Response to the reviewers

The authors would like to express their sincere thanks to both reviewers for their careful assessment. This document responds to each of the points raised. Reviewers remarks are in black, authors answers in blue. A "diff" file is also provided to enlighten the modifications made to the manuscript. The line numbers given in the authors' answers correspond to the ones of the final version of the article.

Reviewer 1

RC1: 'Comment on wes-2026-41', Anonymous Referee 1, 20 Mar 2026

General comments

Reviewer Comment 1.1 — The manuscript addresses a relevant and still open research topic concerning the effects of surge motion of floating offshore wind turbines (FOWTs) on wake dynamics. The study is timely and contributes to the ongoing effort to better characterize wake behavior under realistic turbulent inflow conditions. The literature review is generally well structured and provides an appropriate overview of previous studies. However, the discussion would benefit from a clearer differentiation of results obtained across different Strouhal number ranges and across different types of imposed motions (surge, pitch, heave). This would help to better highlight the specific contribution and novelty of the present work. The experimental methodology is robust. In particular, the combined use of hot-wire measurements and stereo-PIV provides a solid and comprehensive dataset. The application of coherence analysis together with Proper Orthogonal Decomposition (POD) represents an original and valuable approach, offering meaningful insight into the underlying wake physics. Finally, the manuscript should more explicitly discuss how the use of a porous disk instead of a rotating turbine model may influence the results and their applicability to real wind turbines. A clearer distinction between porous-disk-based studies and rotor-based studies is also recommended to improve the interpretation of the findings.

Reply: Except the experimental investigations involving the present research team (Belvasi et al. 2022, Schliffke et al. 2022 and 2024, Hubert et al. 2024 and 2025) and Kopperstad et al. 2020, which are performed with a porous disk, all the other references are based on experiments performed with a rotating wind turbine model. This statement is more clearly provided in the revised version the manuscript.
Regarding the use of a porous disk as an appropriate tool to mimic the wake generated by a wind turbine, one can refer to the introduction sections of two of our previous articles (Schliffke et al. 2024, Hubert et al. 2025): see answer to reviewer 2, comment 2.2.

One limitation of the applicability of the drawn results to real wind turbines resides in the hypothesis of constant thrust/drag coefficient, inherent to the use of a porous disk. Indeed, the static porous disk has a constant drag coefficient and we suppose that this property is conserved when it is moved at such a low Strouhal numbers, but we did not check it experimentally. Even if the static thrust coefficient curve of a commercial wind turbine shows a plateau with a maximal value in control region II (and so, it is the region for which the wake is the most intense), some thrust coefficient variations appear when a motion is imposed, which are strongly dependent on the rotor controller strategy. Neither the porous disk, nor the rotating model controlled with the rotational speed (to the best of our knowledge, small scale models are designed with this control strategy) can reproduce this complex behavior.

Modifications line 40: "Fu et al. (2019, 2020) carried out pitch motion experiments with a reduced-scale wind turbine ($A = [5, 10, 20]$, $St \in [0.0085, 0.28]$) in the case of low-turbulence inflows. They observed a faster wake recovery for imposed motion cases compared to the fixed case, which they attributed to greater shear layer instabilities at the wake border. Moreover, Kopperstad et al. (2020) performed wind tunnel experiments, with a porous disk, and numerical simulations of the wake of a wind turbine"
Line 52: "Bayati et al. (2018); Fontanella et al. (2021) performed wind tunnel experiments with a reduced-scale turbine model"
Line 64: "This phenomenon was also observed by Duan et al. (2022) for a rotating wind turbine model moving at $St = 0.55$".

Specific comments

Reviewer Comment 1.2 — The selection of the Strouhal number range is not fully justified. It is unclear why higher frequencies were not explored, especially given their relevance in previous studies. The authors should clarify the criteria used for this choice.

Reply: To answer this question, we can refer to the statements explicitly drawn in the previous paper from Hubert et al. (2025): Hubert, A., Conan, B., and Aubrun, S.: Spatiotemporal behavior of the far wake of a wind turbine model subjected to harmonic motions: phase averaging applied to stereo particle image velocimetry measurements, Wind Energ. Sci., 10, 1351–1368, https://doi.org/10.5194/wes-10-1351-2025, 2025.

"Regarding these studies and their results, three ranges of motion of the Strouhal number can be drawn according to their impacts on the wake. (i) The first one is motions at low Strouhal numbers ($St < [0.1, 0.2]$), where the wake is moved in a quasi-steady state succession. The motion impacts the global wake, and structures of the flow are displaced according to the turbine motion (e.g., Fu et al., 2019, 2020; Meng et al., 2022; Zhang et al., 2024). (ii) On the opposite side are motions at high Strouhal numbers ($St > [0.6, 0.7]$), where the motion impacts shear instabilities at the wake borders. The motion induces mild oscillations in the wake with a too high frequency compared to its characteristic aerodynamic timescale (e.g., Fu et al., 2023; Messmer et al., 2024; Li and Yang, 2024). (iii) Finally, between these two ranges are motions where they develop wake dynamics (e.g., Kopperstad et al., 2020; Fontanella et al., 2021; Li et al., 2022; Kleine et al., 2022; Messmer et al., 2024; Schliffke et al., 2024)".

