuscript, we clarified that the rotor blades were specifically designed for low-Reynolds-number operation to reproduce the thrust distribution of the DTU 10 MW reference turbine under wind tunnel conditions. We also added the operating Reynolds number range of the experiments. These details are now provided in the Methodology, Section 2.1 (p. 5, lines 136-146).

R2: Does the thrust and power coefficient match the full scale value?

A: In the revised manuscript, we added Appendix A, which presents the thrust and power coefficients of the model rotor and compares them with those of the full-scale DTU 10 MW reference turbine. This appendix demonstrates that the model reproduces the thrust characteristics of the reference turbine with high accuracy and provides a reasonable match in power performance.

2.2 Wind turbine operating settings

R2: L154 'noticeable influence' Can the authors describe what the influence is?

A: We thank the reviewer for this observation. The text has been revised both in the Introduction (p. 3, lines 71–74) and at the beginning of Section 2.2 (p. 8, lines 187–190) to clarify what the “influence” refers to. Specifically, we now state that platform motions induce coherent periodic structures in the wake, which in turn affect wake recovery and influence the loading and performance of downstream turbines.

R2: L163: How much was the thrust (coefficient)? Good to document this for future reference or giving the chance to reproduce or model the results.
A: In the revised manuscript, we reported the value of the thrust coefficient (Ct=0.9) in Section 2.2 (p. 9, line 199). We also clarified that this corresponds to the peak thrust obtained under the standard pitch-to-feather control strategy, not to the absolute maximum thrust achievable by the rotor.

R2: L162: A fixed rpm leads to varying tip speed ratio during pitch or surge motions. Can the authors estimate how much the tips speed ratio varied for the first wind turbine? This can possibly help understand the impacts on unsteady loading.
A: We thank the reviewer for this remark. In the revised manuscript, we added a clarification in Section 2.2 (p. 9, lines 200–204) stating that the tip speed ratio of WT1 varied by approximately 5% relative to its value in the fixed-turbine scenario due to the apparent wind induced by platform motion. We also noted that this small modulation does not substantially alter the aerodynamic regime of the rotor, consistent with the analysis of Fontanella et al. (2021).

R1: Page 7, Line 171: The operating conditions of WT2 were defined in terms of rotor-effective wind speed (URE), at each condition with fixed WT1. Optimal tip-speed ratio was calculated based on URE, which changes depending on the dynamic motion of WT1. How is the optimal-tip-speed ratio maintained for the cases with dynamic motion of WT1 and how does it affect the comparison of the power capture between different cases?
R2: L174: Can the authors describe more precisely how this was calculated? The wind turbine thrust coefficient is calibrated with tip speed, or expected to be constant in the operating range?
R2: L176: How is the optimal tip speed ratio determined for WT2? Tested by varying or theoretical? In general it is not clear in the paper where real wind speed measurements are used and where the reconstructed rotor-effective wind speed is used.
A: We thank the reviewers for these complementary comments, which all concern the definition and use of rotor-effective wind speed, the determination of the optimal tip-speed ratio, and their application under dynamic upstream motion. In the revised manuscript, we clarified this methodology in Section 2.2 (pp. 8–9, lines 214–227) as follows:

1. The rotor-effective wind speed at each WT2 location (with fixed WT1) was obtained by inverting the calibrated relation between thrust, wind speed, and rotor speed measured in free-stream conditions (Appendix B).
2. For the cases with dynamic WT1 motion, the rotor speed of WT2 was not updated instantaneously with the fluctuating inflow but kept constant at the value corresponding to the mean rotor-effective wind speed.
3. The optimal tip-speed ratio of the rotor ($\lambda=7.5$) was derived from the aerodynamic design and used as the target operating condition.

We also clarified throughout the text where wake velocities are obtained from direct hot-wire measurements, and the reconstructed rotor-effective wind speed is used solely to define the operating conditions of WT2.

R1: For placement of downstream turbine at 3D1D and 5D1D, in which the rotor speed was set to maintain the same thrust force as in the free-stream condition, how is the optimal tip-speed ratio maintained in the presence of speed-up? This should be clarified in the manuscript.
A: In the revised manuscript, we explicitly clarified that in the 3D1D and 5D1D configurations the tip-speed ratio of WT2 was no longer optimal. In these cases, rotor speed was chosen to maintain the same thrust force as in the free-stream condition, and the resulting TSR was lower than the design value of 7.5 due to the local speed-up. This clarification is included in Section 2.2 (p. 9, lines 221–226).

