General response to the Reviewers

Dear Reviewers,
We would like to sincerely thank you for your interesting observations that have made improvements in the paper possible. Based on your comments, we tried our best to improve the paper by clarifying some sections and adding new data and analyses. Modifications have been highlighted in blue-colored text both in the revised version of the paper and in the point-to-point response provided in this document.
We really hope that this revised version can be now worthy of publication in *Wind Energy Science*.

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Reviewer #2

Review of "How does turbulence affect wake development in floating wind turbines? A critical assessment". In this manuscript, the authors compare experimental results of wind turbine with platform motion to LES and URANS simulations. The numerical simulations are performed with and without inflow turbulence and the properties of the wake are analysed. The manuscript provides relevant results and unique conclusions on the topic of wakes of floating offshore wind turbines. The manuscript is well-written, however, the title does not fully represent the manuscript and there is lack of description of the methodology. Also, questions can be raised on the flow structures observed in the laminar simulations, as mentioned in the main comments below. Other comments are listed as specific comments. If all comments are addressed, I believe this manuscript will be a relevant contribution to the knowledge on wakes of moving turbines.
The authors would like to thank the Reviewer for the time he/she spent revising the study. The qualified observations and the proactive criticism prompted us to re-think part of the study and try to improve it to provide a more robust piece of research.

Main comments:
1. The title of the manuscript promises more than the manuscript deliver. The experiments are performed with only 1 turbulence intensity and the numerical simulations are performed with only 2 turbulence intensities. In my opinion, the manuscript does not fully answer the question proposed in the title, it is only a piece of a larger picture that is still being discovered by the academic community. I strongly suggest a modification of the title.

We do see your point and agree to change the title to more closely reflect the content of the study. The title has been changed into: "*How does turbulence affect wake development in floating wind turbines? Some insights from comparative LES simulations and wind tunnel experiments*".

2. It is not clear that the flow structures observed on the vortices between 2D and 3D are physical mechanisms, in the laminar case in Fig. 10 (a) and (c). These flow structures look similar to flow structures created by numerical effects. Therefore, the mechanism of vortex breakdown in the laminar case may be due to numerical effects. Please improve the spatial discretization in the wake and show that the results of Fig. 10 are not dependent on the grid.

The Reviewer's point is pertinent. In preparation for this study, where the huge calculation cost allowed for one complete run only, we did our best to assess the accuracy of the calculation settings. While the "RANS-style" mesh sensitivity is not adequate in the case of a LES calculation, we indeed tried a few meshes before selecting the present one, especially regarding the level of embedding (i.e., mesh refinement level) in the wake. The selected mesh was the finest tested, even though another one with one level of refinement less gave already consistent results in terms of flow structures seen in the wake and aggregate performance values. For the reasons explained above, we indeed preferred to stay on the safe side regarding the spatial discretization. Moreover, different from other approaches in the past that made use of progressive wake mesh coarsening, we decided to keep the same level of refinement in the wake from the rotor level up to 3D (see Figure B2), exactly to avoid possible spurious effect due to numerical reasons. In support of the selected settings, we have included in the revised version of the study the new Figure B3, where we report a contour of the Length Scale Resolution (LSR) parameter. This parameter, introduced by (Piscaglia et al., 2013) for this type of mesh topologies and solver, aims to quantify if the local filter size is small enough to solve the turbulent scales up to the viscosity range. Values up to 1 do ensure that all the turbulent scales up to the viscosity range are resolved; as per the recommendation of (Piscaglia et al., 2013), LSR values 3-5 can be considered as the limit within which the LES resolution can still be considered as acceptable. Upon examination of Figure B3, which reports a contour of LSR at the beginning of the last revolution simulated herein, in our LES simulation LSR values are consistently lower than one almost everywhere in the domain, including the critical areas of the near wake (within 3D from the rotor) and in the tip-vortex region. This corroborates the selection of conservative, while computationally-expensive, settings for this study.

3. In Appendix A, is the velocity sampled from an experiment with platform motion or without motion? If the velocity is sampled from a case with motion, then the simulations of the "fixed" turbine with turbulence will be contaminated

by the frequency of motion. If this is the case, this is an important limitation of the study and should be clearly discussed in the results.

The Reviewer's comment is surely correct. Fortunately, this is not the case of our study. The velocity was sampled within the tunnel without platform motion and even without turbine rotation. The spectrum was also compared to that measured before the tests without the experimental setup, obtaining total agreement. A discussion has been added to the paper. By double-checking the text, we indeed noted that it could be misleading and are sorry for this. The text below Figure A1 has been rephrased.

