In the first place, the authors want to thank the editor and reviewers for the time expended in reviewing our manuscript and writing their reports. We made a thorough revision of the manuscript taking into account all the comments. In what follows we answer each comment in particular. Also a version of the manuscript with the changes highlighted is included at the end of this document.

Comments from Reviewer 1

1. *Abstract, Line 19: Possible typo, should it not be "phase averaging"?*

Response: The authors thank the reviewer for pointing this out. The term has been corrected to 'phase averaging' in the revised manuscript (Abstract, lines 11 and 18).

2. *Line 28, I am not sure that the term "latter" works in this syntax. Please rephrase*

Response: The authors agree with the reviewer's suggestion. The sentence has been rephrased for better clarity (Introduction, line 26).

3. *Line 33, The description of the work of Schulz et al., is not clear, especially in the distinction between quasi-steady and unsteady effects. What are these unsteady effects? I believe that in this work Schulz refers to the returning wake effect. I am thinking that there might be no need to provide a description about unsteady effects in this part of the manuscript, and the authors should consider eliminating this part of the sentence.*

Response: The authors appreciate the reviewer's suggestion. Following this recommendation, the description of unsteady effects regarding the work of Schulz et al. has been removed to avoid confusion (Introduction, line 33).

4. *In the UNAFLOW campaign described in the work by Fontanella et al., 2022, the authors used a uniform inflow with about 2% TI. While the wind tunnel can be used to investigate the impact of ABL on structures and wind turbines, no ABL was considered in this or subsequent studies on the same FOWT model.*

Response: The authors thank the reviewer for this clarification regarding the UNAFLOW campaign. The text has been corrected in the Introduction, line 83.

5. *Line 112, I am not sure that the term "considered" is right when describing the hub height. I would suggest simplifying to "the hub height is [. . .]"*

Response: The authors thank the reviewer for the suggestion. The phrasing has been corrected in the revised manuscript (Numerical Setup, line 119).

6. *What was the blockage ratio during the experiments?*

Response: The blockage ratio during the experiments was 0.5%. This information has been added to the revised manuscript (Numerical Setup, line 118).

7. *Is it needed to provide the governing equations in the manuscript? If I am not mistaken, the authors did not modify them in comparison to their usual implementation. In that case, I am not sure that it is necessary to report them here.*

Response: Following the reviewer's suggestion, the governing equations have been removed from Section 2.1 (Numerical Model, line 130), as the implementation follows the standard formulations previously established in the literature.

8. *Line 135, typo, "applied in", I think it should be "applied to"*

Response: The authors thank the reviewer. The typo has been corrected to 'applied to' in the revised manuscript (Section 2.1, line 140).

9. *Line 137: how was the perturbation added in the turbulent case? Did the authors match the turbulent spectra from the wind tunnel? As further details about the inflow are provided in the following sections, the authors should highlight for the interested reader where the additional inflow information is provided.*

Response: The authors have clarified this point in the revised manuscript. A filtered noise boundary condition (based on Klein et al., 2003) was used to match the turbulent length scales of the ABL case. As suggested, in line 144, we have explicitly directed the reader to Section 2.3 for a comprehensive description of the inflow generation and the scaling process used to achieve the target flow.

10. *Line 141, how is the method different from the implementation in the literature?*

Response: The AD approach used here differs from the one presented in Navarro Diaz et al. (2019a, b, 2021) in two main aspects: (i) the nodes are organised in a ring-based layout instead of lines, and (ii) a constant thrust coefficient (C_t) is assumed. Given that these are minor implementation adjustments rather than a fundamental change in the underlying physics, the authors believe there is no need for a detailed description in the text.

11. *Eq (5): I am struggling to follow the reasoning of the authors in this part of the manuscript and I believe that clarity could be improved. When inserting the forces in the domain, when a motion of the platform is considered, I would imagine that the forces are proportional to the relative velocity (i.e., to the vector sum of inflow speed and the structural velocity). Why does the structural velocity not appear in Eq. (5)? Since the thrust coefficient is assumed constant (line 159) would the impact of motion not affect the inserted loads through a variation in apparent inflow speed?*

Response: The authors agree with the reviewer's observation. The thrust force is indeed calculated based on the relative velocity. The text in Section 2.2 (lines 160-166) has been revised to explicitly state how the motion of the platform affects the apparent inflow speed and, consequently, the loads applied through the Actuator Disk, even when a constant C_t is assumed.

12. *Line 159: As porous disk I imagine the authors are referring to the Actuator disk model? Was the thrust coefficient measured in the wind tunnel? Rather than "assumed" the thrust coefficient would be rather set to match the experiment, in that case. The authors should* *consider clarifying this. Additionally, was the thrust coefficient under platform motion cases verified under platform motion? Did previous work observe that the thrust coefficient can be assumed constant in the range of apparent speeds considered in this work?*

Response: The authors thank the reviewer for pointing out this lack of clarity. Indeed, we are referring to the Actuator Disk (AD) model used to represent the physical porous disk from the experiments. The value of $C_T = 0.65$ was not arbitrarily assumed but was set to match the wind tunnel measurements (Aubrun et al., 2019; Schliffke et al., 2024; Hubert et al., 2025). Regarding the platform motion, while the surge kinematics induce a variation in the apparent inflow velocity of approximately $\pm 25\%$, previous experimental campaigns with this specific porous mesh (Aubrun et al., 2019) have shown that its aerodynamic drag coefficient remains largely independent of the Reynolds number within this velocity range. Therefore, keeping C_T constant is physically representative of the rigid porous disk's behaviour under the modelled surge motion. We have clarified these points in Section 2.2 (lines 168-173) and updated the wording as suggested.

13. *Line 161: Why did the authors select a cross section of 10DX10D? If I am not mistaken this would make the cross section of the numerical domain smaller than the wind tunnel one. At this point of the manuscript it is not clear what the relationship between the numerical and experimental results. The authors should clarify why this test case has been selected and why they are matching it in their simulations. If the objective is to use the same thrust coefficient as the one measured in the experiment, should the numerical domain not match the same blockage of the wind tunnel, as this might affect the thrust coefficient?*

Response: The authors have addressed these concerns by clarifying the scope and the setup of the simulations:

(a) Objective: We have clarified in Section 2 (line 119) that while the study uses the same geometric scales and kinematics as the experimental campaigns, the goal is not a direct quantitative validation. Instead, the experiment provides a physically realistic reference framework to isolate and analyse fundamental wake dynamics.