The authors want to focus their study on the configurations where the overall wake dynamics is affected by the imposed motions ($St \in [0.2, 0.6]$), and seek for the most impactful excitation in this frequency range, which appears at a Strouhal number lower than 0.3. The motion system had been therefore designed to generate imposed motions in this Strouhal number range, with an upper technical limit of 0.33.

Added line 189: "For the frequency impact study, a focus is performed on the motions impacting the overall wake - *i.e.* with $St \in [0.2, 0.6]$ (Kopperstad et al. 2020, Fontanella et al. 2021, Li et al. 2022 , Kleine et al. 2022, Messmer et al. 2024, Schliffke et al. 2024, Hubert et al. 2025). The procedure unfolds as follows: first, Strouhal numbers of 3, 5, and 7 Hz are tested - *i.e.* $St = 0.14, 0.24, 0.33$, and their impact on the wake dynamics are investigated to determine which has the most significant one, thus designated as the case of interest. The two frequencies adjacent to the chosen case of interest are investigated, and the one with the greatest impact becomes the new case of interest. These steps are repeated three times to reach an accuracy of the order of 0.25 Hz."

Reviewer Comment 1.3 — The role of the motion amplitude is not sufficiently discussed. It would be useful to explain whether amplitude effects were considered negligible or simply outside the scope

Reply: Indeed, here the amplitude effects are outside of the scope. The authors wanted to focus their study on the impact of the frequency value rather than on the amplitude. A former study (Schliffke et al. 2024) showed in a similar experimental set-up as the present one that the surge motion amplitude impacts the amplitude of the turbulent energy peak in the wake but a parametric study on this motion amplitude effect was not performed neither.

Added line 490: "Finally, the amplitude of motion, out of the scope of the present study, would affect the results, and further studies on the dependence of the results (most impactful Strouhal number, emergence of the subharmonic peak) should be considered".

Reviewer Comment 1.4 — The estimation of S-PIV uncertainty (0.1 pixel) should be translated into physical velocity uncertainty to allow a better assessment of the reliability of the results.

Reply: As explained in the article, the measurement uncertainty is difficult to calculate, and depends of the experimental set-up. Indeed, the velocity uncertainty of the S-PIV systems is a combination of the numerous uncertainties present in the measurement chain, and is related to the set-up and to the post-processing algorithms. For example, errors from out-of-plane motion, particle density, pressure, non-uniform background, calibration, peak locking, algorithm response, vector replacement, might emerged - see Fig. 4 in Sciacchitano: Uncertainty quantification in particle image velocimetry, Meas. Sci. Technol. 30 092001, https://doi.org/10.1088/1361-6501/ab1db8, 2019. Recent developments in the process optimization lead to a reduction of these post-process uncertainties - *e.g.* self-calibration routines, sub-pixel interpolators, correlation peak-finding routines. However, the uncertainties related to the installation remain, such as out-of-plane motions or background non-uniformity. Thus, the 0.1 pixel units uncertainty quantify exclusively the uncertainty of the recorded images and many other 'hidden' factors are not taken into account, leading to the difficulty to predict the actual uncertainty of the velocity fields. Added in line 176: "However, a highly simplistic typical value states that the measurement uncertainty of the particle displacement on each plane obtained after cross-correlation, and before three-components reconstruction, is approximately 0.1 pixel (Adrian and Westerweel, 2011), that leads, in our cases, to an error of the order of 1.5% in the free-stream velocity at hub height."

Reviewer Comment 1.5 — The explanation of vertical wake meandering as a result of the interaction between shear and surge-induced velocity is not fully convincing. The inflow profile appears relatively flat (see velocity profile in Fig. 2, page 6), and the observed variations are comparable to the measurement uncertainty. The authors should strengthen this argument or reconsider the interpretation.

Reply: The inflow profile might appear relatively flat, but a velocity gradient does exist. As explained in the article line 429: "the inflow velocity differences between the top and bottom disk edges are about $0.03U_{\text{hub}}$ (Fig. 2)".
Moreover, the value of $0.03U_{\text{hub}}$ is a local shear, used as a reference value. It is important to note that

the physical phenomenon investigated here is larger than the porous disk, and is therefore exposed to a substantially stronger vertical velocity shear lower in the wind-tunnel boundary layer. Thus, as the observations are performed at $8.125D$ (D the diameter of the model) downstream of the porous disc, a slight local advection velocity difference between the upper and lower part of the wake might lead to a non-negligible time shift, responsible of the inclined coherent structures. Passing through the fixed (y, z) S-PIV measurement plane, they infer a detection of the wake center successively higher and lower than the actual wake center, leading to a vertical variation of the wake center location that can be assimilated to (or confused with) vertical wake meandering.

Additionally, this interpretation is submitted as a possible explanation rather than as a definitive conclusion. The simple calculations using different local advection velocities are presented in order to check if the hypothesis is conceivable, and, as explained line 439 : "Future investigations are necessary to validate this hypothesis and to provide more precise results". Several precautions have been added to the text.