Specific comments:

1. Please define all abbreviations when they first appear. Many of the abbreviations are not defined, some examples being: ALM, URANS, BEM, DOF (list not extensive). AMR was only defined in the appendix.

The Reviewer is perfectly right. All acronyms have been defined at their first occurrence in the text.

2. In line 66-65. (Xu et al., 2024) is mentioned as a paper that suggested that the turbulence change some of the phenomena described so far in numerical studies. However, this work did not only suggest this, it studied this effect. Please rewrite it making clear the contribution of the cited article.

Done. We kept the text short for brevity, but indeed the explanation was incomplete.

3. In line 95. Apparently there is a missing reference ("[X]").

Thank you for noting. No reference was needed there.

4. In section 2, please add the Reynolds number and temperature of the experiment.

Added at the beginning of section 2.

5. In Table 2, please add the tip speed ratio and indicate the rate of pitching frequency to rotor frequency, which affect the growth of instabilities according to Kleine et al, 2022.

Added.

6. The solver is not mentioned in the section 3.1, only in section 3.2. Please include the name of the solver, references and a brief description in section 3.1.

Correct. We have moved the description of the software in 3.1.

7. Please reorganize section 3.1, dividing the section in smaller sections (or subsections), to make clear the parameters of each simulation and numerical method. Please provide a brief description of each numerical method and include references.

Section 3.1 was split into a section describing the CFD domain and boundary conditions, and one describing the grid sensitivity study.

8. In line 122. The term "nose cone" is more specific than "nose".

Thank you for pointing this out. Added.

9. The use of the term URANS-Hybrid is very confusing. I was only able to understand it after reading section 4.4, hence it should be better described in the methodology. More importantly, it is not a hybrid between URANS and LES, as it could be understood from context. It is a URANS simulation with a different strategy of imposing inflow turbulence, without using any of fundamentals of LES. I do not believe the name is adequate.

We agree with the Reviewer. Although we intended the term "hybrid" in the sense explained at the beginning of section 4.4, it can be confusing. We have thus renamed this approach "URANS-STG" as the inflow velocity fluctuations are generated using a synthetic turbulence generator. We have better described the approach in the introduction and also in section 3. The approach that is used to account for free-stream turbulence is also described in section 3.1 and briefly repeated in section 4.4.

10. Please define what is the AMR threshold and provide a reference for the AMR.

A more complete definition of the adopted threshold has been reported in the paper, and a reference is provided.

11. Please indicate the distances from the boundaries to the rotor.

The wind tunnel inlet is placed 8.8 rotor diameters upstream of the rotor. The domain extends up to 6 diameters downstream. These boundary placements were defined based on the wind tunnel test section, which extends a corresponding amount upstream and downstream the rotor. The lateral walls of the tunnel are placed 2.8 diameters away from the rotor center, again based on the physical dimensions of the wind tunnel.

12. In line 189. Please indicate the width of the Regularization Kernel clearly in the manuscript.

The width of the regularization kernel varies along the blade span. In particular, the local width of the kernel is chosen based on (Xie et al., 2021). In particular, the kernel size equals the local chord length, with a lower limit based on cell size: $\beta = \max(c/4, \zeta\Delta)$. In addition, since the grid size in the LES and URANS simulations is different, kernel size was fine-tuned to the specific approach, with the purpose of maintaining – in all the simulations - the same spatial force distribution throughout the computational domain. Therefore, $\Delta = 0.01$ m and $\zeta = 2.4$ in the LES simulations and $\Delta = 0.015625$ m and $\zeta = 1.6$ in the URANS simulations. This choice was adopted to ensure that the blade representation remains as consistent as possible in the compared simulation approaches. The reason for this is twofold, firstly the blade model was verified during the OC6 Phase III comparative campaign, with good alignment with respect to experimental results and other numerical models. Secondly maintaining a consistent blade model ensures that the compared wake solution methods can be compared. This discussion has been reflected in the paper in section 3.3.

13. In line 97. There is a mistake in equation 22 of (Dağ and Sørensen, 2020), as pointed out by (Meyer Forsting et al., 2019) and (Kleine et al., 2023). Please indicate clearly how the correction velocity was calculated. References: [1] Meyer Forsting, A. R., Pirrung, G. R., and Ramos-García, N. "A vortex-based tip/smearing correction for the actuator line." Wind Energy Science 4, no. 2 (2019): 369-383.; [2] Kleine, V. G., Hanifi, A., and Henningson, D.S. "Non-iterative vortex-based smearing correction for the actuator line method." Journal of Fluid Mechanics 961 (2023): A29.