(b) Blockage Ratio: Regarding the domain size, we have added a clarification in Section 2.3 (line 181). A $10D \times 10D$ cross-section results in a blockage ratio of less than 1%. Since this is well below the 5% threshold where confinement effects typically become significant (Sarlak et al., 2016), the domain is wide enough to avoid artificial acceleration and ensures that the C_T remains representative without needing to replicate the exact wind tunnel dimensions, thus preventing an unnecessary increase in computational time.

14. *Line 175, Why 5D height? Should the cross-section not be 10DX10D as previously described in the same section? Or is this difference due to different numerical setups employed for laminar and turbulent simulations? If so, this should be clarified and justified within the text*

Response: The authors thank the reviewer for pointing out this lack of clarity. The reviewer is correct, the domain height differs between the cases. As clarified in Section 2.3 (lines 197-206), the height for the ABL case was set to $5D$ to accurately model the Atmospheric Surface Layer (ASL). This setup, combined with a top stress boundary condition, prevents the artificial decay of turbulence in the upper domain and ensures a consistent ABL profile. Despite the reduced height, the blockage ratio remains low (1.5%), well below the 5% threshold for confinement effects.

15. *Line 182, Why did the authors use a stress BC for both top and bottom boundaries? To match realistic conditions the stress boundary would be placed on the bottom surface only. Additionally, why did the authors use two different models for top and bottom boundary conditions? This should be clarified for the reader*

Response: The authors have clarified this choice in Section 2.3 (lines 214-218). Two different stress boundary conditions were used to maintain a neutral ASL within the truncated domain:

(a) Bottom boundary: Uses Schumann's model to represent the physical surface roughness of the wind tunnel floor.

(b) Top boundary: Applies a shear-stress condition based on the friction velocity u^*.

This dual configuration provides the necessary forcing to sustain the targeted turbulence levels and prevents the profile from drifting.

16. *Line 190: what does "into consideration" mean in this context? Please clarify*

Response: The authors have rephrased this sentence in Section 2.3 (line 224) to remove the ambiguity. The text now explicitly states that the turbulent length scales used for the filtering process were set equal to the values obtained in the ABL flow.

17. *Sect. 2.4, what is the rotational speed? This value is needed to compute the Strouhal number*

Response: The authors would like to clarify that, since a porous disk model is used to represent a wind turbine in the experimental setup, there is no rotational speed involved. The Strouhal number (St) reported in this work is defined based on the surge frequency (f_{surge}), the disk diameter (D), and the reference velocity (U_{ref}), as specified in Section 1 (line 75) and Table 2.

18. *Section 3,Why did the authors use the term "Preliminary" in the section title? What is preliminar about the outcomes of the studies? Any know limitation of the study should be clearly stated in the manuscript.*

Response: The authors agree that the term 'Preliminary' could be misleading. It was originally used because this section introduces the initial LES results before applying further analysis techniques. To avoid confusion, the section has been renamed to 'Mesh sensitivity analysis and wake flow characterisation', which more accurately reflects the content (line 245).

19. *Figure 3, Why did the authors pick the mesh #2 for the analysis of results? It does not feel like the mesh is fully converged, as differences are observed in the wake width and in the velocity profile at the edges of the wake (about 0.5D). Did the authors also check what fraction of turbulence was solved by the LES approach in the wake with the selected mesh, verifying Pope's criterion?*

Response: The authors have modified section 3 to address the concerns regarding mesh convergence and spatial resolution. First, the convergence study was extended to 4D, 6D, and 8D downstream of the AD. The updated Figure 3 and the text in lines 255–259 now demonstrate that Mesh #2 achieves a consistent behaviour across the entire wake width at these distances, showing excellent agreement with the finer meshes in the centre of the wake. While minor discrepancies persist in the high-gradient shear layer at the very edges,

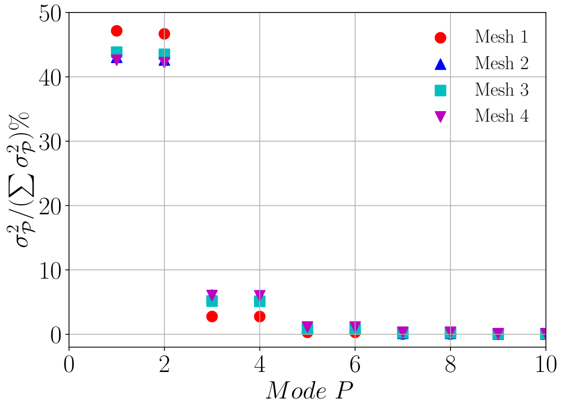
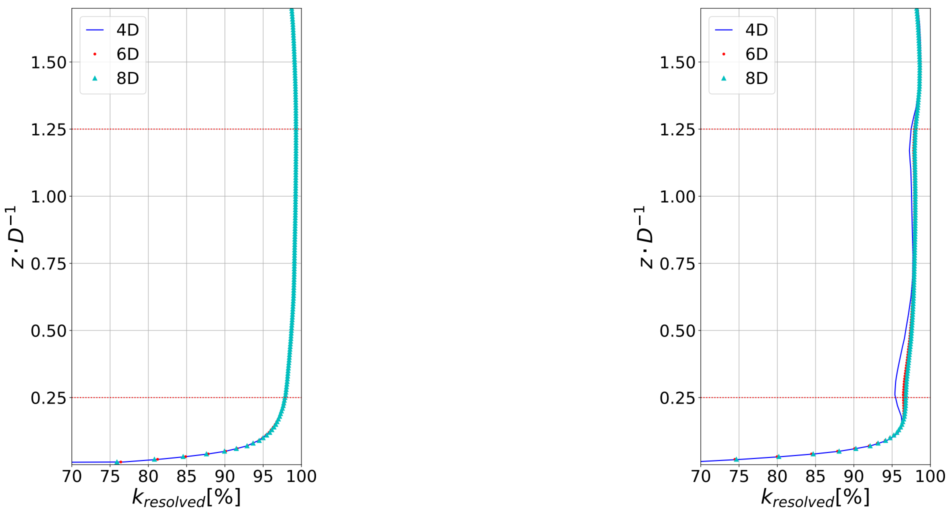


Figure A.1: Comparison of the energy fraction assigned to the POD modes associated with the vortex ring structure across the four different meshes (laminar inflow conditions, $S_t = 0.32$).