Modifications line 421: "Large vertical far wake translations are seen in this surge case (Fig. 9 **(a)**). An hypothesis to explain this behavior is the shear present in the inflow, advecting the turbulent structures created by the disk at different local velocities, as seen for tip vortices in the presence of shear (see Lu and Porté-Agel (2011); Kleusberg et al. (2019))."
And line 426: "To check the plausibility of this hypothesis, an estimation of the phase delay that the surge-generated structures would have at $x = 8.125D$ downstream of the porous disk is performed".
And line 435: "Thus, the slight local advection velocity difference between the upper and lower part of the wake might lead to a non-negligible time shift, responsible of the inclined coherent structures. Passing through the fixed (y, z) S-PIV measurement plane, they infer a detection of the wake center successively higher and lower than the actual wake center, leading to a vertical variation of the wake center location that can be assimilated to vertical wake meandering. It explains why the POD mode associated with the vertical meandering χ_2 presents a significant peak of energy at the surge frequency. Future investigations are necessary to validate this hypothesis and to provide more precise results".

Reviewer Comment 1.6 — Related to the previous point: would the authors expect the vertical meandering phenomenon to disappear in a perfectly uniform inflow?

Reply: Same as the response of the previous comment: the phenomenon is not related to an actual vertical wake meandering but to successive inclined coherent structures.

However, yes, we expect that it does not exist in an uniform flow. That is what the conclusion suggests in line 485: "Indeed, the inclined periodic coherent structures are assumed to be due to the velocity difference between the upper and lower parts of the porous disk subjected to the shear flow. Similar experiments with varying shear could be performed, looking for a correlation between the inflow velocity difference and the resulting inclination of the coherent structures".

Reviewer Comment 1.7 — The assumption of a uniform horizontal inflow profile should be proved.

Reply: The entire wind tunnel, as well as the set-up used to develop an ABL-like boundary layer in the test section, is designed to obtain a uniform horizontal inflow. The verification of the horizontal uniformity is performed each time that an ABL set-up is modified. The difference between two velocity profiles measured at the center and at the $1/4$ width the of the test section presents a maximum of 2%. Added to line 147: " The horizontal homogeneity of the wind profile in the spanwise and streamwise directions in the test section was measured to be less than 2%. "

Reviewer Comment 1.8 — The incoming turbulence intensity (around 5–6%, page 6) is relatively high and may reduce the impact of the imposed motion compared to low-turbulence cases. A comparison of wake deficit and turbulence intensity (*e.g.* at hub height) between fixed and moving cases would help position the results with respect to existing literature.

Reply: Indeed, the aim of this study is to observe motion impact under a turbulent inflow, something that has rarely been performed. Here, the fixed case is only used as a reference. In Fig. 2, the normalized mean streamwise velocity and the turbulence intensity profiles, and, in Table 1, the inflow parameters, such as U_{hub} and $[I_u, I_v, I_w]$, are given as a comparison with literature. If needed, an overview of the wake deficit for the fixed case is given in the appendix Fig. A1 **(a)**.

Reviewer Comment 1.9 — From Fig. A1 (Appendix), it appears that the time-averaged velocity deficit is larger in the moving case than in the fixed case. Please confirm and discuss this observation.

Reply: In the present experimental set-up, the time-averaged available power is about $0.51P_{\text{ABL}}$ in the surge case, while it is about $0.56P_{\text{ABL}}$ (P_{ABL}, the available power in the inflow). The periodic surge motion induces a wake surface modulation that is skewed (higher expansion than reduction compared to the time-averaged wake surface - see Figure 12 in Hubert et al., 2025). Following the momentum conservation theory, it is associated with a similar skewed phase evolution of the velocity deficit, leading to a higher time-averaged velocity deficit. The reason of this skewness was not investigated either in the present paper, nor in the previous one.

Reviewer Comment 1.10 — The static vertical displacement of the wake center appears significant. Is this effect consistent across measurements and statistically robust?

Reply: The time averaged velocity field is calculated based on 14,000 instantaneous velocity fields, giving 3.199 independent measures and an uncertainty of $\bar{u} = 2.83 \pm 0.018$ m/s at the hub position. Thus, the result is statistically robust.

Moreover, this result is discussed line 388: "The position of the time-averaged wake is located above the disk center (Fig. 7), whereas the one for the fixed case is located slightly below, as expected in a positive shear flow or with the influence of the rod. Additionally, this feature was not detected in the previous study performed by Hubert et al. (2025) in a higher sheared inflow and a surge motion frequency of $St = 0.11$ or by Barile et al. (2026) through LES simulations of an actuator disk in a similar flow for a surge motion frequency range of $St = 0.1 - 0.5$ [...]".

Reviewer Comment 1.11 — The identification of a "critical" Strouhal number ($St \approx 0.24$) may be influenced by the experimental setup (porous disk, inflow conditions, measurement location at $8.125D$). The authors should comment on the generality of this result.

Reply: Modifications line 380: "Considering the differences in the studies (motion type and amplitude, experimental set-up, methods), the most impactful motion frequency found in the present work falls within the range of values reported in the literature".

Reviewer Comment 1.12 — The presence of the support rod wake is clearly visible (*e.g.* asymmetry in Fig. 7). It is unclear whether similar effects would persist in a rotating turbine case, where rotor-induced mixing could mitigate this influence.

Reply: Previous studies, using either an utility-scale wind turbine (Abraham et al. 2019) or a LES simulations (Santoni et al. 2017), have demonstrated that the presence of the rod can induce such a velocity deficit asymmetry. Figure A1 shows that both fixed and motion cases have this effect. However, the stronger asymmetry observed in the motion case is not documented in the literature, given the relatively recent development of FOWT research.

