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Wind Energy Science

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Subject: WES-2025-106 Response to Reviewers

Dear Editor,

We would like to express our sincere gratitude for the time and effort devoted by Reviewers to evaluating our manuscript and for the constructive comments provided. We greatly appreciate the insightful suggestions from the Reviewers, which have helped us improve the clarity and quality of the paper.

In response to the comments received, we have revised the manuscript and added clarifications and additional discussion where appropriate. We believe these revisions have strengthened the manuscript and addressed the concerns raised.

In the following, we provide a detailed point-by-point response to all Reviewers' comments. For clarity, these are reproduced in italics, followed by our responses and a description of the corresponding changes introduced in the revised manuscript.

Sincerely,

*Alessandro Fontanella*, on behalf of all the authors

## Response to Referee #1 comments

*I thank the authors for making a significant effort in addressing my comments.*

*However, I believe that in this work there are two main criticisms, one from the methodological standpoint and one about the experimental setup:*

*1) Estimating a reference rotor velocity by assuming that a rotor operating under waked conditions, namely in the presence of both vertical and lateral shear and turbulence different from that of the incoming wind, is conceptually incorrect. Therefore, settings of the rotor tip speed ratio and data analysis can be affected by an erroneous estimate of the rotor velocity.*

*2) I think the data show the occurrence of wind tunnel blockage due to the excessive confinement of the flow in the vertical direction, due to the presence of the two turbines and the wake of the upstream turbine.*

*Below, I add my replies in blue to the authors' rebuttal.*

### Thrust-equivalent velocity

**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. ... 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:** Our intention in introducing the rotor-equivalent wind speed ( $URE$ ) was not to recreate the real inflow conditions, nor to estimate a power/thrust curve in the IEC sense. Instead,  $URE$  is used to prescribe the operating point of the downstream rotor in a consistent manner across configurations. Specifically,  $URE$  represents the wind speed of a uniform inflow that would generate the same mean thrust as the waked inflow. We should infer this was not completely clear in the first draft and apologize for this if so: the paper has been further revised to make it clearer. 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. In other words, we do have an estimation of RPM for WT2 because we are simply setting them as a constant value. 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...

**RR1:** *First, rotor-equivalent wind speed is a well-defined physical parameter in wind energy*

- *Wagner, R., Antoniou, I., Pedersen, S.M., Courtney, M.S. and Jørgensen, H.E., 2009. The influence of the wind speed profile on wind turbine performance measurements. Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology, 12(4), pp.348-362. Wagner, R., Courtney, M., Gottschall, J. and Lindelöw-Marsden, P., 2011.*
- *Accounting for the speed shear in wind turbine power performance measurement. Wind Energy, 14(8), pp.993-1004. Wagner, R., Cañadillas, B., Clifton, A., Feeney, S., Nygaard, N., Poodt, M., St Martin, C., Tüxen, E. and Wagenaar, J.W., 2014, June.*

- Rotor equivalent wind speed for power curve measurement–comparative exercise for IEA Wind Annex 32. In *Journal of Physics: Conference Series* (Vol. 524, No. 1, p. 012108). IOP Publishing.).

To avoid confusion for the reader, I would consider assigning another name to this parameter. For instance, as reported in the rebuttals by the authors, thrust-equivalent velocity.

The equivalent velocity is estimated by assuming that the thrust curve under wake conditions is the same as that under a freestream flow. Of course, this is not the case. Therefore, the authors are estimating velocity from a thrust curve that is not representative of the case under testing. Therefore, I disagree with this statement: "The rotor-effective wind speed is not intended as a reconstruction of the waked heterogeneous inflow; instead,  $U_{re}$  denotes the speed of a uniform inflow that would generate the same mean thrust on the rotor ..." This statement is formally incorrect because you are assuming that a waked rotor and a rotor under freestream conditions have the same thrust curve. If you could estimate the real rotor-equivalent velocity (e.g., with PIV for wind tunnel experiments), you would obtain a different thrust curve. In my opinion, you should perform your analysis only on the basis of the rotor RPM set and the measured thrust force. This approach is not only misleading but also incorrect, for instance, when you set the rotor TRS based on  $U_{re}$  (see discussion in L 244 in the marked-up manuscript).

