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Spatio-temporal ehavior of the far-wake of a wind Turbine model subjected to harmonic motions: Phase averaging applied to Stereo-PIV measurement.

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Summary:

The manuscript entitled "Spatio-temporal behavior of the far-wake of a wind Turbine model subjected to harmonic motions: Phase averaging applied to Stereo-PIV measurement" endeavors to describe periodic influences in the advection of the wake of a floating offshore wind turbine that arise from heave, surge, and pitch motions. The methods employed by the authors are well-founded and build on a rich history of wind tunnel research with a set of porous discs that are now familiar in the literature. While the authors austensibly focus on the phenomena of wake meandering, very little effort is made to connect the resultant phase-averaged wake trajectories to the underlying mechanisms driving wake meandering. The work would be made more impactful overall by connecting the results with model that is use widely in the wind energy engineering space, such as the dynamic wake meandering model or a wake-added turbulence model. As an alternative it would be nice to see the authors connect the observed wake behaviors, such as the large period of vertical wake meandering relative to the heave motion, with broader constraints such as exchanges between the ABL and offshore wind plants.

Comments:

- In the description of the experiment, I'm left wondering how representative the modeled boundary layer is to the real marine ABL that will be seen by operating FOWTs. The authors make a passing comparison to conditions described by ESDU (1985), but it's not clear how similar these conditions are to offshore development areas around the world. Readers of this research would be more able to integrate these findings into their own work if it were more clear what the target conditions are, what region they represent, etc. Please contextualize the boundary layer profiles and boundary conditions (roughness, shear exponent, etc.) with respect to actual obesrved quantities.
- The authors do not justify why the SPIV measurements focus on a single transverse plane 8.125D downstream of the modeled turbine. This location is relatively far in to the wake. At this distance, we expect the wake to break up in many cases, complicating the identification of closed velocity contours and regular periodic motion. We should also expect trajectories to depend on the downstream coordinate, such as a net vertical displacement of the wake, that cannot be described completely with measurements at a single locations.
- In Table 1. the motion of the full-scale turbine is decribed in terms of amplitude and meandering period. For the model-scale turbine, the motion is described in amplitude and frequency. Why present them differently? It is also not clear what the authors mean by "normalized amplitude."

Normalized by what? How representative are the Strouhal numbers of the modeled scale vs the full scale? I presume that the platform motion for the FOWT are driven at specific Strouhal numbers, rather than arising from hydrodynamic forcing, but this isn't explicitly stated in the paper.

- Line 167 — σ should have units of length.
- Equation 5 and throughout — multiplication is implied with a period, but should probably use the `\cdot` macro.
- Figure 6 — it would be far more interesting to plot the estimated wake center data, rather than the sinusoidal with noise. This would help the reader understand what the actual data look like, and how the period behavior is quantified.
- Figure 7 and throughout — some of the vertical axis labels are not rendered correctly and are missing subscripts.
- Figure 11 — It would be much easier to understand these results with error bars or uncertainty estimates in the trends. Also, the authors should comment on the complexity evident in the P0.28 case. Is there some non-linearity or more than a single frequency relevant to the wake center trajectory? As a more general question, how are the authors confident that a simple sinusoidal relationship is sufficient to capture the complexity of the modulation in the wake?
- Figure 12 — the phase-averaged surface metric for the heave case does not match conceptual diagram in Fig. 13. I would expect the surface of the wake to be approximately constant in time, since the authors suggest that the main change is periodic vertical displacement. At the very least, the results and discussion suggest that the wake surface for the heave case should change less than for the surge case, which should show period contraction and expansion.
- The only model mentioned in the manuscript is the Jiménez model from 2010, which described lateral or vertical wake deflection due to static yaw offsets. Without framing the results of this study in terms of a model or underlying physical relationship that can be used to explain the observations, this work will have very limited impact in the field of offshore wind energy.
- Line 435 — the authors state that "Heave motion translates the wake vertically with an amplitude higher than the motion itself." This observation likely arises from the fact that the wake is expanding as it evolves downstream and interacts with turbulence in the inflow boundary layer. If this observation is stating that there is some mechanism amplifying vertical wake motion, it could have pretty big implications for energy fluxes and exchanges between wind turbines and the ABL. Please elaborate.
- line 438 — The authors claim that, "Surge motion leads to contraction and expansion of the wake surface in the crosswise plane, with negligible wake displacement, ..." Is this insight supported by the results in figures 11 and 12? Is the wake center moving vertically or laterally for the surge case?