R1: Is the blockage correction performed? If so, how is it performed when two turbines are present? If the speed ups at 5D1D are attributed to blockage, how does the results obtained at that location can be related to the results in the atmospheric flows where the incoming flow essentially remains unbounded and the blockage is low? It would be nice, if the authors provide clarification.
A: We thank the reviewer for raising this important point. In the revised manuscript, we clarified in the Results section (lines 324–328) that no blockage correction was applied to the results. Classical correction methods are based on one-dimensional flow assumptions and are only valid for isolated rotors in uniform inflow. While they could be applied to WT1 in free stream, they cannot be consistently extended to WT2 when it operates partially or fully in the wake of WT1. To ensure consistency across turbines and configurations, we therefore report the results without correction.
We also added a reminder in the Discussion (lines 613–618), emphasizing that relative differences in WT2 power and loading between fixed and moving WT1 remain directly attributable to motion-induced wake dynamics, since all configurations were tested under the same wind tunnel conditions. However, the potential influence of wind tunnel blockage should be considered when extrapolating absolute performance levels to full-scale conditions in unbounded flows.

2.3 Platform motion scenarios

R1: Page 9, Line 210: The relation expressing the motion of hub in pitch motion appears to be dimensionally inconsistent and needs revision. The parameters should be defined separately for pitch motion. Moreover, r has been defined as a distance between the rotor apex and platform rotation point. I think, this should be the distance between the rotor hub and platform rotation point. This should be corrected.
A: The text in Section 2.3.1 has been rewritten to remove the ambiguity, with separate definitions of the translation and rotation motions parameters. We also corrected the definition of r_{hub}, which now clearly refers to the distance between the rotor hub and the platform rotation point.

R1: Page 10, Line 243: It would be nice if the range of large amplitudes are presented.
A: The original sentence was misleading. We revised the text in Section 2.3.2 (p. 12, lines 300–305) to clarify that at these frequencies there is significant amplification of the excitation from wind and wave forcing. The resulting motion amplitudes are then presented and discussed in the subsequent part of the section (see Fig. 2).

R2: Table 3: can the authors add the reduced frequency to the table?

A: We thank the reviewer for this helpful suggestion. The reduced frequency values have been added to Table 3 (p. 11) in the revised manuscript, which facilitates the interpretation of the subsequent figures and results.

R1: Page 10, Line 244: How are the OpenFAST and SOFTWind System simulations related? Are they performed for the same wind/wave conditions? Some comments for clarifications would be helpful
A: In the revised manuscript, we specified that OpenFAST was used to simulate the SOFTWIND system to generate the platform motion time series employed in the wind tunnel experiments. We also clarified that the simulations were performed under the same wind and wave conditions used in the experiments. This explanation is now included in Section 2.3.2 (p. 11, lines 293–299).

3 Results

R2: L265: How are the velocities for this formula defined or measured? The paper is missing a description of wind speed measurements. Maybe a more clear reference to previous work would help.
A: We clarified that the velocity in these formulas refers to the free-stream wind speed measured by the Pitot tube at hub height. This is now specified in Section 3 (p. 14, lines 320–321).

3.1.2 Impact of platform motion on energy recovery

R2: Figure 5: the wake losses seem very high! Usually, we expect the power of a downstream aligned turbine to be closer to 40%-50% or even a little higher, while the data shows 20% at a spacing of 5D. Is the thrust coefficient of the turbines very high? Is the incoming turbulence very low? It is important to discuss this in the paper, because it can affect the unsteady loading and its sensitivity to platform motion.
A: We thank the reviewer for this important observation. In the revised manuscript, we explicitly acknowledged that the relatively high wake losses observed at 5D are consistent with the combination of a high thrust coefficient of the model rotor and the very low incoming turbulence intensity of the wind tunnel flow. This context has been added to the discussion in Section 3.1.1 (p. 14, lines 367–377), as it is relevant for interpreting the sensitivity of downstream turbine performance to the wake of the upstream turbine.

R2: L304: how is this average wake velocity determined? Is this calculated from the measured thrust force?
A: In the revised manuscript, we simplified the text by removing the calculation of power variations based on velocity measurements. We also clarified that the average wake velocity was obtained from hot-wire anemometry at the WT2 positions, as reported in Fontanella et al. (2025b). These measurements showed only minor increases in mean wind speed relative to the fixed-turbine case.