**AA:** We thank the Reviewer for the detailed clarification and for pointing out the potential conceptual ambiguity associated with the term "rotor-equivalent wind speed".

Following the Reviewer's suggestion, we have renamed this parameter throughout the manuscript as thrust-equivalent wind speed ( $U_{TE}$ ) to avoid any confusion with the rotor-equivalent wind speed defined in IEC power performance standards.

We agree that a rotor operating in a heterogeneous inflow does not follow the same thrust curve as in uniform flow conditions. For this reason,  $U_{TE}$  is not introduced as a physical reconstruction of the actual inflow, nor as an attempt to infer the "true" rotor-equivalent wind speed in the IEC sense. Rather,  $U_{TE}$  is used strictly as an operational parameter to define a consistent operating point of WT2 across different wake configurations.

In practice:

- WT2 operates in open loop with prescribed rotor speed.
- The aerodynamic quantities of interest in this study are the measured thrust and torque, not an inferred inflow velocity.
- $U_{TE}$  is obtained by inverting a calibrated free-stream thrust map solely to determine a rotor speed corresponding to the optimal TSR under an equivalent load condition.

Therefore, no part of the aerodynamic analysis (time-average loads, dynamic loads, power ratios, wake) relies on interpreting  $U_{TE}$  as the true inflow velocity. All reported thrust and torque coefficients are normalized using the measured free-stream velocity  $U$ , which remains constant throughout the tests (see Eq. 6–7).

We acknowledge that the thrust curve of a rotor operating in a heterogeneous waked inflow may differ from that obtained under uniform free-stream conditions. However, Figure 4 demonstrates that, when the measured loads of WT2 are plotted against the thrust-equivalent wind speed  $U_{TE}$ , the resulting operating points remain consistent with the reference characteristics of the rotor obtained in free-stream. Both thrust and torque values fall close to the corresponding curves obtained under uniform inflow conditions.

Moreover, the torque measurements confirm that, when the rotor speed of WT2 is prescribed based on  $U_{TE}$  to target the optimal TSR, the turbine operates along the reference torque curve derived in free stream. This indicates that the rotor speed setting based on  $U_{TE}$  leads to a consistent aerodynamic operating condition, without introducing systematic deviations in the measured performance quantities.

Therefore, while  $U_{TE}$  is not interpreted as the true physical inflow velocity, its use as an operational parameter to prescribe the rotor speed in open-loop control provides a sufficiently accurate and consistent definition of the turbine operating point for the purposes of the present experimental study.

To further clarify this point and avoid possible misunderstanding, we have:

1. Renamed the parameter to thrust-equivalent wind speed ( $U_{TE}$ ) throughout the manuscript.
2. Explicitly stated that  $U_{TE}$  is not a physical inflow reconstruction.
3. Added a sentence in Sect. 2.2 clarifying that all aerodynamic coefficients are normalized with the measured free-stream velocity and are independent of  $U_{TE}$ .

Finally, regarding the suggestion to perform the analysis solely in terms of rotor RPM and measured thrust, this is in fact already the case. The RPM is prescribed and reported (Table 2), and all aerodynamic results are based directly on measured loads.  $U_{TE}$  is used solely to determine the rotor speed setting prior to the tests and to provide a consistent descriptor of the operating condition across configurations.

We hope that this clarification resolves the conceptual concern.

### **Blockage**

**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 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 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.