(a) TKE verification without AD

(b) TKE verification with AD

Figure A.2: TKE verification for ABL inflow conditions, without AD (a) and with the presence of AD (b), at 4D, 6D and 8D downstream the AD.

the overall resolution is sufficient to capture the relevant wake dynamics for this study. Furthermore, to ensure that the mesh resolution does not affect the identification of the dominant coherent structures, we performed a POD mesh sensitivity analysis. Fig. A.1 compares the POD analysis across the four meshes for the laminar inflow condition at $S_t = 0.32$. It can be observed that, starting from Mesh #2, the energy assigned to the modes linked to the vortex ring structure converges and is no longer affected by further grid refinement. This provides additional confidence that Mesh #2 is adequate for the analysis presented in this work. Additionally, Fig. A.2 shows the verification of Pope's criterion for the ABL case, confirming that the resolved turbulent kinetic energy (TKE) exceeds 95% of the total TKE in the wake region, both with and without the AD.

20. *Section 3, I wonder if the structure could be improved, as section 3 presents mostly the mesh-sensitivity analysis. Maybe the Q-criterion results be reported in a following section and Section 3 become something like "Mesh-sensitivity analysis"?*

Response: The authors have renamed Section 3 to 'Mesh sensitivity analysis and wake flow characterisation' to better reflect its content. Regarding the Q-criterion results, we have decided to keep them within this section as they provide the necessary qualitative transition between the mesh verification and the advanced POD post-processing in Section 4. Creating a separate section for the Q-criterion would result in a disproportionately short segment.

21. *Line 242, typo, "a converged statistics"*

Response: The authors thank the reviewer for identifying this error. The typo has been corrected to 'converged statistics' in the revised manuscript (line 300).

22. *Line 327, from the description of results, it is not clear from what result the authors draw a comparison between laminar and turbulent inflow conditions. As investigating the impact of different inflow conditions should be the focus of the analysis (as stated by the authors) the comparison between laminar and turbulent flows takes up a small part of the discussion. To highlight this comparison I wonder if the laminar results could be added to figure 13, at least for figure (c), highlighting differences in energy content, or if the comparison could be better shown in a different way. If the conclusions are drawn only from Figures 8 and 11, I find it interesting that, if I am not mistaken, in the laminar case the coherent pulsation of the first mode is amplified as it is convected downstream, while in the turbulent case it seems to be dissipated.*

Response: The authors appreciate this insightful comment. We have expanded the comparison between laminar and turbulent inflows to address these observations:

(a) Energy Comparison: As suggested, the laminar results have been added to Fig. 13 to provide a direct comparison of energy content. This highlights how background turbulence limits the energy retained by the vortex rings compared to the nearly 100% accumulation in the laminar cases.

(b) Spatial Evolution: We have clarified in the text that while coherent structures in the laminar case maintain their amplitude downstream, they indeed dissipate in the turbulent case due to the interaction with background turbulence and wake meandering.

These additions and the corresponding discussion can be found in the revised manuscript (Section 4, lines 396-407).

23. *Line 411, I wonder if the term wake meandering is adequate for the description of results with low inflow turbulence. In fact, looking at the arrows in Figure 19, it looks like the mode shows a clearly symmetric distribution around the centre line, which would suggest more a pulsation of the wake rather than meandering, with the wake expanding and contracting due to the rotor thrust oscillations.*

Response: The authors agree that modes 1-2 and 4-5 in Fig. 19 show a symmetric distribution corresponding to the wake's expansion and contraction (pulsation) due to thrust oscillations. However, the term 'meandering' is used to describe the behaviour of modes 3 and 6, which exhibit a non-symmetric spatial distribution. As shown in Figure 19, these modes display arrows pointing in directions that, when combined with their temporal evolution, result in a lateral oscillation of the wake.

24. *Figure 21, If possible, the size of the labels should be increased, as I find them particularly small.*

Response: The authors have increased the label size in Figure 21 to improve readability, as suggested by the reviewer.

25. *Line 451, looking at the laminar and low turbulence results of Figure 21 it might be worth highlighting that the intensity of the velocity oscillations is reduced for the turbulent inflow case.*

Response: The authors agree with the reviewer's observation. Section 5 (line 534) has been revised to highlight the diffusion in the intensity of velocity oscillations when moving from laminar to low-turbulence conditions, acknowledging the impact of background turbulence on the coherence of the wake structures.

26. *Line 470, I imagine that the authors want to highlight how for St=0.47, the half amplitude increases much faster, showing the peak value already at 3D. If this is correct, the sentence "except the highest frequency" might need further clarifying.*

Response: The authors agree. Section 5 (line 566) has been updated to clarify that for $S_t = 0.47$, the behaviour is similar to the laminar case, reaching its peak value at $3D$ followed by a continuous decay until $9D$.

27. *Line 470, from the description of the low-frequency results it seems that the onset of decay of the wake pulsation happens closer to the actuator disk than for the higher frequency cases. However, looking at results, the trend for the lowturbulence case seems opposite, as the velocity oscillation maxima gets closer to the AD with increasing St in Figure 22, b. I imagine the authors are referring to a faster decay in comparison to the laminar results of Figure 22, a. I would suggest clarifying this point for the reader by making the comparison with the laminar results explicit.*

Response: The authors have revised Section 5 (line 568) to make the comparison with the laminar case explicit. The text now clarifies that for lower frequencies ($S_t = 0.12 - 0.24$), the shift of the oscillation maxima towards the AD indicates an earlier onset of decay when compared to the laminar results, which is consistent with the lower energy levels observed in the POD analysis.

28. *Line 474, could the fact that the wake pulsation at these St numbers is unaffected by turbulence be correlated to the observed spectra in Figure 12 a and b? The wake spectra without motion shows a more significant frequency content for lower frequencies, which is then dampened out after about St=0.3, i.e, when the location of the peak seems not affected by turbulence.*

Response: The authors thank the reviewer for this insightful observation. While the frequency content in the background spectra (Fig. 12) suggests a potential correlation with the persistence of the wake pulsation near $S_t = 0.30$, a more detailed investigation would be required to confirm this link definitively, especially since in the case of ABL flow this pattern would not explain why all frequencies are affected with the exception of $S_t = 0.32$. This interesting point has been noted for future work.