This article will not be able to answer that question, and further investigations, particularly studies comparing porous-disk and reduced-scale wind turbine models under motion conditions, are needed to clarify the respective roles of the support structure.

Reviewer Comment 1.13 — The origin of the subharmonic peak ($St/2$) requires further clarification. In particular, why does this phenomenon become more pronounced only at specific Strouhal numbers?

Reply: A complete answer to this question requires further investigation and probably to perform a stability analysis of this specific pulsating wake flow. We assume that this subharmonic peak is the signature of a secondary instability, which is a sub-product of the primary instability and can develop only when the primary instability, linked to the generation of coherent structures in the wake, is the most energetic. This presently happens when the excitation is performed at Strouhal numbers close to the natural wake instability, *i.e.* $St \in [0.2, 0.3]$ for canonical wake flows.

Reviewer Comment 1.14 — In Fig. 12 (coherence analysis, page 18), HW2 and HW4 appear in phase opposition while both are in phase with HW3. A more detailed physical explanation of this behavior is needed.

Reply: The authors believe that this inconsistency is more related to a methodological limitation than to an underlying physical phenomenon. Phase analyses are typically considered reliable only when the coherence between signals exceeds $C_{i,j} = 0.5$. However, under the high-turbulence inflow conditions considered in the present study, coherence levels between the hot-wire signals are relatively low, leading us to reduce the threshold to $C_{i,j} = 0.1$. The coherence $C_{3,4}$ at the subharmonic frequency barely reaches this value and therefore makes the assessment of the associated phase even more uncertain. Two options are possible here to address this issue. (1) Either keep the threshold as it is, but better stress in the text that the interpretation of phase values is uncertain, considering the low coherence values, or (2) increase the threshold to 0.2 or 0.25 and remove more values.

We opt for the first option and add in the manuscript line 327: "Furthermore, considering the high turbulence inflow in this study, the low level of coherence can lead to inconsistency in the phase results - *e.g.* HW2 and HW4 signals appear in phase opposition while both are in phase with HW3 one. Thus, a coherence threshold value of $\mathcal{C}_{i,j} = 0.1$ is defined in order to limit the uncertainties, but phase results should be interpreted with caution".

Reviewer Comment 1.15 — Regarding the POD results (Fig. 14), the repeatability of the modal amplitudes should be discussed. The observed discrepancies (especially for $St = 0.28$) should be quantified to distinguish between physical variability and measurement uncertainty.

Reply: The authors do not understand the comment of the reviewer. Figure 14 represents the amplitudes of the different POD modes, estimating the energy level of each mode on the global flow. This figure does not refer to any measurement uncertainty and, if differences between the studied cases are visible, they are not relative to any measurement issues.

Technical corrections

Reviewer Comment 1.16 — Figure 7: phase indications are incorrect.

Reviewer Comment 1.17 — Figure 14: labels and legend are incorrect or unclear and must be revised.

Reviewer Comment 1.18 — Figure A1: crosses (wake centers) and contour lines are missing

Reply: The technical problems reported in the comments 1.16, 1.17, and 1.18 neither appear in the primary .pdf nor the downloaded one for the authors. This might be caused by an issue with the .pdf reader software, and should not be visible in the final version.

Reviewer 2

RC2: 'Comment on wes-2026-41', Anonymous Referee #2, 14 Apr 2026

General summary

Reviewer Comment 2.1 — The manuscript presents a wind tunnel experiment on the wake of a porous disk, subject to surge motions. The well documented methodology includes hot-wire and PIV measurements, by that combining temporal and spatial resolution. The post processing is adequate and the two main findings are i) the Strouhal number corresponding strongest wake response and ii) the occurrence of a subharmonic component in the wake dynamics. The paper is well structured and the results are presented in a comprehensible way. While the topic of investigation is a very narrow and specific case, (and further abstracted by the use of a porous disk instead of a model turbine), the processing chain can be inspiring for follow-up research. Please consider the following remarks to improve the manuscript. Kind regards

General comments

Reviewer Comment 2.2 — Porous disk vs. (model) turbine: limitations, *i.e.* wake rotation, tip vortices

Reply: This recurrent question had been addressed in our previous publications (Schliffke et al. 2024 and Hubert et al. 2025):
Schliffke, B., Conan, B., and Aubrun, S.: Floating wind turbine motion signature in the far-wake spectral content – a wind tunnel experiment, Wind Energ. Sci., 9, 519–532, https://doi.org/10.5194/wes-9-519-2024, 2024.
"In Belvasi et al. (2022) and in the present study, the modeling concept for the wind turbine is based on the actuator disk theory, *i.e.* a porous disk. The porous disk has proven to be a cost-effective and reliable approach to experimentally generate a wake similar to a wind turbine wake. Aubrun et al. (2013) find that the porous-disk model produces a far wake nearly indistinguishable from the wake

of a rotating turbine at $x/D > 3$ downstream of both models, if immersed in a typical atmospheric boundary layer (ABL). All significant streamwise quantities, the statistical quantities and the integral length scale are similar beyond this distance, and the rotational momentum is not measurable. Several studies have shown that tip vortex signatures at the edges of the wake are undetectable at $x/D > 3$ (Aubrun et al., 2013; Zhang et al., 2012, 2013). The comparability has been extended to higher order statistics (Neunaber et al., 2021). Camp and Cal (2016) identify a limit of $x/D > 3.5$ for differences between the wakes of a rotating model and a porous disk to be negligible".

