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Date: October 15, 2026

Subject: WES-2026-151 Response to Reviewers

Dear Referees,

We would like to express our sincere gratitude for the time and effort you devoted to reviewing our manuscript, “Experimental evidence of wake–rotor synchronization and its effects on the aerodynamic response of tandem floating wind turbines”, and for your constructive feedback. Your comments helped us clarify the experimental methodology, strengthen the physical interpretation of the results, and better define the contribution and limitations of the study.

We have carefully considered each comment and revised the manuscript. Below, we provide a point-by-point response explaining the changes and, where additional measurements were not available, the limitations of the evidence. The accompanying marked-up manuscript highlights the changes relative to the original submission.

Sincerely, on behalf of all authors,

Alessandro Fontanella

# Response to Referees' comments

We retain the original order and numbering of the comments. **R1** refers to Referee #1, **R2** to Referee #2, and **A** to the Authors' response.

Line numbers quoted in the referee comments refer to the first version of the manuscript. Section, figure, table and equation references in our responses refer to the revised manuscript (wes-2026-151_v2), unless explicitly stated otherwise.

## Referee #1

**R1:** The manuscript presents a valuable and timely experimental dataset regarding tandem floating offshore wind turbine aerodynamics, but it requires a comprehensive and rigorous revision before it can be considered for publication.

General comment: In particular, I would suggest the authors include more relevant literature to clearly identify the existing body of knowledge, gaps, and their own novelty (especially since many results were reported by previous studies).

**A:** We thank the referee for recognizing the value of the manuscript. We have revised the Introduction, in particular the text from lines 139 to 149, to clarify the specific contribution of this article compared with the existing literature. We reported several studies examining the wakes of floating wind turbines by means of wind tunnel experiments, full-scale measurements, and CFD. We also covered studies of wake interactions between different floating turbines (i.e., all turbines with platform motions), which have so far been investigated using CFD only. We have expanded the literature review to include additional, recent, experimental and numerical investigations of floating wind turbine wakes, with particular attention to studies addressing wake interactions between multiple floating turbines.

In light of the reviewed literature, the main novelty of this work lies in the experimental identification and quantification of aerodynamic coupling between the periodically modulated wake of an upstream turbine and a moving downstream rotor. Beyond characterizing the wake velocity oscillations generated by upstream turbine motion, the present work examines how downstream turbine motion reinforces or counteracts their effects on rotor loads. To isolate this interaction, the experiment combines test configurations in which either turbine moves independently with configurations in which both turbines move at prescribed relative phases. The resulting measurements – published as an open-access dataset – characterize this wake–rotor synchronization under controlled conditions, providing a basis for assessing its relevance to floating wind-farm dynamics and for validating numerical simulation tools in this regard.

**R1:** 1. It seems that the lowest tip-swept point/clearance of the turbine model is only 6.9 cm from the platform; any evaluation of this effect?

**A:** We acknowledge the limited blade-tip-to-platform clearance as a limitation of the present experimental setup (it is about 1/3 of the scaled distance from blade tip to sea level in a full-scale floating turbine). Although we do not expect it to significantly affect the main findings of the study, we cannot completely rule out some influence on the measured aerodynamic response.

Nevertheless, the upstream turbine load response and its wake behavior discussed in the manuscript are consistent with those reported in other recent studies – experimental and numerical – using different setups.

**R1:** 2. Lines 160-165: Maybe explain or cite a reference regarding the rotor design and where this analysis was made.

**A:** Section 2.1 now explicitly directs the reader to Fontanella, Da Pra and Belloli (2023), "Integrated Design and Experimental Validation of a Fixed-Pitch Rotor for Wind Tunnel Testing" (Energies, 16, 2205; https://doi.org/10.3390/en16052205), for the detailed rotor-design methodology.

**R1:** 3. Lines 168-169: Please specify the dynamic parameters for the motion platform/motor, or cite relevant references.