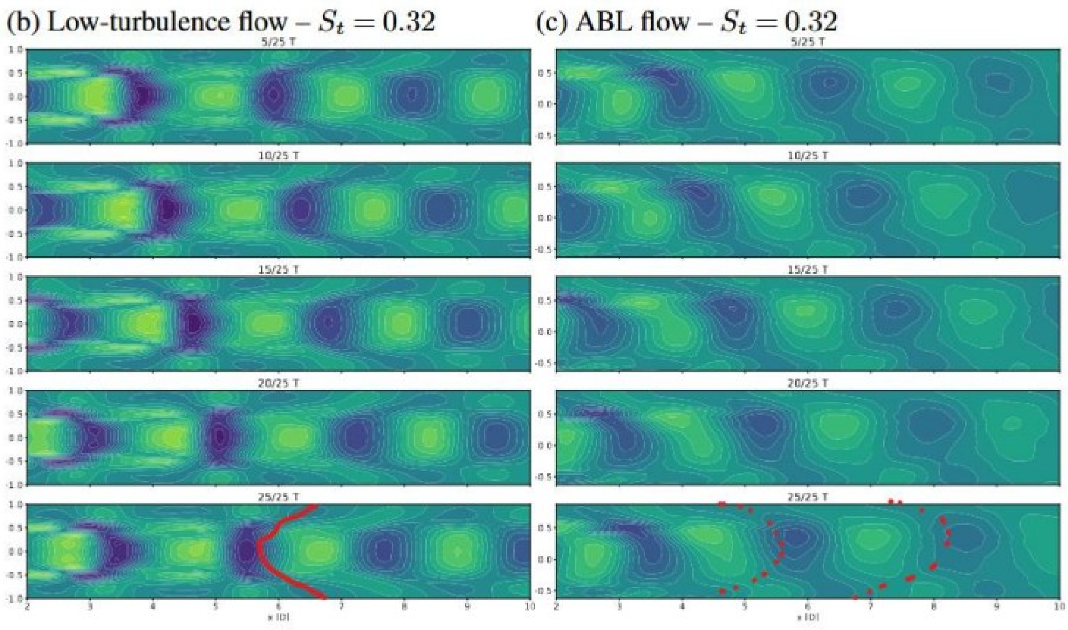
29. *Line 494, "inclination towards the left" could maybe be improved to inclination in the negative x direction, or towards the AD or something else?*

Response: The corresponding paragraph has been entirely rephrased in the revised manuscript (Section 5, line 591). The terminology mentioned by the reviewer is no longer present in the updated text, which now provides a clearer description of the observed phenomena.

30. *Line 508, I wonder if it is correct to describe the wake as the superposition of pulsation and wake meandering. Indeed, if I am not mistaken, it seems to me like the observed wake meandering is due to the interaction between the wake pulsation and shear layer of the ABL, which deforms the pulsation in the vertical direction leading to the observed velocity patterns. In fact, was there vertical wake meandering in the fixed-bottom ABL case which could justify this comment? My idea would also fit with the following description proposed by the authors about the impact of different vertical advection velocities and the onset of wake meandering (line 520).*

Response: The authors have updated Section 5 (line 612) to describe the wake as exhibiting contraction and expansion coupled with a vertical deformation induced by wind shear. Regarding the reviewer's query, vertical meandering was already present in the fixed-bottom ABL case (without surge motion), confirming that the observed results under surge conditions represent a coupling between the forced pulsation and the pre-existing meandering.

31. *Figure 23, I wonder how this result combines with the ones in figure 21. In fact, there the authors described an inclination towards the left, while here the same conditions are seen as an inclination to the right (i.e., in the streamwise velocity). While I agree with the latter, I wonder if in Figure 21, we are actually observing an inclination to the left, or if the inclination is still towards the right, but the low-velocity zones are merging across different wake pulsations. In fact, in Figure 23, the shift of the wake pulsation in the bottom side of the wake is about 2D, matching the lines I have drawn in the Figure markup and the expected slower advection due to the ABL. Otherwise, If the authors do not agree with this explanation, how do they connect the different inclinations between Figure 23 and 21?*



Response: The authors thank the reviewer for this observation. We agree with the as-

sessment and have re-evaluated the description of Figure 21 to ensure consistency with the physical evidence presented in Figure 23. To clarify that this complex pattern is likely caused by the merging of flow regions rather than a physical upstream tilt, the text in Section 5 (lines 544-555) has been revised as follows:
"Regarding the structural patterns, in both the laminar and low-turbulence cases, the regions of minimum fluctuation along the AD centreline appear to merge with off-centre zones, forming an upstream-facing curvature (towards the left) in the near wake ($x < 5D$). Further downstream, this pattern seems to invert into downstream-facing arcs. This behaviour could be attributed to the difference in advection velocities inside and outside the wake, a hypothesis that will be thoroughly analysed in the subsequent subsection. In the ABL scenario, the lower-fluctuation structures initially exhibit a downstream tilt up to $x \approx 4D$, likely resulting from the reduced advection velocity below the centreline caused by the shear flow. Beyond this point, an apparent sudden shift in inclination occurs, which may be caused by the merging of a low-velocity region from one structure with the subsequent one. Given the complexity of these flow features, they will be examined in greater detail and with clearer visualisations in the following subsection."

32. *Conclusions, Do the authors believe that the AD model can provide similar results to an actual three-bladed turbine model when describing the formation of coherent vortex structures? In fact, for a three bladed turbine under surge motion, the helical tip vortices, which are not captured in an AD model, might impact the behaviour of the vortex ring coherent structures. A comment should maybe included in the manuscript to highlight possible limitations of this study and/or future work.*

Response: The authors agree that while the AD model is robust for far-wake analysis, it cannot capture the discrete helical tip vortices or their initial breakdown. We have added a comment in the Conclusions (Section 6, lines 677-684) highlighting this limitation. This section now explicitly suggests that future work should utilise Actuator Line Models (ALM) or blade-resolved simulations to investigate how these helical structures transition and interact under realistic ABL conditions.

Comments from Reviewer 2

- *The main contributions should be more clearly articulated in the abstract and introduction to better guide the reader and emphasize the novelty of the work.*

  Response: As suggested, the Abstract and Introduction have been revised to more clearly articulate the main contributions and novelty of this work. Specifically, the Abstract now emphasizes three key findings:

  1. Energy-Frequency dependence: The identification of specific Strouhal numbers ($S_t =$ 0.30 and 0.32) where the vortex ring structures reach maximum energy across different inflows.
  2. Meandering Coupling: The discovery that surge-induced structures and meandering act as decoupled phenomena in low-turbulence flows, but become actively coupled under ABL conditions due to the interaction with shear flow.
  3. Directional Decoupling: The observation that vertical and lateral meandering in ABL conditions rely on distinct physical mechanisms, with lateral meandering being independent of the vortex ring structure observed in the present study.