Hubert, A., Conan, B., and Aubrun, S.: Spatiotemporal behavior of the far wake of a wind turbine model subjected to harmonic motions: phase averaging applied to stereo particle image velocimetry measurements, Wind Energ. Sci., 10, 1351–1368, https://doi.org/10.5194/wes-10-1351-2025, 2025.
"The majority of experimental studies are performed with rotating wind turbine models, but the complexity of their design related to the difficulty of reproducing the right aerodynamic loads, especially under realistic turbulent conditions caused by the absence of Reynolds similarity, leads to the use of porous disk models (España et al., 2011; Aubrun et al., 2013). Due to the absence of tip vortices and angular momentum, a porous disk can be considered a far-wake generator, focusing the problem on the far-wake instabilities rather than on any other sources of instabilities. The goal of a porous disk is to reproduce the pressure difference found throughout the rotor of a real wind turbine. The turbulence and the rotational momentum created by each individual blade are not reproduced by this model, but it has been proven that these structures are negligible in the far wake (Vermeer et al., 2003) – *i.e;*, from $3D$ to $4D$ downstream of the wind turbine – and that a three-blade rotating wind turbine and a porous disk present the same wake properties even under low turbulence intensity ($I_u = 4\%$ after a downstream distance of $3D$; Aubrun et al., 2013)". Regarding the use of a porous disk in the case of imposed motions, we voluntarily limit our investigations to configurations with low Strouhal numbers (based on the disk/rotor diameter and on the motion frequency), *i.e.* $St < 0.6$, above which previous studies performed with rotating wind turbines (mentioned in the previous papers) showed that the motion preferably acts on the annular shear layer structuring close to the rotor, which is strongly linked to the tip vortices.

Added line 113: "The question of the limitations of the porous disk model compared to the reduced-scaled wind turbine one has already been addressed in Schliffke (2022); Hubert et al. (2025)."

Reviewer Comment 2.3 — Phase-averaged method: variance of individual cycles w.r.t. phase-averaged effects

Reply: Taking the surge case with $f = 5$ Hz, there are 3000 data in one cycle, and, taking into account the timescale of the test, this corresponds to about 2000 individual data for one cycle. Thus, this value allows the authors to be confident regarding the phase-averaged results.

Moreover, the uncertainty for each phase-averaged wake parameter is estimated with a confidence interval of 95%, based on the phase-averaged velocity fields, and are given with the colored zones in Fig. 9. The method is discussed in a previous paper: Hubert, A., Conan, B., and Aubrun, S.: Spatiotemporal behavior of the far wake of a wind turbine model subjected to harmonic motions: phase averaging applied to stereo particle image velocimetry measurements, Wind Energ. Sci., 10, 1351–1368, https://doi.org/10.5194/wes-10-1351-2025, 2025.
Section 2.2.2: "The phase-averaging method is applied to the S-PIV velocity fields according to the harmonic motion imposed on the porous disk, in FFoR and in MFoR. A kernel smoothing, defined by an Epanechnikov function (Wand and Jones, 1995; Hastie et al., 2009), is used to smooth the velocity

deficit fields over phases. The Epanechnikov kernel smoother is defined as:

$$K_h(t_\phi, t) = \frac{3}{4h}\left(1 - \left(\frac{t_\phi - t}{h\,\lambda}\right)^2\right) \tag{1}$$

where t is the time, t_ϕ is the reference time corresponding to the function maximum abscissa, and h and λ are the length scale and the bandwidth, also called window width, of the function, respectively". "Figure 6 illustrates the algorithm using the example of a noisy sinus function of period T: each sample $m(t)$ **(a)** is associated to its respective reference time phase t_ϕ, according to a phase ϕ of the imposed harmonic motion **(b)**, and the Epanechnikov kernel smoother K_h is computed for each phase increment to obtain the phase-averaged curve $\widetilde{m}$ **(c)**:

$$\widetilde{m}(t_\phi) = \frac{\sum_{i=1}^{N} K_h(t_\phi, t_i)\, m(t_i)}{\sum_{i=1}^{N} K_h(t_\phi, t_i)} \tag{2}$$

where N is the total number of data in the defined interval of the Epanechnikov function at the reference time t_ϕ (corresponding to the gray zone in Fig. 6 **(c)**). The statistical uncertainties, using a confidence interval of 95%, $I_{\widetilde{m}}$, are calculated using the formula:

$$I_{\widetilde{m}}(t_\phi) = \left[\ \widetilde{m}(t_\phi) - 2\,\frac{\sigma_{\widetilde{m}}(t_\phi)}{N}\ ;\ \widetilde{m}(t_\phi) + 2\,\frac{\sigma_{\widetilde{m}}(t_\phi)}{N}\ \right] \tag{3}$$

with $\sigma_{\widetilde{m}}$ the standard deviation of $\widetilde{m}$:

$$\sigma_{\widetilde{m}}(t_\phi) = \frac{\sum_{i=1}^{N} K_h(t_\phi, t_i)\, m(t_i)^2}{\sum_{i=1}^{N} K_h(t_\phi, t_i)} - \widetilde{m}(t_\phi)^2 \tag{4}$$

".