**A:** Section 2.1 describes the parallel-kinematic robotic platform and cites Fontanella et al. (2024), "Design of a robotic platform for hybrid wind tunnel experiments of floating wind farms" (Journal of Physics: Conference Series, 2767, 042008; https://doi.org/10.1088/1742-6596/2767/4/042008), for its detailed design.

**R1:** 4. Why was an inflow with no-shear and 2% turbulence intensity used? For the reason of revealing pure motion effects, shouldn't it be more appropriate to use laminar flow?

**A:** An ideally uniform, turbulence-free inflow would provide a cleaner reference, but the large test section used here has residual background turbulence of approximately 2%. This facility accommodates relatively large model rotors, helping to limit the low-Reynolds-number, manufacturing and instrumentation difficulties associated with smaller models.

The fixed tower-base and prescribed-motion cases were tested under the same background inflow conditions, allowing the effects of platform motion to be assessed relative to a consistent baseline.

Section 2.2.1 now describes the inflow as approximately uniform and low-turbulence, explains why it was selected to isolate motion-induced effects, and refers to the measured wind profiles in Appendix A.

**R1:** 5. Line 176: Any evidence or reference that shows the profiles of velocity and turbulence intensity? Seems it is quite close to the ground that part of the model would be immersed in the wall boundary layer.

**A:** Thank you for raising this point. We have added Appendix A and Fig. A1, with vertical profiles of mean streamwise velocity and turbulence intensity at both turbine locations, measured before the experiments with both turbines removed. Measurements were taken at four lateral positions, $y_i = -0.25$, 0, 0.5 and 0.75 m.

The spatial average of the mean streamwise velocity shows deviations ranging from -3% to +1% relative to the reference wind speed $U_0$ between the upper and lower rotor edges. For a fixed z position, there are min/max variations of approximately ±5% compared with the mean value. The mean wind speed decreases towards the floor, but the variations within the rotor area are small and comparable to the min/max variations in the lateral direction.

The turbulence intensity in the upper half of the rotor is between 1% and 2%. In the lower half of the rotor, it rises to approximately 3.5% on average for a given height.

**R1:** 6. Lines 197-198: I would expect evidence or references regarding this. And how would fixed rotor speeds make the results differ from varying ones? Which scheme is more likely to be used in reality?

**A:** Section 2.2.2 now links the statement about limited changes in mean wake velocity to Section 4.2.2, where Figure 4 shows only small differences between the fixed and moving cases. WT2 was kept at 360 rpm, corresponding to a nominal TSR of 7.3 based on the thrust-equivalent wind speed of 3.1 m $s^{-1}$. This fixes the mean operating condition while allowing small deviations in TSR due to instantaneous wind speed variations.

Section 2.2.2 also distinguishes the choice of fixed rotor speed from full-scale wind turbine control strategies. Commercial turbines vary rotor speed to follow their operating set point, whereas our experiment fixes rotor speed and blade pitch primarily to isolate the effects of platform motion on wind farm aerodynamics. Section 2.2.2 cites Fontanella et al. (2018) – on model-scale wind turbine control – and Bergua et al. (2023) – on aerodynamic response with variable rotor speed – to explain that a variable-speed experiment would introduce additional changes in aerodynamic rotor loads and complexity in the experiment and its results.

**R1:** 7. Lines 232-234: "At full scale…" References should be provided to support the ranges of motion frequencies.

**A:** Section 2.2.3 gives the supporting references alongside the ranges of motion frequencies: Lemmer et al. (2020) for the SWE TripleSpar, Wise and Bachynski (2020) for the LIFES50+ Nautilus and NTNU spar, and Arnal (2020) for the Softwind spar.

**R1:** 8. Lines 247-248: "…, while aerodynamics… scale linearly…" Why does aerodynamic force scale linearly with frequency? Is it applicable to all ranges of frequencies (or St)?

