Review of manuscript: wes-2024-140
Title: Wake Development in Floating Wind Turbines: New Insights and Open Dataset from Wind Tunnel Experiments
Authors: Fontanella et al

Overall comments:

The submitted paper performs a unique and comprehensive set of experiments of a scaled wind turbine in kinematic motion to investigate both unsteady loads and wakes relevant to floating turbines. The objective to simultaneously study loads and wakes is critically important and valuable to the wind energy science community. Generally speaking, the paper is well written and the experiments are well described and thorough.

There are aspects of the paper that can be improved to increase the value and impact of the experimental results. I hope the authors may consider the following comments in a revision.

General comments:

1. The results would be much easier to follow and to compare with existing literature (previous lab experiments, field measurements, and models) if they were reported nondimensionally. Dimensional results are specific to the setup and make it hard to compare to other studies.
2. Relatedly, the authors can more clearly explain and justify the choice of nondimensional parameters that were investigated here. How were the amplitudes and frequencies (Strouhal numbers) selected?
3. Several conclusions are oversimplified, for example:
    a. The level of agreement with the linear regression in Figure 5 is overstated, which is important, because it affects the degree to which a linear model is justified (it seems not to be justified).
    b. More problematically, in the conclusions on Line 350, the authors state: “The mean velocity within the wake, regardless of the direction of movement, closely matches that of the bottom-fixed scenario.” This statement cannot be made, as it will depend on the nondimensional parameters. For some Strouhal numbers and amplitudes of motion, the wake will match bottom fixed, and for others not. The authors could rephrase as: “The mean velocity within the wake, regardless of the direction of movement, closely matches that of the bottom-fixed scenario **for the dynamic regimes we have investigated here**” or similar. Otherwise, the authors would need to investigate why their results differ from published studies including Messmer 2024 and others.
    c. Similar unclear points elsewhere in the study are identified below in point comments

Point comments:

1. The literature review is thorough and useful. It may be interesting to discuss the findings of recent articles on the subject of kinematic motion/unsteady inflow affects on power [1] and wakes [2,3], especially relevant to the studies of Messmer in 2024.

2. The motivation for the article on Line 47 is clear and concise. I would suggest also including research questions and hypotheses that you seek to address in this article.
3. Section 2.1: Presumably the previous studies referenced here included validation of the lab model against the DTU 10 MW reference, but that should be made more explicit here (and perhaps included in an Appendix) so that this article is self contained.
4. Section 2.1: The authors should report the nondimensional numbers that govern the dynamics explicitly for both the wind tunnel tests and also the true DTU 10 MW reference turbine. Specifically: Reynolds number, tip-speed ratio, and Mach number for 'fixed bottom' turbine operation.
5. Line 83: "Substantial agreement was found between the two measurements."
   This is a vague statement and the measurements are not shown. The measurements should be shown to confirm this or this statement should be removed
6. Line 85: It seems limiting to only have a single wire rather than a cross wire, especially if yaw misalignment and sway are investigated, where lateral velocities become non-negligible
7. Line 107: Relative to the literature review in the introduction, especially Messmer et al., 2024b, the experiments were selected to be performed in laminar inflow. More discussion of this choice would be appropriate, as it will reduce the relevance of the wake measurements to the true system.
8. Figure 2: There appears to be a consistent structure in the deviations from hub-height wind speed in figure 2(a). Do the authors have explanations for this? i.e. is it a boundary layer from the bottom surface? Interactions with the wall/flow speed up on the outside of the rotor. Boundary layer from the top surface?
9. Line 118: The experiments are conducted with a constant rotor speed. This is probably a good choice, because it eliminates controller feedback which would make the results more complicated. But I suggest discussing this choice and explaining it more.
10. Section 2.1: More discussion of the reduced frequencies investigated in this study compared to the full system is needed. Also, this is more commonly called the Strouhal number.
11. Line 138: Consider adding the angles introduced here to Figure 1
12. Line 148: "With surge, the blades velocity is equal to the platform velocity and the apparent wind experienced by the rotor is: …"
    The authors can more carefully define the 'apparent wind' terminology they are using because the velocity at the blades will be affected by induction. Similar comments throughout this section.
13. Section 5.1: Useful to report dimensional loads but it would be much more useful to also report the nondimensional results so it can be more directly compared to the full-scale system, other wind tunnels, and models. Likewise, plotting the results against reduced frequency would be better than dimensional frequency.
14. Line 217: "The differences in the mean value of Fx are likely due to the zero blade-pitch recalibration done during testing."
    Can the authors elaborate on what this means?
15. Figure 4: The experimental curves are remarkably smooth. Has any smoothing/postprocessing been applied to the plot?
16. Line 225: "This method of representation demonstrates that the loads change linearly with respect to the platform motion amplitude, as evidenced by the normalized points aligning with the