**RR1:** *L 244 in the marked-up document: "...in the 3D1D and 5D1D configurations. In these two cases, the URE for WT2 is higher than both the free-stream wind speed and the wind speed measured by the lateral Pitot system, which accounts for the blockage effect of WT1. The additional increase in velocity observed at WT2 is attributed to the local blockage caused by the WT2 rotor itself."*

*A single rotor does not generate blockage. A single rotor has an induction, namely, a velocity reduction upstream of the rotor and a slight increase of the velocity in the near wake at lateral locations. A velocity increase at the rotor plane is a sign of wind tunnel blockage, thus due to the large blockage ratio for the experiment and the confinement of the ceiling of the test section.*

**AA:** We would like to clarify that, in the manuscript, the term blockage effect of WT1 refers specifically to wind-tunnel blockage induced by the presence of WT1 within the confined test section.

In unconfined flow, a rotor induces a velocity deficit upstream and in the wake, accompanied by lateral acceleration around the rotor due to continuity. However, in a confined wind-tunnel environment, the presence of a rotor occupying a finite fraction of the test-section area necessarily generates global blockage effects, as described by classical wind-tunnel interference theory (e.g. Glauert correction; see Inghels, 2013, also used in OC6 Phase III).

In a closed test section, two mechanisms contribute to velocity increase outside the wake:

1. Geometric blockage: the rotor reduces the effective flow area.
2. Wake blockage (momentum deficit effect): the wake carries lower kinetic energy, and because the overall mass flow rate in the test section is constrained, the flow outside the wake accelerates to conserve mass.

This effect is well documented in wind-tunnel testing of wind turbines and is the reason blockage corrections are routinely applied for isolated rotors (see Appendix B of the manuscript).

The effect of blockage on the flow was quantified experimentally. Wind speed in the wind tunnel test section was measured at three locations shown in Figure 1 and the values are reported in Table 1.



Figure 1. Locations of wind speed measurement in the wind tunnel test section.

Table 1. Wind speed measured at different locations within the wind tunnel test section.

Location	Wind speed
Upstream	4.0 m/s
0.8D downstream WT1	4.2 m/s
WT2 rotor plane	4.3 m/s

The first increase of wind speed in correspondence of WT1 (+0.2 m/s,  $\approx 5\%$ ) is attributable to the global blockage induced by WT1, as already reported in Sect. 2.2 of the article. The additional  $\approx 0.1$  m/s observed at the WT2 rotor plane occurs when WT2 is present and is consistent with the same confinement mechanism acting on the first rotor. Importantly:

- The total speed-up at WT2 ( $\approx 0.3$  m/s over 4.0 m/s) corresponds to  $\approx 7\text{--}8\%$ .
- This magnitude is consistent with the blockage ratios and thrust coefficients of the present setup.
- It does not explain the much larger relative power differences observed between aligned and offset configurations, which are primarily governed by wake exposure.

To avoid ambiguity, we have revised the manuscript to clarify that the velocity increase at WT2 is attributed to wind-tunnel confinement effects associated with both rotors, rather than to a rotor-induced acceleration mechanism in unconfined flow. We have also clarified the expression “blockage effect of WT1” as wind-tunnel blockage induced by the presence of WT1 within the confined test section.

Furthermore, Sect. 4 (Discussion) already explicitly addresses vertical confinement and its influence on wake recovery mechanisms, and we have reinforced this discussion to make the role of confinement clearer.

## Response to Referee #2 comments

**R2:** *The paper discusses a very detailed and unique dataset for studying the impact of platform motions of floating turbines on downstream turbines. The experiments are unique due to the detailed load measurements, and the accurate control of the motion of the first turbine platform. The experiments allow to study idealized scenarios, as well as realistic irregular wave patterns, which delivers very interesting comparisons.*

### Limitations and context of the results

**R2:** *However, as is the case in almost every experiment: there are some differences or approximations from full-scale offshore conditions in this experiment. In general, the authors have done already a good job in discussing the limitations and context of the results, and what this means for comparison to full scale real offshore conditions (for example line 680 - 688). For example: the relatively small turbine spacing, low level of turbulence in the incoming flow, and the fixed platform of the second turbine. (I want to stress that this is not an issue, as it allows to study specific idealized scenarios which make it possible to understand better individual mechanism etc., and which are also more easily simulated).*