**A:** Section 2.2.3 now qualifies the statement: at constant platform motion amplitude, aerodynamic load oscillations scale approximately linearly with motion frequency for reduced frequencies up to approximately 1.1. In this range, the rotor aerodynamic response is primarily governed by the apparent wind-speed amplitude.

**R1:** 9. Lines 250-251: "Previous…negligible." At such high St, it generates super-strong wake-rotor interactions and unsteady aerodynamics, with wake or flow responses quickly decay/dissipate in the very near wake or induction region. Maybe the authors referred to the far wake?

**A:** We have clarified this point in Section 2.2.3. The text now states that the wake response at reduced frequencies of approximately 1.44–2.4 is negligible "beyond the near-rotor region", citing Fontanella et al. (2025a) and Messmer et al. (2024a).

**R1:** 10. Line 264: The amplitudes should be normalized.

**A:** The motion amplitudes are now reported in Section 2.2.3 as 0.08 m (0.067D) and 0.02 m (0.017D), for reduced frequencies of 0.12 and 0.48, respectively. The caption of Table 2 also gives these normalized values.

**R1:** 11. Line 332: The thrust variation could be harmonic but might not be sinusoidal; maybe justify the expression of the sine function representation.

**A:** Section 3.1.2 now states explicitly that the sinusoidal expression represents the fundamental harmonic component of thrust at the imposed motion frequency. It is not a representation of the complete thrust signal. The amplitude and phase are extracted at that frequency by Fourier Transform analysis.

**R1:** 12. Line 350: The sampling frequency is 10 kHz, but the signal was filtered; then why not use 100 Hz for sampling?

**A:** Section 3.2 now distinguishes acquisition at 10 kHz from subsequent low-pass filtering at 100 Hz and explains the reasons for these choices.

The 10 kHz sampling frequency was selected to ensure adequate temporal resolution of the original hot-wire signal and to avoid aliasing of higher-frequency fluctuations (mainly from electrical noise) that could affect the low-frequency content of the signal. The 100 Hz low-pass filter was subsequently applied during post-processing to remove electrical noise above it while retaining the turbulence scales relevant to the present analysis. Sampling directly at 100 Hz would not provide the same anti-aliasing protection and flexibility in post-processing. This distinction has been clarified in Section 3.2.

**R1:** 13. Can any evidence of statistical convergence be provided?

**A:** We have added an acquisition-duration sensitivity study in Appendix B (Fig. B2) and corrected the measurement duration in Section 3.2 from 24 s to 20 s. This corresponds to 10 motion cycles at 0.5 Hz and 40 cycles at 2 Hz.

For WT1 undergoing surge motion at 2 Hz, we evaluated the sensitivity of the wake metrics at 3.5D downstream of WT1 to the number of motion cycles included in the analysis (10–40 cycles). The quantities considered were the mean streamwise velocity, turbulence intensity, and phase-averaged rotor-area-averaged streamwise velocity.

As can be seen in Fig. B2, the mean streamwise velocity and the turbulence intensity do not vary significantly with the number of motion cycles included in the analysis. Slight variations are observed for the instantaneous spatially averaged streamwise velocity over the rotor region. The response remains periodic and nearly sinusoidal over one motion period. The maximum decreases from $0.641U_0$ in the 10-cycle case to $0.628U_0$ in the 40-cycle case, while the minimum decreases from $0.529U_0$ in the 10-cycle case to $0.517U_0$ in the 40-cycle case.

**R1:** 14. Section 4.2. For inflow turbulence this low, the near-wake region could be extended to as far as 7.5D. I would suggest the authors more carefully specify this and restrict the results and conclusions to avoid mixing with the far wake properties.

**A:** Thank you for this comment. We agree that with the low turbulence intensity considered in the experiment the wake deficit is quite persistent downstream of the turbine and in particular at the distance where the WT2 turbine is placed.

This aspect is now discussed in Section 4.2.1 describing the persistent double-Gaussian profile at 3.5D and 4.3D and explaining that the wake has not yet broken down, in Section 4.5 relating the low WT2 power at 5.75D to the persistent wake deficit, and Section 5.4 when discussing results of downstream turbine power.