4. Inclined structures: The inclination of the structures present in the wake due to gradients in the advection velocity under ABL flow.

- *The rationale behind the selection of the simulated cases should be explained in greater detail.*

  Response: The rationale behind the selection of the simulated cases has been expanded in Section 2.4 (lines 233-245). Specifically, we have added a detailed explanation of how the chosen cases relate to previous experimental campaigns, while establishing a link between these parameters and the operational conditions of modern large-scale turbines (10–15 MW).

- *A brief introductory description of the data analysis techniques employed would improve accessibility for readers who may not be specialists in these methods.*

  Response: The authors agree that a more detailed introduction to the data analysis techniques enhances the manuscript's accessibility. Accordingly, Section 4.1 (POD - Laminar) has been extended to provide a clearer framework for interpreting the Proper Orthogonal Decomposition results.

- *Further clarification is needed regarding the validity and representativeness of results obtained using an actuator disk model at model scale, particularly in relation to full-scale, bladed wind turbines.*

  Response: The authors have addressed the validity and limitations of the AD model in both the Introduction and Conclusions. The AD model is a well-established and robust tool for investigating far-wake evolution and the global impact of platform motion. Furthermore, the surge motions investigated in this study are characterised by a Strouhal number (based on the rotor diameter) below $S_t = 0.5$. In this regime, the primary aerodynamic interactions are expected to occur at the global rotor scale rather than at the individual blade or tip-vortex scale. Nevertheless, we explicitly acknowledge that the AD approach bypasses the generation and breakdown of discrete helical tip vortices. The revised text clarifies that while the fundamental surge-induced modulation remains consistent, future studies using Actuator Line Models (ALM) or blade-resolved simulations are necessary to explore the near-wake interactions of these helical structures under realistic ABL conditions.

General Comments

- *Section 2.4 – Cases Analysed. I recommend adding a brief discussion linking the selected Strouhal numbers (St) to the characteristic frequencies of floating wind turbines in the 10–15 MW range. When combined with near-rated wind speed conditions, the St values considered in this article are expected to correspond to the frequency range associated with the rigid-body modes of spar or semisubmersible platforms. Providing this context would help readers better understand the physical relevance of the selected cases. In addition, it would be useful to clarify why higher reduced frequencies (potentially representative of the wave frequency range, where large platform motions may occur due to wave excitation) were not considered.*

Response: The authors agree that providing a physical context for the selected Strouhal numbers is essential. As suggested, Section 2.4 has been expanded to link the S_t range (0.12–0.47) to the natural frequencies of 10–15 MW spar and semisubmersible platforms (0.005–0.05 Hz). In addition, we have specified that higher frequencies ($S_t > 0.5$), typical of first-order wave excitation, were excluded to focus strictly on the low-frequency, large-amplitude dynamics. Furthermore, at these higher Strouhal numbers, the relevant aerodynamic interactions occur predominantly at the blade scale (such as tip vortex dynamics). Investigating these high-frequency motions would therefore require more detailed approaches, such as Actuator Line Models (ALM) or blade-resolved simulations, rather than the Actuator Disk model employed in this study.

- *Section 4. The presentation of the POD results could be made more didactic to benefit readers who may not be familiar with this type of analysis. For instance, further explanation could be provided on why certain modes appear as opposite pairs and how they should be physically interpreted. This is particularly important given the paper's structure, in which similar types of results are presented repeatedly for different cases. I recommend including a concise introductory explanation of how to interpret POD results, either at the beginning of Section 4 or by using the results in Section 4.1 as a guiding example, to support the reader throughout the remainder of the section.*

  Response: The authors agree that a more didactic approach to the POD results is beneficial. Following the reviewer's recommendation, Section 4.1 (POD - Laminar) has been significantly expanded to serve as a guiding example for the rest of the manuscript. We have included a detailed explanation of the physical interpretation of POD modes, specifically addressing why they often appear as opposite pairs (sine/cosine phases) to represent convective downstream motion of coherent structures. By establishing this framework in the first subsection, we hope to provide the reader with the necessary tools to interpret the more complex cases (low-turbulence and ABL) presented subsequently.

- *L512: "this phenomenon ... of the wake". I consider this one of the most interesting findings of the study, and it deserves greater emphasis in both the abstract and the conclusions. The result appears to be relevant to any situation in which the free-stream inflow is non-uniform, which broadens its potential impact.*

  Response: The authors appreciate the reviewer's positive assessment of this finding. We agree that the tilt of the wake structures due to the vertical gradient in advection velocity is a key contribution that extends to broader non-uniform inflow scenarios. As suggested, this phenomenon has been given greater emphasis in both the Abstract and the Conclusions.

- L560: *"the present study ... in previous experiments". This statement would benefit from further clarification. It would be helpful to more explicitly explain the relationship between the present results and those reported in previous experiments, highlighting similarities, differences, and possible reasons for any discrepancies.*

  Response: The authors have clarified the relationship between the current findings and previous experimental work in both Section 5.1 (lines 603–607) and the Conclusions (lines 671–672).

Specific Comments

- *Abstract: The abstract is long. I recommend shortening it to focus more clearly on the innovative aspects of the work and the most significant results.*

  Response: The Abstract has been shortened and restructured to focus exclusively on the study's innovative aspects and most significant findings, as recommended.

- *L2: "due to mooring lines": The motion is more directly related to the high compliance of the floating foundation rather than to the mooring lines alone. Please consider clarifying this point.*

  Response: The text in the Abstract (line 2) has been corrected as suggested.

- *L30: "movements": Please specify whether these are rigid-body motions or motions associated with the overall platform response.*

  Response: The term has been specified as 'rigid-body motions' in the Introduction (line 28).

- *L33: "by the surge frequency": Quasi-static effects are not dependent on the motion frequency. Rather, they are driven by the apparent wind speed experienced by the rotor, which is induced by platform motion regardless of its frequency. I suggest revising this statement accordingly.*

  Response: The statement in line 32 has been revised to clarify that quasi-static effects are driven by the induced apparent wind speed resulting from platform motion, rather than the motion frequency.

- *L59: "this structure": It is not clear what "the structure" refers to. Please be more specific.*

  Response: The text in line 58 has been updated to explicitly specify the 'vortex ring structure', clarifying the previous ambiguity.