Reviewer Comment 2.4 — The role of the dynamic inflow effect in view of the surge motion (consider time scales)

Reply: The authors do not completely understand the comment of the reviewer; the Strouhal number represents precisely the ratio of the characteristic time of motion to the characteristic time of advection. In the incoming atmospheric boundary layer, the turbulence length scales at the hub height on the order of $3D$, corresponding to a Strouhal number of approximately $St = 0.3$. Thus, the most impactful surge motion is excepted to have a Strouhal number around this value, which is consistent with the present result ($St = 0.24$).

Added line 383: "Moreover, considering the presence of the atmospheric boundary layer, the turbulence length scales at the hub height are on the order of $3D$, which correspond to a Strouhal number of approximately $St = 0.3$. Therefore, the observed result at $St = 0.24$ is consistent with expectations, as the porous disk motion should interact more strongly with the incoming turbulent structures at this scale".

Reviewer Comment 2.5 — Sharpen the discussion of the involved wake meandering (both in introduction and discussion, following line 423)

Reply: Added line 120: "In the context of a turbulent inflow, a low-frequency non-harmonic phenomenon appears: the wake meandering induced by the turbulent scale larger than the turbine diameter (Larsen, 2007; España et al., 2011). This phenomenon is characterized by a global displacement of the wake, and should not be confused with motion-induced wake meandering. Moreover, Hubert et al. (2025) showed that the phase-averaged method, used also in the present article, erase the wake meandering linked to large-scale turbulence structures of the ABL".
And line 457: "It is important to note that the phase-averaging method employed in this study filters out the wake meandering induced by the large-scale turbulent structures present in the ABL. Thus, the observed wake dynamics can be attributed to the imposed motions only".

Reviewer Comment 2.6 — The conclusion got too long and detailed, repeating much of the introduction and methods but also bringing up new thoughts. Besides, the detailed literature references are not needed here – they suit better in the previous discussion chapter. I recommend to considerably shorten and condense the conclusions. Apart from that, the paper is well structured and the dedicated discussion chapter is a good choice.

Reply: Modifications line 461: "

- Hot-wire measurements are performed to identify the Strouhal number at which the surge motion has the greatest impact on the far wake of the turbine model. The impacts are estimated with the energy levels reached by the frequency peak associated with the motion in the wake velocity spectra of the hot-wire signals. The results show an energy peak increase until a maximum and a decrease, exhibiting a critical Strouhal number of $St = 0.24$.
- The far wake dynamics of the porous disk subjected to surge motion at this critical Strouhal number are investigated using an S-PIV system. It appears that surge motion leads to the contraction and the expansion of the wake surface in the crosswise plane, and modifies the available wind power within the wake. Inclined periodic coherent structures are induced by the surge motion in the far wake, leading to high variations of the vertical wake position that cause vertical wake meandering. The results show that wake crosswise surface and velocity modulations are in phase opposition: a large wake surface implies high velocity deficit (thus, a low velocity), and vice-versa, consistent with momentum conservation. Moreover, a quasi-steady-state analysis is performed, indicating that the surge motion impacts are amplified in the wake.
- Investigation of the inclination of the periodic coherent structures generated in the far wake indicates that the shear present in the inflow - *i.e.* velocity difference between the upper and lower edges of the disk - could lead to phase shifts similar to those seen in the experiments. This suggests that the coherent structures generated in the wake of a disk subjected to surge motion are elongated by the shear present in the ABL, resulting in the observed phenomenon.
- An unexpected energy peak is observed at the subharmonic frequency in the spectrum of the velocities in the far wake of the porous disk subjected to surge motion cases with $St > 0.25$.

It appears to be pseudo-periodic, with a maximum reached at half the motion frequency. POD and coherence calculations are performed, showing that the phenomenon behind this subharmonic frequency peak is essentially confined in the lateral direction, with left and right velocity variations in phase opposition. Thus, the far wake appears to meander in the lateral direction, adding up with the meandering induced by large-scale turbulent structures present in the inflow.

"

Specific comments

Abstract

Reviewer Comment 2.7 — Fine and informative.

Introduction

Reviewer Comment 2.8 — The first paragraph starts very fundamental with the reference to the 19th century. In my opinion you could shorten here and assume general awareness of the role of (offshore) wind turbines from a WES reader.

Reply: The first two paragraphs have been merged in one in order to be more succinct.

Modifications line 19: "In the context of growing global energy demand, wind turbines are increasingly located far from the coast, where the environment offers substantial advantages such as unobstructed wind flow and stronger winds. This industrial trend leads to the emergence of the floating offshore wind turbines (FOWT)".

Reviewer Comment 2.9 — Line 100: "quasi-steady-state analysis with basic wake models". Please name and reference the wake models here. The generic formulation is fine for the abstract though.

Reply: Modifications line 95: "Moreover, a quasi-steady-state analysis with basic wake models was performed (Bossuyt et al., 2021), showing that the observed spatiotemporal wake behaviors appear to be amplified and are not simply passively advected by the flow".

Reviewer Comment 2.10 — Line 110: Please conclude the research gap more thoroughly. The literature review is very detailed, but the take-away could be formulated more clearly.

Reply: Added line 106: "Several questions remain unanswered, as no clear consensus has yet been reached regarding the effects of platform motion on the wake of FOWTs. Therefore, experimental studies are essential to validate existing hypotheses and improve the understanding of the underlying physical mechanisms."