**R1:** 15. Line 426-429: “Surge motion has a limited… In contrast,…” This was already reported by Duan et al. (https://doi.org/10.1016/j.enconman.2026.122028); it would be better to relate the findings. Also, the results in Lines 520-524.

**A:** We agree that the limited change in mean wake velocity alongside a “signature” in the wake velocity spectra is consistent with previous work. A citation to Duan et al. (2026), “Power and wake of a floating wind turbine in surge motion: Theoretical and experimental investigations” (https://doi.org/10.1016/j.enconman.2026.122028), has been added to the Introduction.

Section 4.2.2 compares the small changes in wake mean velocity that were found in this experiment with those reported by Messmer et al. (2024b) and Fontanella et al. (2025a) in previous experiments. Section 4.3 relates periodic load response of a fixed downstream turbine observed in this experiment to results of Fontanella et al. (2026) and Miroux et al. (2026).

We have clarified that the novel contribution of this article lies in quantifying how the downstream turbine load response depends on its motion phase relative to the upstream turbine, and in explaining how this interaction alters the equivalent aerodynamic damping of the downstream turbine.

**R1:** 16. Lines 463-464: Is there a more physics-based explanation of the “mask” effect by turbulence fluctuations?

**A:** Section 4.2.2 replaces the vague “masking” explanation with a distinction between motion-synchronized velocity fluctuations and residual turbulent fluctuations. The spectral peak at the imposed motion frequency exists in both reduced frequency cases, but at $f_r = 0.12$ the wake response coherent with platform motion is found only at some lateral positions within the wake. Spatial averaging therefore reduces the prominence of the motion-synchronized velocity fluctuations relative to residual turbulent fluctuations that are not completely removed by phase averaging.

The text also reports that the response at $f_r = 0.48$ remains smoother when averaged over 10 motion cycles as for $f_r = 0.12$. The response at $f_r = 0.48$, calculated using 10 motion cycles is shown in Appendix B (Fig. B2) as part of the acquisition-duration analysis. Thus, the difference in wake response at $f_r = 0.12$ and $f_r = 0.48$ is not due to the number of motion cycles considered in the analysis.

**R1:** 17. Lines 485 490: The Cp and Ct values for WT2 are much lower than WT1, even based on its own “inflow”; can the authors explain? Is it due to Reynolds number effect? How do these affect the generalizability of the results and conclusions?

**A:** Section 4.3 now discusses this difference explicitly. Based on the local thrust-equivalent velocity, WT2 has $C_p = 0.11$ and $C_t = 0.70$, compared with 0.28 and 0.82 for WT1. Lower inflow speed and rotor speed reduce the blade Reynolds numbers of WT2 compared to WT1. At these low Reynolds numbers, changes in airfoil performance may affect torque more strongly than thrust. The spatially non-uniform inflow set by WT1 wake also changes local blade operating conditions, even when the nominal TSR is maintained.

We also clarify in Sect. 2.2.2 that the velocity of 3.1 m/s is a thrust-equivalent velocity obtained by inverting a calibrated uniform-inflow BEM relationship. It is not a directly measured rotor-area average or an energy-equivalent velocity. Normalizing power by this velocity therefore does not fully account for the non-uniform wake inflow.

Section 5.4 now explicitly discusses the limitations of quantitative extrapolation of the experiment results to full scale. The text explains that the scaled blades were designed primarily to reproduce thrust characteristics and their sensitivity to inflow, rather than to achieve similarity in power-coefficient. Comparisons of results obtained with platform motion with those of the fixed configuration reduce some effects of operating at a small scale that are in common to all configurations, but do not completely remove these scaling limitations.

The Discussion clarifies that the experiment demonstrates the influence of wake–rotor synchronization on loads and power of a downstream turbine, while the magnitudes of the observed power gains and aerodynamic damping should not be directly extrapolated to full-scale turbines.