**A:** We thank the Reviewer for this constructive comment and for recognizing the effort made to contextualize the experimental results and their applicability to full-scale offshore conditions. Following this suggestion, we further clarified the discussion of the experimental limitations and their implications for extrapolation to real floating wind farms. In particular, the manuscript now explicitly discusses the influence of the relatively short turbine spacing, the low inflow turbulence intensity, the wind-tunnel confinement effects, and the assumption of a fixed downstream turbine. These aspects are addressed in the Discussion section, where we emphasize that the simplified experimental configuration was intentionally adopted to isolate specific wake-motion interaction mechanisms under controlled conditions, which are also more suitable for comparison with numerical simulations.

### Blockage of the ceiling

**R2:** *However, one limitation should receive a bit more discussion in the paper, and that is the blockage of the ceiling. While the authors mention that they think this won't play a role in relative comparisons, I think it may affect to some degree how the wake can spread due to the different motions. In either case, a bit more context for the reader would be good to interpret the results.*

*In general, I only have a few minor comments, and one request to discuss a more the limitations of the results due to the ceiling blockage.*

*Specifically: a spacing of  $0.2D$  between the rotor tips and the ceiling is small. This will most likely affect how the wake recovers or deflects, because it is closer or similar than the typical distance between the bottom tip and the ground, and we know that ground effects play a role in vertical wake deflection, thus the ceiling will limit the amount of wake deflection or how the wake expands to some degree.*

**R2:** *Line 405: The reviewer thinks that the small wake recovery may also be a sign of the blockage above the wind turbines. See for example the papers on vertical entrainment of mean kinetic energy in a wind farm required for wake recovery. Due to the ceiling blockage, the wake recovery must come mostly from the sides, while the vertical contribution (usually the largest) is missing. Can this also explain the slower wake recovery?*

**A:** The Reviewer's comment is on point. We agree that the limited vertical clearance between the rotor and the wind-tunnel ceiling may reduce vertical entrainment of high-momentum flow

into the wake, which is known to be a key mechanism governing wake recovery in wind farms. As suggested by the Reviewer, this confinement may contribute to the relatively slow wake recovery observed in the experiment, with wake mixing occurring primarily through lateral entrainment.

To address this point, we have expanded the discussion in the manuscript to explicitly acknowledge the potential influence of vertical confinement on wake recovery and to clarify that the ceiling may limit vertical entrainment compared to open-field conditions. The revised discussion now highlights that the persistence of the wake deficit in the present experiments is likely influenced not only by the low inflow turbulence and high thrust loading of the upstream turbine, but also by the restricted vertical mixing imposed by the wind-tunnel boundaries.

**R2:** *Line 419: That WT2 operates at a higher power when moved just outside of the wake of WT1 is something that has been observed in the literature before. Several simulation studies show an acceleration around the turbine. Furthermore, the wind tunnel experiments of Bossuyt et al. 2018 (Effect of layout on ... figure 13) also notice this effect when turbines are shifted 1D apart, though in that case at a smaller streamwise spacing. The fact that the tests in this paper see these phenomena at 5D downstream may be another indication of an extra blockage effects?*

**A:** We thank the Reviewer for pointing out this relevant reference and interpretation of results. We agree that the increased power observed when WT2 is positioned just outside the wake of WT1 is consistent with previously reported acceleration effects around turbines operating near the edge of a wake. Following the Reviewer's suggestion, we have added a discussion of this phenomenon in the revised manuscript and now explicitly refer to previous observations in the literature, including the wind tunnel experiments of Bossuyt et al. (2018).

In addition, we acknowledge that the relatively strong acceleration observed in present experiments—particularly at the larger downstream spacing of 5D—may be influenced by wind-tunnel confinement and associated blockage effects, which can enhance lateral flow acceleration around the wake. The discussion has therefore been expanded to clarify that while the qualitative behavior is consistent with prior studies, the magnitude of the effect may partly reflect the confined flow conditions of the experimental setup.