Reviewer Comment 2.11 — Line 114: Please mention here (or earlier in the introduction) the disk diameter of the experiment, so the reader already has an ap-front information of the involved dimensions.

Reply: Modifications line 111: "First, the 'critical' Strouhal number, for which the wake dynamics are the greatest, is determined by a spectral analysis based on hot-wire measurements performed $8.125D$ downwind of a porous disk ($D = 160$ mm) subjected to surge motions".

Methodology

Reviewer Comment 2.12 — The measurement systems are generally well described.

Reviewer Comment 2.13 — Line 148, 165: Good that you describe the HW calibration. Please comment on the accuracy at such low wind speeds (< 2 m/s in the wake), which might be tough to handle for the hot-wires. What's the wind speed range used for calibration?

Reply: The calibration includes 5 points in the range $[0.3 - 2]$ m/s, meanings that the accuracy of the hot wires in this range is correct. Moreover, at 1 m/s, the pressure sensor has an accuracy of 3%.

Modifications line 154: "They are calibrated every day in the free flow of the wind tunnel with a reference Pitot tube equipped with a Furness differential pressure sensor. The calibration procedure consists of 13 wind speed steps ranging from 0.3 m/s to 7 m/s including 5 steps between 0.3 m/s and 2 m/s. At 1 m/s the pressure sensor has an accuracy of 3%. The King's law coefficients (King, 1914) are then estimated using a best-fit approach with temperature and pressure corrections, resulting in a fit goodness value of about $\chi^2 = 1 \times 10^{-3}$".

Reviewer Comment 2.14 — Line 169: Please sharpen: 1px is just the algorithm accuracy. Type B uncertainties considering camera orientation might be larger that that. That might rather impact the uncertainty of *e.g.* the wake centre than that of the absolute wind speed.

Reply: See response to Comment 1.4.

Reviewer Comment 2.15 — Line 190: Please comment: when calculating the difference in PSD w.r.t. a fixed case: in how far could a meandering motion of the fixed disk distort the result. Maybe generally comment on what to expect for the fixed case, e.g. comparing to (Muller et al., 2015).
Update: I see that Fig. 5 actually shows this aspect well. Still, please comment on the background dynamics, either here or in section 3.1.

Reply: Results from the literature show that the meandering after a fixed wind turbine do not generate a sharp peak in the PSD (Muller et al., 2015, Schliffke et al., 2024) but rather a broad band response. In contrast, sinusoidal imposed turbine motion generate clear signature in the PSD (Schliffke et al., 2024). A modification is proposed in section 3.1 to recall this aspect.

Modifications line 244: "In Fig. 5 (a), E^* has no distinct peak, meaning that the natural wake meandering has a broad-band response, in agreement with results from Schliffke (2022). In contrast, all motion cases exhibit a clear signature at the motion frequency, indicating an impact of the porous disk on its wake dynamics, as seen in previous studies (Belvasi et al., 2022; Schliffke et al., 2024)".

Reviewer Comment 2.16 — Line 193: It's intuitive that HW1 shows best response to wake motion. Still, do the other HWs agree with the amplitude peak at St = 0.24? please comment briefly here or in the results section. Update: as I read the results section, I see the aspect discussed. Please reference to that section here, because it sounds as if the other positions were not considered for the processing from the start.

Reply: Added line 208: "The results of all HW are, nevertheless, discussed in Sec. 3.1 and in Fig. 6".

Result analysis

Reviewer Comment 2.17 — Line 247: consider changing the formulation "thanks to".

Reply: Modifications line 264: "Observations of the wake dynamics for the aforementioned motion frequency of $St = 0.24$ ($\mathbf{SPIV_{0.24}}$) were obtained using S-PIV measurements".

Reviewer Comment 2.18 — Line 260: comment on the impact of the tower wake. It seems to impact, and its relevance in comparison to the disk wake might rise, especially at phase angles of low disk wake you mention, seemingly pulling down the wake centre. The different tower diameter (in comparison to the disk diameter) could mean different mixing dynamics (coupled to St) are on hand for the tower.

Reply: Indeed, the Strouhal number linked to the tower is far lower than that of the porous disk - *i.e.* with a ratio of $St_{\text{disk}}/St_{\text{tower}} = 32$, meaning that they will affect the inflow at different frequencies, and thus, will show different wake dynamics.

In the present article, the wake is defined with a velocity deficit thresh value, and is not assumed to be round as is the case in other wake center tracking methods (see Hubert et al. 2022, hal-04280175). This leads to the observed lower location time-averaged wake center compared to the porous disk center.

Modifications line 278: "Furthermore, the upper and lower parts of the wake are not symmetric, due to the rod wake, which is taken into account in the wake center calculations."

Reviewer Comment 2.19 — Fig 7: check phase labels

Reviewer Comment 2.20 — Fig. 9c . check legend

Reply: For both comments, see response to Comment 1.18

Reviewer Comment 2.21 — Please comment on the presence of systematic y_c variations. It seems unexpected in a symmetric setup and with no wake rotation.

Reply: y_c variations appears significantly lower than those of z_c. Indeed, these variations could be assimilated to a bias of the wake center estimation algorithm, linked to the phase-averaged calculations, or to a experimental set-up issue, rather than to a physical phenomenon. For example, the porous disk might be out of alignment compared to the flow, resulting in a slight y_c variations at $8.125D$ downstream. Thus, the authors did not study these variations in detail.