R2: L306: I cannot find the 16% on figure 5.
A: We thank the reviewer for pointing out this inconsistency. The 16% value was a derived estimate of WT2 power gain obtained under the simplifying assumption of constant power coefficient, based on wake velocity measurements of Fontanella et al. (2025b). As this estimate mixed measured results with hypothetical assumptions, it created a mismatch with

Figure 5 and risked confusing the interpretation. To avoid ambiguity, we have removed this paragraph from the revised manuscript.

R2: L315: These observations indicate a high thrust coefficient of WT1. Is the magnitude expected to be so high? any references in the literature?
A: We thank the reviewer for this observation. In the revised manuscript, we clarified that the relatively high wake losses are due to the persistence of the wake and its limited recovery, consistent with the combination of a high thrust coefficient and low inflow turbulence. We also explained that the high value of C_t is consistent with the operating conditions of previous wind tunnel experiments on floating wind turbine aerodynamics, where model rotors were typically operated near rated conditions and produced similarly high thrust coefficients. This clarification, together with literature references, has been added to the Results (Section 3.1.2, p. 16, lines 370–376).

3.2 Dynamic loads from upstream turbine movement

R1: Page 17, Line 384: The amplitude of pitch motion appears to be too small (and like that of surge motion) to bring noticeable vertical wake movement. It would be nice to corroborate the presence of second harmonic in WT2 in lieu of the wake measurements performed in the present (or past) campaigns.
A: In the revised manuscript, we clarified that this is not the case for pitch motion at reduced frequency 0.3, which corresponds to an amplitude of 2.5°. Indeed, this amplitude is sufficient to induce a vertical displacement of the wake and, consequently, to generate the second harmonic observed in WT2. To support this point, we added a new figure showing the wake displacement under these conditions (Fig. 8).

R2: L345: 'delay of ' Assuming the disturbance travels with an average convective velocity in the wake: can the authors use this time difference to estimate the convective velocity and see how it compares to the free stream velocity?
A: We thank the reviewer for this interesting suggestion. Estimating a representative convective velocity from the load response of WT2 is not straightforward, because wake structures are convected at different velocities across different portions of the wake (slower close to the rotor and accelerating further downstream). A similar estimate was performed in the near wake using closely spaced hot-wire probes and PIV, as reported in Fontanella et al. (2022), where the delay was referred to the most upstream wake measurement. However, applying the same approach to downstream turbine load measurements would be highly uncertain, and we therefore did not attempt it in this study.

R2: Figure 6 : How is the thrust and power coefficient determined / how is the velocity measured to determine these coefficients? This is not clear in the paper. Velocity measurements are not really discussed as part of this paper. It looks like CT for WT1 and WT2 are normalized by the same velocity? which is not conventional: usually it is defined wrt the incoming velocity of each turbine, which is lower in the case of WT2.
A: In the revised manuscript, we clarified that the same free-stream velocity U is used for the normalization of both WT1 and WT2 loads. This convention is often adopted in wake and wind farm studies (e.g., Van der Hoek et al., 2024), as it facilitates a direct comparison of upstream and downstream turbine performance under identical reference conditions.
We also specified in the discussion of Figure 6 (p. 17, lines 409–414) that C_t and C_q are calculated using Eqs. (6) and (7), with the free-stream velocity as the reference. The

methodology section was updated to make this explicit and to avoid ambiguity about whether reconstructed or local inflow velocities were used.

R2: L374: The higher the reduced frequency, the shorter the streamwise wavelength of the velocity perturbations caused by the motion of the turbine.
Based on the incoming wind velocity and the frequency the authors could estimate the wavelength of the velocity fluctuations created by the turbine motion.
If this wavelength is of the same size as the integral length scale of the incoming turbulence, or perhaps the rotor diameter, it would make sense that it dissipates or decorrelates much faster. Perhaps the highest motion frequency passes a specific threshold explaining why the turbulent structures dissipate quickly? When the authors say weaker: do they mean smaller in amplitude, shorter in wavelength, or both?
A: We thank the reviewer for this insightful comment. In the revised manuscript, we rephrased the text to clarify that by "weaker" we refer to perturbations of lower amplitude. We explained that at higher frequencies the velocity oscillations have reduced amplitude and rapidly become comparable to the background turbulence in the wake of a fixed turbine. At the same time, we preferred not to introduce further interpretation of the coherence loss, since this would require supporting data that is not available in the present study. In particular, velocity oscillations grow and dissipate differently in different regions of the wake (e.g., shear layers vs. wake center), making it difficult to provide a useful or realistic simplified description without dedicated measurements. This clarification is now included in Section 3.2 (p. 19, lines 456–464).