- *L101: "data": Consider replacing "data" with a more precise term, such as "measurements of velocities" or another description that clearly specifies the type of data used.*

  Response: The term 'data' in line 100 has been replaced with 'streamwise velocity measurements obtained experimentally' to provide more technical precision.

- *L203: "10s are left ... following 40s": How do these time intervals relate to full-scale conditions? Providing this information would be valuable, particularly for readers interested in applying a similar methodology to full-scale turbine simulations rather than a small-scale experimental setup.*

  Response: The relationship between experimental and full-scale time intervals has been added to line 253. The text now specifies that, based on a time scaling factor of 150, the intervals correspond to approximately 25 minutes of initialization and 100 minutes of data collection at full scale.

- *L204: "4D": Why was 4D selected? Are the results obtained at 4D representative of other downstream distances? Please clarify.*

  Response: While 4D was initially selected to maintain consistency with previous experiments, the study has been broadened to ensure the results are representative of the entire wake. Accordingly, Section 3 and Figure 3 have been updated to include the mesh independence study at 4D, 6D, and 8D.

- *L213: "Then, the two mesh refinements ... are recorded": The simulation procedure is not entirely clear. Please clarify whether this refers to a single continuous simulation with successive mesh refinements or to multiple separate simulations that were later combined.*

  Response: The simulation procedure has been clarified in lines 267–271.

- *L221: "experimental values": Please make explicit that the experimental results were used as the reference or target for the simulation setup.*

  Response: It has been made explicit in line 275 that the experimental results served as target for the validation and calibration of the simulation setup.

- *L242: "a vertical plane": It seems that multiple vertical planes may have been considered. If so, please use "vertical planes," or clarify that the same analysis is applied to several planes.*

  Response: To clarify, the POD analysis in the first sections is performed using several snapshots (time instances) but all within the same single vertical plane. The study then repeats this procedure for a horizontal plane in Section 4.4.

- *L527: "new": Please clarify in what sense this is "new" (e.g., new methodology, new physical insight, new configuration, or new application).*

  Response: The term has been clarified in the Conclusions (lines 632–634) to specify that the 'new' aspects refer to new physical insights regarding the vortex ring structure.

Comments from Reviewer 3

Specific comments

1. *L159, "The porous disk is assumed to have a constant CT". In my opinion, this assumption is correct in the case of a fixed inlet velocity and a still platform, but not so strictly in the cases analysed in the paper, since the relative velocity of the disk changes together with the platform motion. Perhaps the authors have previously observed that such changes do not come with a significant change in the value of CT. If this is the case, I suggest adding a brief explanation to strengthen the validity of the assumption. Furthermore, a discussion of a possible loss of generality (if any) when extending results of an actuator disk to a three-bladed rotor would help clarify the applicability of the framework presented in the paper.*

Response: The authors thank the reviewer for this observation. The assumption of a constant C_T has been clarified in Section 2.2 (lines 168–178). We explain that while surge motion induces variations in apparent wind speed of approximately ±25%, experimental data from Aubrun et al. (2019) confirm that the aerodynamic drag of the rigid porous mesh remains stable within this Reynolds number range. Additionally, we have added a discussion acknowledging that while this approach isolates the kinematic interaction between the motion and the wake, it does not account for the C_T variations (pitch control, tip-speed ratio) typical of a real three-bladed rotor. Furthermore, we have extended the discussion regarding the limitations of the AD approach in both Introduction and Conclusions.

2. *The choices described in section 2.3 regarding the dimensions of the computational domains should be clarified more in details. For example, if the intent is to compare experimental and numerical results to obtain a validation, it is not clear why the dimensions of the domain are smaller than those of the wind tunnel. Also, clarifications should be required to support the choice of a 5D height for the ABL domain, as well as for the use of different boundary conditions for the top and bottom walls.*

   Response: The details regarding the computational domain and boundary conditions have been expanded in Section 2.3. Specifically:

   (a) Domain Dimensions and Blockage: We clarify that the domain cross-sections result in a blockage ratio of less than 1% for uniform flows and 1.5% for the ABL case. Both are well below the 5% threshold where confinement effects become significant (Sarlak et al., 2016), ensuring that the smaller numerical domain (relative to the physical wind tunnel) does not introduce artificial acceleration.

   (b) Domain Height (5D): The choice to limit the height to $5D$ in the ABL case is justified as a strategy to model the Atmospheric Surface Layer (ASL). This prevents the artificial decay of turbulence that occurs in deep domains without temperature-driven inversion layers.

   (c) Boundary Conditions: We have clarified the use of stress boundary conditions for the ABL case. A Schumann's model is applied to the bottom to represent surface roughness, while a top shear-stress condition (based on friction velocity u^*) is used to provide the necessary forcing to maintain the turbulent profile within the truncated domain.

3. *Section 2: The dinstinction between the three conditions is appreciated and essential to understand the following of the paper, including section 2.3 for instance. IS shear considered in the low turbulence case?*

   Response: It has been clarified in line 222 (and further detailed in Section 2.3) that the low-turbulence case does not include shear. The targeted mean flow for this case is entirely uniform, with only the turbulent fluctuations superimposed on the signal.

4. *The conclusions drawn from the mesh convergence study presented in section 3 may be leave room for discussion, as the mesh #2 does not seem to be fully converged (for example in the shear layer between 0.75 and 0.25 D). Furthermore, demonstrating the level of convergence of more than one section downstream would help to provide a stronger generality to the conclusions of the convergence study.*

Response: The mesh convergence study in Section 3 has been expanded to address these concerns. We now include wake profiles at 4D, 6D, and 8D downstream of the AD to demonstrate the generality of the results. The updated text clarifies that while minor differences exist in the high-gradient shear layer, Mesh #2 shows consistent behaviour and strong agreement with the finer meshes in the centre of the wake at all three locations, confirming that the spatial resolution is adequate for the study's objectives. Furthermore, to ensure the robustness of our primary methodology, we conducted an additional POD mesh sensitivity analysis. As illustrated in Fig. A.1 (page 5 of this document), the energy assigned to the modes associated with the vortex ring structure (evaluated under laminar inflow conditions at $S_t = 0.32$) converges from Mesh #2 onwards. This confirms that the selected grid resolution accurately captures the coherent structures driving the wake dynamics without being affected by further refinement.

Technical corrections

Response: All technical corrections listed below have been addressed and incorporated into the revised manuscript.