**R1:** 18. Line 533. What do you mean by "constructive and destructive"?

**A:** Section 4.3 now defines the terms through the apparent wind speed at WT2. A constructive interaction occurs when an increase in incoming wake velocity coincides with upwind rotor motion, reinforcing the apparent wind-speed oscillation. A destructive interaction occurs when the same increase coincides with downwind motion, partially compensating the rotor-motion contribution.

**R1:** 19. Line 539-543: To say a wake is in or out of phase with motion, it would be more accurate to have correspondence PSD of the two signals. I think because wake convection is not the same as motion speed, it would be strange to compare them descriptively and qualitatively.

**A:** We agree that the phase relationship between the incoming wake fluctuations and downstream rotor motion should not be inferred directly from the phase difference between turbine motions. Section 4.3 explains that wake perturbations reach WT2 after a convection delay, are not uniform in space, and are affected by the induction of WT2 itself. Consequently, the relative motion phase alone does not determine their alignment with the downstream rotor velocity. The present hot-wire arrangement did not measure velocity at the operating WT2 rotor plane. Removing WT2 would change the induction field and would not reproduce the flow experienced by the operating rotor. We therefore base the quantitative result on the measured thrust amplitude and phase in Fig. 6.

Section 4.3 also identifies non-intrusive, phase-resolved velocity measurements at the WT2 rotor plane as the means to characterize the mechanism further.

**R1:** 20. Section 5.2. I think the title is a bit too general, considering only two different reduced frequencies were selected and tested.

**A:** The title of Section 5.2 has been changed to "Effect of relative motion phase on turbine–wake interaction and aerodynamic damping". The discussion specifies the tested wind turbine spacing of 5.75D, the use of a low-turbulence inflow, the turbine operating conditions and the two reduced frequencies of platform motion. It thus describes the phase dependence within the investigated parameter space without implying a comprehensive sweep of all relevant parameters.

**R1:** 21. Lines 694-696: How do these conclusions differ from existing literature? For example, Hu et al. (https://doi.org/10.1016/j.apenergy.2026.12799) and Li et al. (https://doi.org/10.1016/j.renene.2024.122062)?

**A:** We thank the referee for highlighting these relevant studies. The Introduction already discusses the tandem-turbine simulations of Li et al. (2024, 2025), and we have now added Hu et al. (2026), "Impact of surge motion on wake characteristics, power performance and fatigue loads of two tandem floating wind turbines" (Applied Energy, 416, 127999; https://doi.org/10.1016/j.apenergy.2026.127999). The added paragraph summarizes their LES study of synchronized surge motions, including enhanced wake recovery and power of the downstream turbine.

We have also clarified the scope of the comparison in Section 5.1: the passage corresponding to the original lines 694–696 now explicitly refers to "previous single-turbine experimental studies". It compares the present measurements with the experimental evidence cited there, rather than providing a comparison with the CFD results.

The specific contribution of the present work remains the experimental characterization of the interaction between a motion-modulated wake of an upstream turbine and a moving downstream rotor, including the dependence of loads and equivalent aerodynamic damping on relative motion phase.

## Referee #2

**R2:** The article presents an experimental study on the effects of synchronization of the wake of a downstream turbine with platform motions of an upstream one under surge motion. Aerodynamic response and wake measurements of two tandem turbines are characterized under combination of motion characteristics such as frequency, amplitude and phase of platform motions. The topic of the article is clearly of interest to the readership of Wind Energy Science, and the results are presented in a clear manner. I'd recommend the article for publication given the following concerns/comments are appropriately addressed.

**A:** We thank the referee for the positive assessment of the article and for the constructive comments. The responses below describe the additional details on the experimental methodology and interpretation of the results included in the revised manuscript.