The Reviewer's comment is on point, and the first draft of the paper was indeed not sufficiently clear on this aspect: our apologies. When we first implemented the tip correction for our ALM code, we did it for a simple wing, thus the additional induction term due to the wake structure (cited eq. 22 in the original paper) was not needed and only took the basic structure of the Dağ and Sørensen model. This is where the original test on the ALM was partially copy/pasted in the first draft. When introducing the model in Converge, we instead added the term, and we did it with the formulation of Meyer Forsting et al. (your suggested ref. [1]). The authors were aware of the work of Kleine et al., but no attempt to implement it has been made so far. The paper has been rephrased to clarify.

14. Section 3.3 is not clear. Please define mathematically the desired velocity perturbation, the exponential distribution function and the input velocity perturbations. Include more details on the generation zone.

The desired velocity perturbation $\Delta u = U - U_{x;\infty}$ is the difference between the desired velocity accounting for turbulent inflow fluctuations U and the undisturbed velocity $U_{x;\infty}$. Additional details on the generation fzone, exponential distribution function have been added to section 3.4 and to Appendix A. The input velocity perturbations are generated using the synthetic turbulence generator TurbSim with the velocity spectrum as input. Additional details can be found in section 3.4.

15. Please present the the measured properties/statistics that shows that the position of the generation zone of turbulence is adequate for both the LES and URANS ("hybrid") simulations.

Mean velocity and turbulence intensity ($TI = stg(U - \bar{U})/\bar{U}$) have been sampled from preliminary empty-box simulations (i.e. without the rotor), specifically designed to verify the insertion of turbulence perturbations. These results have been added to Appendix A, and referenced in section 3.4.

16. In line 220 and appendix B. Some distances are indicated in meters, without indicating non-dimensional distances.

Non-dimensional distances have been added.

17. In section 3.4 and figure 12. Please indicate the sampling window in number of cycles instead of seconds. Please also indicate the number of rotor revolutions.

Thank you for pointing this out. We have indicated sampling windows in seconds because the discussed windows apply both to simulations with and without pitching motion, where defining the sampling window based on pitching cycles is inconsistent. We have, however, added additional details to better clarify this point in section 3.5 of the revised manuscript.

18. Please define Δ in tables 5 and 6.

The tables and the table legends were amended to better clarify this.

19. In line 292. What do you mean by left part of the wake? Positive or negative y?

The text was unclear. Corrected.

20. In figure 9, reference to iso-surfaces.

The caption has been corrected.

21. In figures 9, 10, 15, 16, 19 and 20. Include the coordinates in every axis.

Coordinates were added in terms of distance from the rotor center, in rotor diameters.

22. In equation 3. Probably there is a typo. Is it non-dimensionalization by area? Which is the integrated area?

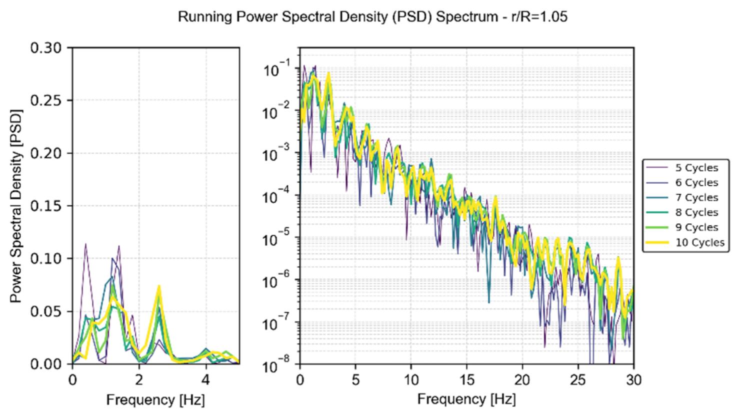
Thank you for noting the typo, the area has been included in the non-dimensionalization. The integrated area -more specifically, the rotor area - has been specified in the text.