3.2.1 Effect of turbine relative positioning

R2: L390: Again not clear how the velocity is measured.
A: In the revised manuscript, we clarified that the wake velocity measurements shown in Figure 9 (and in other figures presenting wake velocity data) are obtained from hot-wire anemometry carried out in the experiments reported in Fontanella et al. (2025b). This clarification has been added throughout the manuscript wherever wake velocity results are presented.

R2: L409: It would be good to state that this conclusion is for the specific streamwise spacing. For a larger streamwise spacing the wake spreading will be larger, possibly changing the conclusion.
A: We agree with the reviewer. In the revised manuscript, we added a clarification that this conclusion specifically applies to the tested streamwise spacing of 3–5D. For larger spacings, greater wake spreading could alter the interaction mechanisms and potentially change the outcome. This note is now included in Section 3.2.1 (p. 21, lines 503–505).

3.2.3 Yaw motion

R1: Page 19, Line 430: The reduction in net load oscillations appears to be strange. Should the higher oscillations in velocity not increase the fluctuations in load? Or does it increase the fluctuations in side-to-side loads not the thrust load of the turbines? Further, clarifications on the phenomenon to justify the results would improve the quality of the manuscript.
A: We thank the reviewer for this important remark. In the revised manuscript, we clarified that when WT2 is perfectly aligned with WT1, different parts of its rotor disk experience velocity fluctuations of opposite sign (one side slightly higher, the other slightly lower). These effects cancel in the rotor-averaged thrust, leading to small net axial load oscillations. What remains are asymmetric fluctuations, which manifest as yaw or tilt moments rather than in the thrust

force. When WT2 is laterally offset, this cancellation no longer occurs: one side of the rotor is exposed to a periodically varying flow, and thrust/torque oscillations increase. This explanation has been added to Section 3.2.3 (p. 23, lines 525–533).

Discussion

R2: L511: In this study only the first turbine is moved. By moving it the unsteady loading also increases for WT1. How would the findings in this paper change if WT2 also moves with waves? And what if the motion of WT2 is spatially and temporally correlated to the motion of WT1 such that movements of WT2 could be in sync or anti-sync with changes in windspeed from WT1? It would be helpful for the paper if the authors could make a comment about this for context.

R2: L530: See previous comment. For a bit more completeness, it would be good to add to this summation as well ' spatio-temporal correlation of waves through the wind farm'.

A: We agree that including WT2 motion would introduce additional physics, particularly through potential phase correlation between wake fluctuations and downstream rotor motion. To address this, we added a paragraph in the Discussion (Section 4, p. 28, lines 640–648) noting that spatio-temporal correlations of turbine motions and waves across the wind farm could significantly affect wake–turbine interactions and downstream loading. We also highlighted this as a promising direction for future experimental campaigns.

R2: The discussion section is a little awkward. It doesn't seem to present a new discussion but rather an overview of previous conclusions.

A: We respectfully disagree with the reviewer's assessment. The purpose of the Discussion is not to restate the results, but to:

1. Synthesize findings across the different tests by comparing sinusoidal vs. irregular motions, aligned vs. offset configurations, and motion types.
2. Interpret the physical mechanisms, distinguishing wake pulsing from wake meandering as the main drivers of cyclic loading, and identifying the conditions under which each mechanism dominates.
3. Highlight trade-offs such as modest downstream power gains versus increased cyclic loading, which is an aspect particularly relevant for floating wind farm design. This perspective was also positively noted by Referee #1, who wrote that *"discussion of the results is performed identifying the need of tradeoffs between the increased power and dynamic loading while designing the floating offshore wind farms."*
4. Place the findings in a broader context by linking them to realistic atmospheric conditions, turbine spacing, and platform motions that were beyond the scope of the present experiments.

We believe these elements go beyond a summary and instead provide a framework for interpreting the experimental findings in the context of floating wind farm operation and design.

Additional changes

- We have thoroughly revised the manuscript to improve readability.
- The measured free-stream wind speed is 3.95 m/s. In the original manuscript, this value was rounded to 3.9 m/s; in the revised version, we report it as 4.0 m/s for consistency with previous work.
- Due to an error in the post-processing code, the phase-averaged velocities shown in Figures 10 and 11 (numbers refer to the revised manuscript) were originally reported with an offset of T/4 applied to the fraction of the motion period. This error has now been corrected. Apart from this adjustment, the content of the figures remains unchanged: the curves are only shifted.