regression line."
I'm not sure I agree with this conclusion. It appears to me that the trend is not linear as evidenced by the lack of collapse at the higher frequencies.

17. Line 227: "Additionally, the loads exhibit a linear increase with frequency."
    Same comment as above, but now with respect to frequency.
18. Line 233: "This deviation, already seen by Bergua et al. (2023), is attributed to fluctuations in rotor speed and the flexible response of the tower, which could affect the wind turbine behavior under these testing conditions."
    Is there any evidence of this or is this speculation? Specifically, I thought the turbine was designed for rigidity as explained earlier. Are the authors sure there are not other physical mechanisms causing nonlinear response?
19. Line 238: Is there a reason the authors selected such a low yaw amplitude? Even fixed bottom turbines exhibit yaw variations of 10 degrees from turbulence.
20. Line 248: Similar to above, it's not clear to me that the thrust changes are linear.
21. Equation 8: Since the authors are using a single probe hot wire, this should be streamwise velocity rather than wind speed. Likewise for the streamwise standard deviation and streamwise TI.
22. Figure 7(c): Are these results phase averaged or instantaneous?
23. Line 275: "Among the conditions explored in this study, the strongest perturbation of the wake occurs when the reduced frequency of platform motion is 0.6."
    Is this shown somewhere? If not, please add a result that proves this or remove such statements.
24. Line 282: "The asymmetry might be due to flow in inhomogeneity in the wind tunnel seen in Fig. 2, potentially reducing the correlation between wake flow structures. This hypothesis can be verified through numerical simulations of the experiment, incorporating a high-fidelity model of the wind tunnel inflow."
    This is a pretty weak statement. Further investigation should be performed with the available measurements.
25. Line 350: "The mean velocity within the wake, regardless of the direction of movement, closely matches that of the bottom-fixed scenario."
    This statement cannot be made. This will depend on the parameters that govern the movement. One could say: for the parameters investigated, the mean velocity in the wake is similar to the fixed bottom scenario. But the statement as written is not correct.
26. Line 356: "In offshore environments turbulence intensity depends on the wave height and typically ranges from 3% to 6%."
    This is a statement which is far too over-simplified. Turbulence intensity in offshore environments does not 'depend' on the wave height only, but may partially depend on the wave height, or better yet, is 'correlated' with the wave height.

References

[1] Wei, Nathaniel J., and John O. Dabiri. "Power-generation enhancements and upstream flow properties of turbines in unsteady inflow conditions." Journal of Fluid Mechanics 966 (2023): A30.
[2] Wei, Nathaniel J., Adnan El Makdah, JiaCheng Hu, Frieder Kaiser, David E. Rival, and John O. Dabiri. "Wake dynamics of wind turbines in unsteady streamwise flow conditions." Journal of Fluid Mechanics 1000 (2024): A66.
[3] Li, Zhaobin, Guodan Dong, and Xiaolei Yang. "Onset of wake meandering for a floating offshore wind turbine under side-to-side motion." Journal of Fluid Mechanics 934 (2022): A29.