**R2:** 1. The study indicates a uniform, low shear profile with TI of 2%. Is that the case across the streamwise length of the wind tunnel test section or only upstream of the first turbine? If the flow shear or TI changes along the length of the test section, how would that affect the results from the study?

**A:** Section 2.2.1 and the new Appendix A (Fig. A1) now report inflow profiles at both turbine locations. They were measured before the experiments with turbines removed. The profiles at the two locations are very similar within the rotor region, so there is no indication of a substantial difference in background inflow between the two turbine stations.

**R2:** 2. From Fig. 1 I can see that the platform has a significant height relative to the test section. I'd imagine that would change the flow shear/TI in the turbine wake compared to a case with no platform. Is there any quantification of this effect or is this effect assumed to have insignificant effect on the flow?

**A:** We do not have measurements of the wake of the same rotor with the same inflow and at the same downstream location as in this experiment, but without the robotic platform (i.e., with a simple tower), which is the information needed to address this point.

The only information we can show here is that we measured the wake of the upstream wind turbine, with a fixed tower base, along vertical lines at $x_i = 3.5D$ and $x_i = 4.3D$, with $y_i = 0.15D$. The offset in $y_i$ from the rotor centerline was introduced to avoid measurements directly behind the turbine tower which may not be representative of a full-scale turbine (the robotic platform has a width of 0.31 m and extends from $y_i = –0.13D$ to $y_i = 0.13D$).

The vertical profiles of streamwise velocity and turbulence intensity are reported in Fig. R1. At both $x_i = 3.5D$ and $x_i = 4.3D$, the mean velocity profiles are fairly symmetric about the rotor axis ("Hub" in the figure) and have a relative maximum at its location. The velocity in the lower half of the wake is 5–10% lower than in the upper half of the wake. The turbulence intensity profiles have a more marked asymmetry with respect to the rotor axis. In the upper portion of the wake, the turbulence intensity increases towards the rotor edge from about 7% near the rotor axis; near the rotor edge, there is a peak attributable to the shear layer. In the lower part of the wake, the turbulence intensity decreases to 4–6%, and there is no evident shear layer. This behavior, with slightly lower recovery in the lower part of the wake accompanied by lower turbulence intensity than in the upper portion of the wake, may be associated with reduced momentum exchange and turbulent mixing due to the proximity of the wind tunnel floor.

Although the available measurements do not allow us to isolate and quantify the influence of the robotic platform, the vertical wake profiles in Fig. R1 do not reveal obvious flow disturbances that can be directly attributed to its presence. Nevertheless, some influence of the platform on the wake development cannot be excluded.