**R2:** *Line 420: about the conclusion 'the strongest impact by yaw motion'. Please consider that the proximity of the ceiling will dampen or block pitch or vertical wake deflections or motions in general. Thus, my opinion is that the authors should be careful to use this dataset to make this specific conclusion. If the authors find studies in the literature confirming that yaw would indeed have the strongest impact factor, then this can be used to justify that the conclusions in the paper still hold, even considering the blockage effects.*

**A:** We thank again the Reviewer for this important observation. We agree that the limited vertical clearance between the rotor and the wind-tunnel ceiling may constrain vertical wake deflection and therefore influence the relative effectiveness of different motion types, particularly pitch motion. As suggested by the Reviewer, we have revised the manuscript to clarify that the ranking of motion types in relation to wake recovery that has been observed in the present experiments may be influenced by the vertical confinement of the flow.

In the revised text, we now explicitly state that the ceiling may limit the vertical displacement of the wake and associated mixing mechanisms, which could reduce the apparent impact of pitch motion compared to yaw or crosswind motions in this specific experimental setup. The conclusions have therefore been reformulated more cautiously, emphasizing that while yaw

and crosswind motions produced the strongest wake recovery effects in the present experiments, further studies under less confined flow conditions are needed before generalizing this ranking to full-scale offshore environments.

**R2:** *Line 660: ‘The blockage effect ... ‘ It is great that the reviewers put in perspective the conditions for which these conclusions are relevant. However, the blockage could also affect even the relative comparison, in my opinion. For example, the vertical blockage by the ceiling may limit how much the wake can deflect vertically and also affect its wake recovery in deflected state. Thus, I think this sentence needs more nuance, and it would be helpful if the authors could discuss a bit more what the impact of blockage may be on wake recovery and wake deflection, and what this means for the conclusions in this paper.*

**A:** We again agree that flow confinement in the wind tunnel may influence not only the absolute magnitude of wake losses but also the relative comparison between different motion scenarios. In particular, the proximity of the ceiling may limit the vertical deflection of the wake and reduce vertical entrainment of high-momentum flow, which can affect both wake recovery and the effectiveness of motion-induced wake deflection mechanisms.

Following the Reviewer’s suggestion, we have expanded the discussion in the manuscript to provide a more nuanced interpretation of the potential effects of wind-tunnel confinement. The revised text now explicitly addresses how vertical blockage may influence wake recovery, wake deflection, and the relative effectiveness of different motion types. We also clarify that, while the experimental setup enables controlled investigation of motion–wake interaction mechanisms, the quantitative results and the ranking of motion effects may be influenced by the confined flow conditions and should therefore be interpreted with this limitation in mind when extrapolating to full-scale offshore wind farms.

**R2:** *For example, some thoughts:*

- *slower wake recovery & a more pronounced wake: this could accentuate more the individual impacts of the motions of WT1 on the loads and power of WT2. If the wake recovery would be faster, the wake would spread faster and the wake boundary would be less defined, and thus if we then move WT2 radially out of the center of WT1 (the 1R or 1D positions) it would likely see a slower decline in loads (depending on the motion of WT1)?*
- *Alternatively: if the inlet flow would contain much more turbulence, like in an atmospheric boundary layer, we can expect background turbulence to increase wake meandering, also reducing the sharp wake boundaries?*
- *blockage on the other hand, may reducing the effects of wake deflection in this experiment?*

**A:** We thank the Reviewer for these insightful observations and for outlining possible mechanisms through which the experimental conditions may influence the observed wake–turbine interaction. We agree that the relatively persistent wake in the present experiments, resulting from the combination of low inflow turbulence and wind-tunnel confinement, leads to stronger velocity deficits and sharper wake boundaries than would typically occur under atmospheric boundary layer conditions. As suggested by the Reviewer, this may amplify the sensitivity of the downstream turbine to motion-induced wake disturbances and influence how rapidly the loads decrease as WT2 is moved away from the wake centerline.