1. *L3, possible typo, I think "has been the subject of study" would sound better.*
2. *L4, possible typo, I think "as most studies" would sound better.*
3. *L10, typo, "with the highest energy at Strouhal numbers30 and 0.32, respectively".*
4. *L54, typo, "... six degrees of freedom...".*
5. *L60, typo, "Due to these reasons".*
6. *Figure 4 caption, typo, "All profiles are zoomed in the refined region. The dotted red...".*
7. *L260, typo, "see Table 2".*
8. *L260, possible rephrasing for improved clarity, i.e. "In all figures, the most energetic modes include pairs with similar energy content".*
9. *L283, possible rephrasing, "ST = 0.32 exhibits higher energy in the first pair of modes and lower energy in the harmonics".*
10. *L310, please rephrase "The range of frequencies in these modes, not shown here, are a great match with..." or correct the verb "are" with "is".*
11. *L339, typo, "mode pairing".*
12. *L391, typo, "This comprehensive comparison with laminar and low-turbulence inflows has revealed ..."*
13. *L395, perhaps "ABL inflow conditions" sounds better than "ABL flow".*
14. *L444, typo, "the local mean velocity is subtracted" or "the local averaged velocity is subtracted".*
15. *L503, typo, "... due to the surge motion".*
16. *Coherence should be maintained with British or American English, since in the manuscript terms like visualise/visualize or analyse/analyze are present at the same time. The same applies to the terms disc/disk.*

Comments from Reviewer 4

General

- *The study is obviously designed in a way that the experiments of Schliffke et al. could be reproduced. However, no comparison with their results has been performed, which is quite surprising. Please explain in the paper, why this is the case and why you still choose to replicate the rest of the setup. I guess this is due to the fact that Schliffke et al. Considered a Ct of 0.5, which is rather low. However, at least a short validation case with Ct=0.5 and a comparison with the experimental data would be very beneficial. This uncertainty (what is actually replicated here and why?) remains through the paper and should be clarified to strengthen the red line of the study. It is also very important to discuss the chosen motion conditions. They have been adapted from Schliffke, however, according to my understanding, they represent a rather small 2MW rotor at very slow motions (between 100 and 16s of motion period at 8m/s) and quite large motion amplitudes (12.5% of the rotor diameter). The chosen motion parameters should be set into the context of modern turbines.*

Response: The authors thank the reviewer for these insightful comments, which have led to significant clarifications in Section 2:

1. Replication vs. Validation: We have clarified that while the study adopts the geometric scale, inlet profiles, and motion parameters of the experimental campaigns, its primary goal is not a direct quantitative validation. Instead, it uses this well-characterised setup as a physically realistic reference framework to isolate and analyse fundamental wake dynamics under controlled conditions (Section 2, lines 119-122).

2. Thrust Coefficient (C_T): We particularly appreciate the reviewer's observation regarding the C_T value. While previous literature (Schliffke et al., 2024) mentioned a $C_T = 0.5$, it was confirmed to be 0.65. This discrepancy was addressed with the experimental authors, leading to an official corrigendum in the respective journal (doi:10.5194/wes-9-519-2024-corrigendum). Our study correctly uses $C_T = 0.65$ to match the experimental measurements of the porous disk (Aubrun et al., 2019), as now detailed in Section 2.2.

3. Context of Modern Turbines: We have added a dedicated discussion in Section 2.4 regarding the scaling of the motion parameters. Although the original experiments were based on a 2 MW turbine, the analysed Strouhal numbers ($S_t = 0.12$ to 0.47) correspond to the natural frequencies (0.005 Hz to 0.03 Hz) and rigid-body motions of modern 10 MW to 15 MW floating turbines. The motion amplitude of $D/8$ is justified as a bounding scenario for extreme resonant responses in these large-scale structures.

Abstract

- *The abstract is comparatively long and it is recommended to make it shorter. The description of the simulation study is quite detailed for an abstract, however, still difficult to follow since it seems that the authors try to explain the complete study in detail here. It is recommended to reduce the level of detail and focus on the basic idea of the simulation study and the main findings, while leaving out how this was achieved. It would be beneficial to move a bit more towards a bird's eye perspective.*

Response: The abstract has been significantly condensed as suggested. We have reduced the technical procedural details of the simulation and focused the text on the core objective and the most significant findings.

Introduction, numerical modelling and experimental studies

- *A number of relevant publications in the field are listed and described here. However, only very few conclusions relevant for this study are drawn from the description. It would be beneficial to really discuss some of the studies in the context of the present work. For example, the role of the motion frequency is not really discussed. The suitability or limitations of the used method could also be discussed using the literature.*

Response: The section of the Introduction regarding the numerical modelling of FOWT wakes has been specifically rewritten to address the reviewer's request for a deeper discussion of existing literature. The new paragraph now discusses the role of motion frequency and justifies the use of the AD methodology.

Solver

- *It is written that "a newly implemented solver in OpenFOAM (OpenCFD-Ltd, 2004), which was constructed using the SOWFA libraries as a basis (Churchfield et al., 2012a, b)" was used in to perform the simulations. First: Is this an in-house development? If yes: Please either describe the implementation or give a source. What is implemented and how is it verified? What part is taken from other sources and what is the own contribution? If no: Please give a source, where the actual implementation is described. If you use the SOFWA code, the version of the code should be named and the source for the actual implementation should be cited (at least the github repository).*

Response: The description of the numerical solver has been clarified to specify that it is based on the standard SOWFA libraries (NREL, 2012; Churchfield et al., 2012a, b), effectively utilizing its well-verified core framework. We have made explicit that our implementation introduces only two specific modifications to the standard code:

1. Simplified Physics: Temperature variations and buoyancy effects were disabled to focus exclusively on the fluid mechanical behaviour.
2. Adapted Driving Mechanisms: We modified the forcing to suit different inflow cases. For the ABL, a forced pressure gradient is calculated based on a precursor region (restricted to hub-height velocity to avoid bias from the wake), while the laminar and low-turbulence cases are driven by inlet boundary conditions (fixed velocity or filtered noise) rather than a pressure gradient.

The github repository for the utilised version is included in the citation.