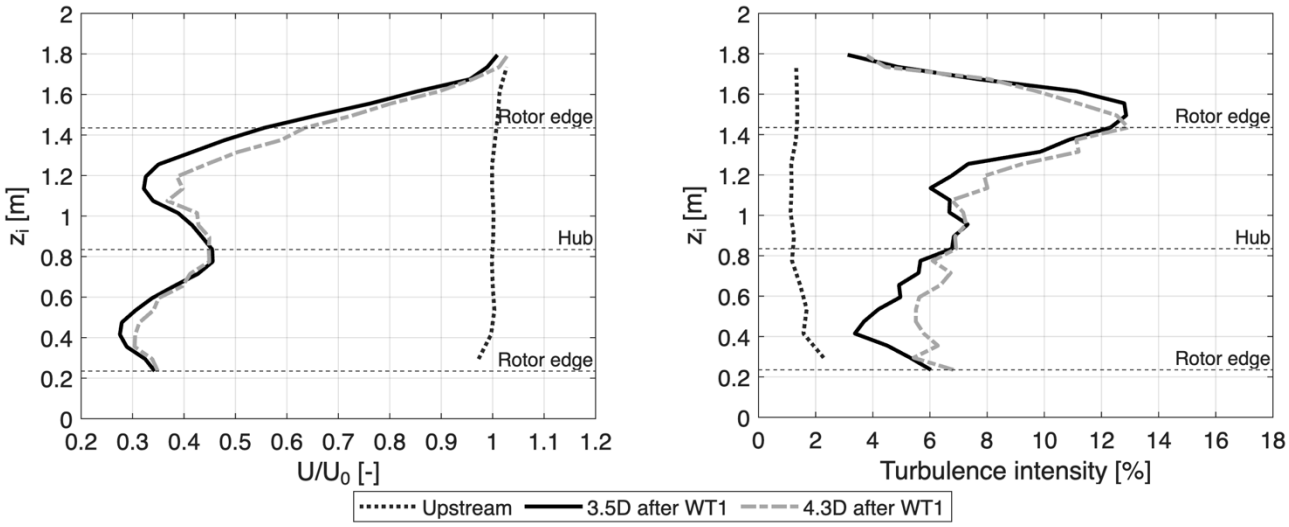



Figure R1. Vertical profiles of mean streamwise velocity and turbulence intensity at $y_i$ = 0.15D and different $x_i$ positions.

**R2:** 3. Section 2.3 mentions that hot wire probes were used for measurements and 29 measurement points were selected. Does this mean 29 probes were used simultaneously for measurements? Also, how were the probes calibrated? Please clarify this.

**A:** Section 2.3 now clarifies that two hot-wire probes were mounted on a traversing system and moved sequentially across 29 lateral stations at each streamwise location. The 29 measurements were not simultaneous. The motion-synchronized spatial distribution is reconstructed by aligning each measurement with the platform-motion phase, as explained in Section 3.2 and Appendix B. This procedure does not reconstruct simultaneous broadband turbulent fluctuations.

Section 2.3 explains that the probes were calibrated using a reference nozzle over the expected velocity range. The uncertainty assessment used Monte Carlo propagation of uncertainty contributions including the acquisition board, reference pressure transducer and thermocouple used for temperature correction. Section 2.3 reports average expanded uncertainties at 95% confidence of 0.17 m $s^{-1}$ for velocity and 0.4% for turbulence intensity.

**R2:** 4. The authors note that velocity measurements are carried out for 24 seconds. Is that enough for statistical convergence? This is important to establish that the differences in wake velocity between fixed and surge cases are statistically significant.

**A:** The stated duration of 24 s was an error and has been corrected to 20 s in Section 3.2, corresponding to 10 cycles at 0.5 Hz and 40 cycles at 2 Hz, or approximately 20 minutes at full scale. Appendix B, Fig. B2 now examines the sensitivity of wake metrics to acquisition windows spanning 10–40 cycles at 2 Hz, at 3.5D behind WT1.

Mean velocity and turbulence intensity change only slightly when increasing the number of cycles from 10 to 40. The phase-averaged spatially averaged velocity retains its periodic response, with peak-to-peak amplitude changing from $0.112U_0$ to $0.111U_0$ as the analysis

window increases from 10 to 40 cycles. This supports stability of the main velocity metrics with the acquisition window length but does not demonstrate statistical significance of the small mean velocity differences between cases with fixed tower base and platform motion. Section 4.2.2 explicitly states that repeated wake measurements were unavailable to quantify their uncertainty and that these differences should be interpreted as indicative trends.

**R2:** 5. In section 4.3, normalized coefficients of WT2 based on rotor effective wind speed show large differences from those for WT1. Is it because of reduced aerodynamic performance in the wake or some other reason? Please explain.

**A:** Section 4.3 now discusses this difference explicitly. Based on the local thrust-equivalent velocity, WT2 has $C_p$ = 0.11 and $C_t$ = 0.70, compared with 0.28 and 0.82 for WT1. Lower inflow speed and rotor speed reduce the blade Reynolds numbers of WT2 compared to WT1. At these low Reynolds numbers, changes in airfoil performance may affect torque more strongly than thrust. The spatially non-uniform inflow set by WT1 wake also changes local blade operating conditions, even when the nominal TSR is maintained.