We have expanded the discussion in the manuscript to address these aspects more explicitly. In particular, the revised text now highlights that (i) the pronounced wake deficit and stronger velocity gradients in the present setup may enhance the sensitivity of WT2 loads to wake

oscillations compared to higher-turbulence atmospheric conditions, where increased mixing and wake diffusion would smooth the velocity gradients and reduce this sensitivity; and (ii) the vertical confinement imposed by the floor and ceiling may limit wake deflection, particularly in the vertical direction, thereby affecting the effectiveness of motion-induced wake displacement mechanisms. These aspects are now discussed in the context of the applicability of the present results to full-scale floating wind farm conditions.

### **Other minor comments**

**R2:** *Line 185: In the no-wind tests: because the rotor is not spinning, the centrifugal loads are not included. Is this a problem or big impact? Would be helpful to discuss in the text.*

**A:** Thank you for pointing this out. We have clarified this aspect in Sect. 2.1.1 when describing the load measurement and processing methodology. The revised text now explains that centrifugal loads arise from rotor rotation and therefore do not appear in the no-wind, rotor-locked tests. These loads are consequently not removed by the subtraction procedure used to eliminate inertial and gravitational loads associated with rigid-body platform motion. We also clarify that centrifugal imbalance loads appear at the rotor rotational frequency and its harmonics, which explains the 1P peak observed in the load spectra (Fig. 7).

**R2:** *Line 197: This one sentence contains a lot of information. If I understand correctly each hot-wire time signal is synchronized with the platform motion, and then conditionally averaged to specific positions of the platform motion? Should there be more context here about the number of samples measured per hot wire and per conditional point?*

**A:** We agree with the Reviewer that the previous text condensed too much information. The description of the wake velocity measurement methodology has therefore been expanded in Sect. 2.1.2. We now provide additional details on the synchronization of the hot-wire signals with the platform motion, the conditional averaging procedure, and the number of motion cycles used to ensure convergence of the phase-resolved velocity fields.

**R2:** *Line 366: include a relative measure of deviation (in %).*

**A:** Done. The relative deviations are now reported in the revised text.

**R:** *Line 386: typo for average.*

**A:** Thank you for noticing this. The typo in the section title has been corrected.

**R2:** *Figure 10 and 9: be clear on figure y-axis label: these are not just amplitudes, but amplitudes of the oscillations, correct?*

**A:** Yes, this is correct. To avoid ambiguity, the y-axis labels in the figures have been revised to explicitly indicate that they represent the amplitudes of the oscillations.

**R2:** *Line: 565: the apparent wind speed with opposite signs is not clear. A: the reviewer confusion is fully justified.*

**A2:** We thank the Reviewer for pointing out this lack of clarity. The text in Sect. 3.2.3 (Yaw motion) has been revised to better explain the mechanism. The description now clarifies that the observed velocity oscillations arise from the lateral wake meandering induced by the yaw motion of the upstream turbine. As the wake shifts laterally during the motion cycle, different portions of the downstream rotor are alternately exposed to lower- and higher-velocity regions of the inflow, which explains the observed oscillatory load response.

**R2:** *Interesting to check: how does the amplitude of the rotor motion compare to the amount of wake deflection for sway, yaw and pitch?*

**A:** We thank the Reviewer for this suggestion. Following this comment, we extended the analysis to quantify the wake displacement associated with the motion cases that induce periodic wake deflection (pitch, yaw, and surge–sway). The peak-to-peak displacement of the wake over one motion cycle is now estimated and discussed in the results section. In addition, for the surge–sway cases we compare the lateral displacement of the nacelle with the resulting lateral displacement of the wake. The results show that the wake displacement is significantly larger than the nacelle motion, indicating an amplification of the imposed perturbation as it develops in the wake. This observation is consistent with recent findings reported by Messmer et al. 2025 (<https://doi.org/10.1017/jfm.2025.10509>).