Reviewer Comment 2.22 — Section 3.3.1: please introduce the coherence functions and phases

Reply: The authors consider that coherence functions and phase calculations are well-known post-processing methods that do not require an introduction.

Added line 321: "Coherence and phase analyses are performed to investigate the spatial manifestation of the observed phenomenon within the wake."

Reviewer Comment 2.23 — Fig. 14 fix legend

Reply: See response to Comment 1.18.

Reviewer Comment 2.24 — Section 3.3.2: (for later in discussion: why is the lateral mode considerably larger than the vertical one? Comment on the role of shear and the impact of the tower wake, please.

Reply: The authors are not sure to understand the question properly. We assume that it refers to the amplitude of the POD modes representative of the lateral and vertical meandering, respectively (Fig. 14), where a slight difference of energy is visible between modes 1 (associated to lateral meandering) and 2 (associated to vertical one). The exact values are given in Table 1. One can additionally notice that, since they represent the integration of the temporal modes energy spectra, they are not sensitive to the frequency signature of the motion, which is characterized by a thin frequency-band energy peak. Thus, the energy levels present in Fig. 14 are representative of meandering phenomena linked to large-scale turbulent structures present in the inflow. Since the ambient turbulence is anisotropic ($\sigma_u > \sigma_v > \sigma_w$, see Fig. 2), lateral meandering is more pronounced than the vertical one, resulting in a higher amplitude for mode 1 than for mode 2. Moreover, these observations are in line with those of previous studies, as specified in the article line 349: "As observed in several previous studies for a fixed wind turbine wake (Bastine et al., 2014; De Cillis et al., 2020), the curves show a slow decrease in the mode contribution to the total energy".
And line 353: "The results of the $\mathbf{SPIV_0}$ case are in accordance with previous works for a fixed wind turbine wake - *e.g.* Andersen et al. (2013); Bastine et al. (2014); Geibel and Bangga (2022)".

Tested case	mode 1	mode 2
$\mathbf{SPIV_0}$	0.043	0.032
$\mathbf{SPIV_{0.14}}$	0.044	0.031
$\mathbf{SPIV_{0.28}}$	0.044	0.033

Table 1: Amplitudes of the modes 1 and 2, normalized by the total amplitude of the POD (TKE), for the $\mathbf{SPIV_0}$, $\mathbf{SPIV_{0.14}}$, and $\mathbf{SPIV_{0.28}}$ cases.

Reviewer Comment 2.25 — Line 343: Maybe "preferably" isn't the best term here.

Reply: Modifications line 365: "However, the second mode seems related to the motion"

Discussion

Reviewer Comment 2.26 — Line 360: please comment on the role of the downstream distance on the results. 8.xx D seems at the upper limit of what's seen in the field.

Reply: The role of the downstream distance between the turbine model and the measurements is out of the scope of this study; the authors are unable to comment since only one distance has been tested. At the beginning of the paragraph, the authors highlight the link between the downstream distance and the obtained result, line 374: "At $8.125D$ downstream of the turbine model, the most impactful surge motion frequency is found for a Strouhal number of $St = 0.24$".

However, the authors understand the importance of specify the experimental set-up used in the present article, with modifications line 380: "Considering the differences in the studies (motion type and amplitude, experimental set-up, methods), the most impactful motion frequency found in the present work falls within the range of values reported in the literature".

Reviewer Comment 2.27 — Line 362: could this finding relate to the introduced threshold in combination with a smeared-out tower footprint for the motion cases?

Reply: In the motion case, the velocity deficit values are globally higher than in the fixed case, whether the wake of the porous disk or the wake of the rod. With the employed methods (threshold value and blur function), a stronger rod wake should lower the position of the time-averaged wake. Thus, the fact that the position of the time-averaged wake in the motion case is located above the disk center seems related to a stronger velocity deficit in the upper part of the wake.

Reviewer Comment 2.28 — Line 382: add a discussion of the influence (model) turbine vs. porous disk on the wake dynamics here.

Reply: The discussion is realized in the conclusion of the article, line 482: "One main limitation in the present study is the use of a porous disk instead of a rotating turbine model. This approach simplifies the study by focusing on the far wake response to external disturbances, but a comparison between both model types involved in similar atmospheric and floating motion conditions is needed to be fully confident in the conclusions drawn."

Furthermore, to the best of the authors' knowledge, no study has yet been conducted comparing the two models under motion.

Reviewer Comment 2.29 — Line 394-401: this is a helpful and relevant interpretation. Also, the numerical example in the following is helpful. Would it be possible to refine on the transport velocity here (*e.g.* refining the simple assumption of a transport velocity averaged over downstream distance)?

Reply: The authors cannot go further here; the measurements are performed at only one downstream distance, and making an assumption about this fact risks to be pure speculation. However, the discussion about the transport velocity is relevant, could greatly improve our understanding of the wake of a FOWT, and should be explored further in future studies.

References

Reviewer Comment 2.30 — Line 536: Incomplete reference

Reply: Modifications line 559: "Hubert, A., Conan, B., and Aubrun, S.: Spatiotemporal behavior of the far wake of a wind turbine model subjected to harmonic motions: phase averaging applied to stereo particle image velocimetry measurements, Wind Energy Science, 10, 1351–1368, https://doi.org/10.5194/wes-10-1351-2025, 2025"