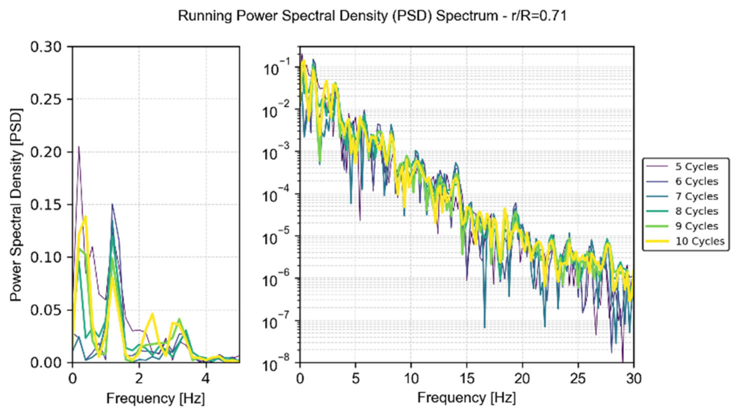
23. Please define the differential wake deficit mentioned in the caption of Fig. 12.
The Reviewer is indeed right, and this aspect was too vague in the first draft. For best clarity, both the definition of the wake deficit adopted in this section and the definition of the differential wake deficit have been included in the text.

24. Please indicate clearly the frequency of movement, the blade passing frequency and the frequency of the rotor.
Frequency of movement was added to figure 13. Rotor frequency was added to Table 2. No significant trace of the rotor frequency (4Hz) or the blade passing frequency (12 Hz) was noted in the wake, thus the higher frequencies are omitted from Fig. 13.

25. Please explain why the results from Fig. 13 are far from symmetric.
The Reviewer raises an interesting point. PSDs are indeed asymmetric even in the experimental data, which indicates the good ability of the simulations to reproduce the physical behavior of the test case. While a definitive explanation of the phenomenon has not been determined, hence the reason why this discussion was omitted from the first version of the manuscript, it is likely due to the combination of clockwise rotation with rotor pitching motion. In fact, rotor pitching motion introduces variations in angle of attack depending on the blade azimuth and rotor position during the pitching cycle. This ultimately results in different angles of attack variations on the left and right sides of the rotor, which may lead to the observed asymmetry. The discussion of Fig. 13 was improved to reflect this.

26. Show that the PSD converged. Convergence of the mean values (discussed in section 3.4) does not imply convergence of all statistics.
We agree with the Reviewer in the fact that convergence of means does not imply full statistical convergence. Mean values are shown in section 3.4 as these are used to evaluate wake recovery, which is an important point of discussion in floating wind turbine wakes, and one of those we have highlighted throughout the paper. Nevertheless, in preparation of the study, we made several checks on the convergence of our calculation. In particular, the PSD at 5D downstream the rotor is shown at r/R=0.71, where the mean values in Fig. 4 are slowest to converge and at r/R = 1.05 where the spectrum shown a peak in Fig. 13. As shown below, good convergence can be noted. To reflect this point, a comment was added to section 3.4, and the standard deviations were added to the mean values in Fig. 4. Good statistical convergence can be noted for this statistical moment as well.





27. Do the results from Fig. 13 change as the position move downstream? Can the same peaks in frequency be observed downstream?

The Reviewer highlights an interesting point. The spectra shown at 3D in figure 13 are shown at 5D in Figure 28. The spectra at 3D are also partially repeated in Fig. 27, which allows for a more convenient visual comparison with Figure 28. We decided to “condense” the results this way to keep the paper more synthetic. As discussed in section 4.4 as we move downstream, the peaks at 1 Hz can still be seen, but low-frequency peaks also start to appear. We attribute the latter to the breakdown of some of the coherent wake structures.

28. Please show the PSD at 5D.

PSDs at 5D are shown in Fig. 28. We preferred to include this here to avoid redundancy. A comment has been added to the text to reflect this in section 4.2.

29. In lines 395 to 397. The explanation for the presence of a peak in 1 Hz for the fixed turbine is not clear. Please provide a more detailed explanation.

The peak in the spectrum at one hertz is believed to originate from the peak in the inflow velocity spectrum at this frequency, a shown and discussed more clearly in the revised Appendix A. The discussion of Figure 13 was reworked to address the Reviewers’ comments.

30. In section 4.4. Please show figures similar to 19 and 20.

The Reviewer’s request is pertinent. Indeed, we decided not to include the pictures in the first draft only for brevity. However, to ensure completeness, snapshots with results from URANS_t, URANS_stg and LES_t have now been included.

31. In Figure 29(b) and page 27. The definition of ΔWD is not clear.

The Reviewer is right, and the definition was not completely clear. ΔWD is calculated as an average - over each point along the horizontal traverse - of the amplitudes of wake deficit variations. Additional details have been included in the paper for better clarity.

32. Figure A1. Caption is probably incorrect.

The Reviewer is right. Probably, there was a copy/paste issue. The caption has been fixed.

33. Appendix A. Where was the velocity sampled?

Details have been added as requested.