## Response to Referee #3 comments

The paper “Experimental investigation of the effects of floating wind turbine motion on a downstream turbine performance and loads” is well written and of very good quality. The experiments carried out are well described and results are presented in understandable way. Nevertheless, I would recommend addressing a few minor aspects to improve the readability and quality even more.

### Experimental set-up

**R3:** Where were the hot-wire probes positioned? Results e.g. in figure 8 and 9 and others, suggest a more or less continuous scan but that is not what happened. The authors should add markings where they measured the velocities and make clear that these figures are a results of interpolation.

**A:** We thank the Reviewer for pointing out this lack of clarity. In the original manuscript, the spatial distribution of the hot-wire measurement points and the interpolation procedure used to reconstruct the velocity fields were not described sufficiently clearly.

To address this, we have revised Sect. 2.1.2 (*Wake velocity measurements*) to explicitly describe the measurement grid and the positions where the wake velocity data were acquired. In addition, we have clarified in the captions of Figures 8, 10, and 11 that the velocity fields shown in these figures are obtained by interpolating hot-wire measurements acquired at discrete spatial locations. This makes it clear that the continuous-looking velocity fields result from interpolation of measurements performed on a finite grid of probe positions.

**R3:** Does the robotic platform generate a vertical velocity profile for the turbine WT2 in the wake?? It is not clear if the measurements in section 2.2 presented for WT2 in "free-inflow" are with or without the robotic platform in front or not. The influence of the robotic platform should be quantified.

**A:** The Reviewer’s comment is relevant. In the free-inflow characterization tests, WT1 was removed from the test section while the robotic platform remained installed. This configuration was intentionally adopted to reproduce the same flow conditions as in the two-turbine experiments, with the only difference being the absence of WT1. This clarification has now been added to the methodology section.

To further address the Reviewer’s concern regarding possible flow perturbations introduced by the robotic platform, we added a new appendix in which we compare measurements of the WT1 wake along vertical and horizontal lines passing through the rotor axis. The results show very similar velocity profiles along the two directions within the rotor-swept region, indicating that the robotic platform does not introduce significant distortions of the flow field affecting the downstream turbine. These additional results provide quantitative support for the assumption that the presence of the platform does not significantly alter the inflow experienced by WT2.

**R3:** section 2.1.1.: can the authors say anything about the accuracy of their calibration to exclude inertial and gravitational forces from their dynamic measurements? I know that it is a hard task, nevertheless it would be interesting to see if there could be put a number on this at least an estimation?

**A:** We thank the Reviewer for this important comment. Following this suggestion, we have added a dedicated appendix in which we assess the accuracy of the procedure used to remove inertial and gravitational loads from the aerodynamic force measurements. The analysis is

based on repeated no-wind tests with identical prescribed platform motion, which allow us to quantify the residual forces that remain after applying the subtraction procedure. These residual signals provide an estimate of the uncertainty associated with possible variations in structural response and noise in the measurements between tests.

### **Downstream turbine power**

**R3:** *section 3.1.2: even in the fixed case the measured power is way under the expected from other experiments — again the question remains, did the authors measure the WT2 performance for “free-inflow” with or without the robotic platform which could have an impact on the vertical profile in the wake.*

**A:** We thank the Reviewer for raising this point. The free-inflow characterization of WT2 was performed with the robotic platform installed in the wind tunnel, while WT1 was removed. This configuration was intentionally adopted to reproduce the same geometric conditions used in the two-turbine experiments, ensuring that the inflow experienced by WT2 remained consistent across the test campaign. This aspect is now clarified in the revised manuscript.

To further address the Reviewer’s concern, we added additional discussion and references to the new appendix, which compares vertical and horizontal wake velocity profiles. These results indicate that the robotic platform does not introduce significant perturbations to the flow within the rotor-swept region.