Inflow conditions

- *A discussion on the chosen ABL parameters is missing: Is there only one ABL? What is special? What is chosen and why?*

Response: The physical characteristics of the chosen ABL have been clarified in Section 2.3 (lines 226–230), where we specify that the flow is modelled to replicate the scaled wind tunnel boundary layer from the experimental study by Schliffke et al. (2024) rather than a specific geographic site. To ensure a physically realistic and reproducible setup, the target velocity and turbulence intensity profiles were configured following the VDI guideline 3783 for atmospheric boundary layer wind tunnel modelling. This configuration was selected to provide a standardised, strongly sheared, and highly turbulent baseline, which is essential for establishing a clear contrast with the uniform, non-sheared inflow cases and thus isolating the impact of ABL-specific features on the wake dynamics.

POD laminar

- *"Furthermore, the frequencies exhibited in the modes, as illustrated in Fig. 9a, show a smooth distribution across a range of frequencies. Additionally, modes 3 and 4 display a complementary spectrum when compared with modes 1 and 2. Modes 5 to 10 (not shown here) also demonstrate analogous complementary behaviour with regard to frequency. This finding suggests that the observed structure is inherent to all frequencies within a range, thereby confirming the absence of any structure being advected that can be associated with a specific phenomenon."*
  This seems a bit inconsistent to me. Actually, Figure 8 shows clear structures in modes 1-4. The corresponding Fourier spectra actually show that there is not one isolated frequency associated with the modes, however, the range of corresponding dominant frequencies is rather narrow (St 0.2-0.55), and far from being distributed randomly. Therefore, I cannot really follow, why there should be an "absence of any structure being advected that can be associated with a specific phenomenon". The fact that this structure seems to be rather weak (6 magnitudes smaller) seems convincing on the other hand. However, when summing up the Fourier amplitudes in a range of St e [0.2-0.55], I would expect to see a similar (or even higher) value compared to the other two cases. Let's assume that there are structures generated in case of St=0; however, these structures would be related to some kind of "natural" meandering or sth. similar. In this case, the structures would not necessarily evolve at the exact same frequency, since their generation is somehow of a statistical nature. As known from other studies, such meandering should be pronounced around St 0.3. In this case, the assumed structures in the wake would be present, however, their frequency and lateral expansion would slightly vary over time. A Fourier transform on this (let's say for a time of 40s), would actually not show a clear peak, but more a stochastic distribution around St 0.3. From my point of view, this could be in line with what we see in Figure 9a. If this is true, we would actually have some stable structures in the wake. which slowly vary their frequency. However, due to the long window of time, we cannot exactly see them in the POD analysis, since it requires the structures to happen at the EXACT same frequency and lateral expansion.

Response: The authors are very grateful for the reviewer's insightful reflection on the frequency distribution for the $S_t = 0$ case. We agree that a broadband spectrum in the range of $S_t \approx 0.2$–0.55 could indeed suggest physical mechanisms whose frequency and spatial expansion evolve statistically over time, such as wake meandering. However, as the reviewer correctly noted, the energy content of these modes is extremely low (six orders of magnitude smaller than the mean field). Furthermore, the spatial modes remain strictly symmetric, which is inconsistent with the typical alternating patterns of meandering. Consequently, while we have incorporated the reviewer's observation into the text to acknowledge the possibility of evolving structures in lines 335-342, we conclude that these represent negligible residual fluctuations rather than dominant coherent phenomena.

POD turbulent

- *For the St=0 case, the maximum frequency of Mode 1 occurs somewhere around 0.075Hz, which is far from the maximum frequency observed in the laminar case. Does this frequency somehow relate to the turbulence length scale of the low turbulence case (6D)?*

Response: The authors appreciate this detailed observation regarding the spectral peak in the low-turbulence case for $S_t = 0$. We have carefully reviewed the relationship between the peak frequency and the turbulent length scale ($L_x \approx 910$ mm). It is important to note that the figures are presented in terms of the Strouhal number (S_t) rather than absolute frequency. For an inlet velocity of $U_{ref} = 2.71$ m/s and a disk diameter of $D = 0.16$ m, the peak mentioned by the reviewer at $S_t \approx 0.075$ corresponds to a frequency of approximately 1.27 Hz. On the other hand, the characteristic frequency associated with the longitudinal turbulent length scale ($f_L = U_{ref}/L_x$) is approximately 2.98 Hz, which yields a $S_t \approx 0.17$. While these values do not coincide exactly, they are within the same order of magnitude. This suggests that the energy distribution in these modes is indeed influenced by the scales of the incoming turbulence, possibly linked through harmonic interactions or the spectral decay of the low-turbulence inflow.

Phase averaging

- *I understood that the phase averaging in Fig 21 was performed at the motion frequency (St=0.32). Is this correct? If yes, I would expect a behaviour in Fig. 21 (a), which is somehow similar to modes 1 and 2 in Figure 8 (c), since those occur at the exact same frequency. However, in Fig 21 (a), the distance in downstream direction between minima and maxima of the pulsation is approximately twice as high, which indicates a much lower frequency (half the motion frequency?). Maybe I am getting something wrong here. It would be helpful if you could explain this.*

Response: The reviewer is correct that the phase averaging was performed at the motion frequency ($S_t = 0.32$). The perceived difference in the spatial wavelength between Fig. 21(a) and the POD modes in Fig. 8(c) is due to the physical quantity being represented. While the phase-averaged fields in Fig. 21(a) show the streamwise velocity component, which includes the sign of the fluctuations, the POD spatial modes in Section 4 are based on the velocity magnitude. We have added an explicit clarification in line 538 to ensure the reader understands this distinction.

Conclusion

- *"Overall, the study highlighted that the vortex ring structure exhibits a significantly different behaviour depending on the inflow conditions. In particular, the presence of ABL shear not only modifies the spatial shape of the modes but also strongly influences their amplification and energy distribution."*
  From my point of view, this has not been shown directly. I guess, the fact that the "ABL shear ... strongly influences their amplification and energy distribution" is derived from Figure 12 (a-c). Here, it is shown that the amplification strongly changes from the low-turbulence to the ABL case. However, the difference between the low-turbulence to the ABL flow is not only the ABL shear, but also a significant increase of the overall turbulence level. Therefore, I would not be able to distinct if the shear or the increased level of turbulence caused this.

Response: The authors agree with the reviewer's observation. In our original text, we attributed the observed changes primarily to the ABL shear, whereas the transition from the low-turbulence case to the ABL case involves both the introduction of shear and a significant increase in the background turbulence intensity (TI from $\approx 1.2\%$ to $\approx 9\%$). It is indeed the combination of these two factors that drives the differences in wake modulation and energy distribution. We have revised the Conclusions (line 680).