We also clarify in Sect. 2.2.2 that the velocity of 3.1 m/s is a thrust-equivalent velocity obtained by inverting a calibrated uniform-inflow BEM relationship. It is not a directly measured rotor-area average or an energy-equivalent velocity. Normalizing power by this velocity therefore does not fully account for the non-uniform wake inflow.

Section 5.4 now explicitly discusses the limitations of quantitative extrapolation of the experiment results to full scale. The text explains that the scaled blades were designed primarily to reproduce thrust characteristics and their sensitivity to inflow, rather than to achieve similarity in power-coefficient. Comparisons of results obtained with platform motion with those of the fixed configuration reduce some effects of operating at a small scale that are in common to all configurations, but do not completely remove these scaling limitations.

The Discussion clarifies that the experiment demonstrates the influence of wake–rotor synchronization on loads and power of a downstream turbine, while the magnitudes of the observed power gains and aerodynamic damping should not be directly extrapolated to full-scale turbines.

**R2:** 6. Line 522, it is noted that for WT2 fixed, the thrust amplitude is smaller than WT1 fixed case. It would be beneficial to give some physical insight into why this is the case.

**A:** We have clarified the motion configurations in Section 2.2.3 and their distinct forcing mechanisms in Section 4.3. In the “WT1 fixed” configuration, WT2 moves within the wake of a stationary upstream turbine, and its surge velocity directly produces apparent wind-speed oscillations. In “WT2 fixed”, WT2 has no imposed motion, and its thrust oscillations are instead driven by the periodically varying incoming wake generated by WT1. The measured response to this wake forcing is smaller than the response to direct rotor motion under the tested conditions as quantified in Fig. 6.

To further clarify the different aerodynamic-load response of WT1 and WT2 to imposed motion, the new Appendix C compares the thrust oscillations of WT1 moving in the free stream with that of WT2 moving behind a fixed WT1. The quasi-steady aerodynamic thrust model introduced in Appendix C accounts for both the mean inflow velocity and the variation of $C_t$ with TSR. For the same apparent wind-speed amplitude of 0.25 m $s^{-1}$, Table C1 gives predicted thrust amplitudes of 0.67 N for WT1 and 0.46 N for WT2, compared with measured values of

0.63 N and 0.43–0.50 N. The linearized expression of the model in Eq. (C5) shows shows that the thrust oscillation amplitude is approximately proportional to the mean wind speed, which is lower in the wake compared to free stream.

**R2:** 7. Finally, I think it would be nice to put the study in context relative to existing literature on the topic with single or multiple turbines in the discussion section, especially since the study explored a limited set of frequency and amplitude. That would really enhance the scientific discussion on the topic, as it is a very rich topic with a lot of variables.

**A:** We agree that discussing the results in the context of existing literature is particularly important given the limited range of motion frequencies and amplitudes investigated.

In the revised manuscript, we have expanded and reorganized Section 5 (Discussion) to better contextualize our findings with respect to previous experimental and numerical studies. Section 5.1 discusses the influence of motion reduced frequency on wake dynamics and power production of the downstream turbine, comparing our observations with previous single-turbine studies (e.g., Messmer et al., 2024a; Fontanella et al., 2025). These comparisons highlight the consistency of our results with previous findings that intermediate reduced frequencies can promote wake recovery and enhance downstream power production.

Section 5.2 discusses the influence of the relative phase between turbine motions on the aerodynamic response of the downstream turbine. Section 5.3 further discusses the evolution of wake dynamics downstream of the second turbine.

Finally, we have expanded Section 5.4 (Limitations of this experiment and outlook) to acknowledge the limited range of motion frequencies and amplitudes investigated, alongside other experimental limitations related to inflow conditions, prescribed harmonic motions, and turbine operating conditions. We clarify that the experiment was designed to isolate the wake–rotor synchronization mechanism rather than characterize its dependence on all relevant parameters. We also identify the need for broader parametric investigations under more realistic operating conditions as a direction for future research.