The relatively low power levels of WT2 in the wake of WT1 are therefore attributed to a combination of factors discussed in the revised Sect. 3.1.2:

1. the low inflow turbulence intensity in the wind tunnel, which limits wake mixing with the free stream;
2. the high thrust coefficient of the upstream turbine, which produces a strong and persistent wake deficit; and
3. the limited vertical entrainment of kinetic energy due to flow confinement between the wind-tunnel floor and ceiling.

These aspects have now been clarified in the discussion of the downstream turbine power results.

### **Dynamic loads of the downstream turbine**

**R3:** *section 3.2: the loads in WT2 occur with a delay (line 466) relative to the loads on WT1. What is the advection velocity (line 473)? Does that match the theoretical values?*

**A:** We thank the Reviewer for this interesting question. Estimating an advection velocity directly from the phase shift between the WT1 and WT2 load signals is not straightforward. The phase of the WT2 thrust oscillation is defined modulo  $2\pi$ , meaning that the measured phase lag ( $-140^\circ$  at the excitation frequency) is indistinguishable from phase shifts corresponding to additional whole motion periods ( $-140^\circ-360^\circ = -500^\circ$ ,  $-140^\circ-720^\circ = -860^\circ$ , ...). Therefore, the phase information alone does not uniquely determine a travel time.

Moreover, the WT2 load signal represents an integrated aerodynamic response over the entire rotor disk rather than a point measurement of a convecting flow structure. Several factors therefore complicate a direct interpretation in terms of a single advection velocity: (i) the non-uniform wake deficit across the rotor modifies the effective phase of the integrated load response; (ii) the coupling between WT1 thrust oscillations and near-wake velocity fluctuations

is not purely convective, as the near wake includes induction effects and coherent structures (e.g., tip vortices and shear-layer structures) that convect at speeds different from the local mean velocity; (iii) wake convection velocity varies with downstream distance and radial position; and (iv) the induction of WT2 modifies the incoming flow as structures approach the rotor.

For these reasons, we consider that estimating a single advection velocity from the measured phase shift would be highly uncertain and potentially misleading. We therefore chose not to include such an estimate in the manuscript and instead describe the observed delay qualitatively as being associated with the downstream advection of wake disturbances.

### **section 3.2.4 – Cross wind motion**

**R3:** *The caption in the figure 11 is not quite clear since the meaning of the angle gamma is not clear. In the text, it is written that gamma = 0° is pure surge motion — is gamma = 90° pure sway? What describes the amplitude of 0.032m? Since it is a combination of surge and sway each motion has an amplitude. Two sinusoidal signals (surge and sway) would result in a Lissajous figure as the motion pattern of the turbine model, depending on their phases it is either a straight diagonal, a circle or an ellipses. I think it would be helpful to add a sketch of the motion pattern of the turbine. The angle gamma does not really help to understand the motion of the turbine.*

**A:** We thank the Reviewer for this helpful comment. We agree that the original presentation of the surge–sway motion was not sufficiently intuitive. To improve clarity, we have revised both the text and Figure 11.

In particular, we added a schematic in Figure 11 showing the direction of the imposed translational motion and the definition of the angle  $\gamma$ . We also clarified in the text of Sect. 3.3.2 that  $\gamma = 0^\circ$  corresponds to pure surge motion, while  $\gamma = 90^\circ$  corresponds to pure sway motion. In addition, we now explicitly state that  $a_m = 0.032$  m is the amplitude of the imposed harmonic displacement  $d(t)$ , whose components along the surge and sway directions are obtained through the relations defined in Sect. 2.3.1. The figure caption has also been revised accordingly.

Because the surge and sway components are in phase in the present formulation, the turbine follows a rectilinear oscillatory motion along a direction forming the angle  $\gamma$  with the wind, rather than a Lissajous-type trajectory. We believe that the added schematic and the revised wording now make this motion definition much clearer for the reader.