Review of manuscript wes-2025-106.R1, titled "Experimental investigation of the effects of floating wind turbine motion on a downstream turbine performance and loads", by A. Fontanella, S. Cioni, F. Papi, S. Muggiasca, A. Bianchini, and M. Belloli.

I thank the authors for mostly addressing my comments. However, I have further comments that should be addressed as well. The main comments are:

- I am not sure you can estimate the rotor equivalent velocity under wake conditions using the thrust curve estimated for a uniform freestream incoming wind velocity. The presence of the wake leads to radial shear over the rotor and higher turbulence intensity, which lead to completely different aerodynamic performance of a turbine rotor than for the case with a uniform freestream incoming wind field. This is also mentioned as well, for instance, in the IEC standards for the experimental characterization of power/thrust curves: "wind directions with a potential of wake interaction should be neglected for the characterization of the power curves". Even though measurements of the incoming wind are prohibitive, a direct measurement of the RPM of the rotor should be needed, in my opinion.
- The second point, which is a kind of puzzling, is the occurrence of speedups (power increase up to ~30% of the freestream unperturbed case) when the turbines are misaligned. The fact that the speedup increases moving the downstream turbine in the transverse direction from 0D, to 0.5D and 1D, and in the streamwise direction from 3D to 5D, in my opinion are all signs that the models within the wind tunnel cross-section create a large blockage factor and large confinement, especially in the vertical direction, leading to this speedup. If my discussion is not correct, then the authors should discuss this in the manuscript and provide experimental evidence.

Further comments are provided in the following.

Comments

1. Sect. 2.1 – Can you specify the distance of the rotor top tip from the ceiling of the test section and comment if you anticipate any effects on the generation and evolution of the tip vortices?
2. L 171 – 173 – I believe it is important to summarize this methodology here for the sake of completeness.
3. L 184 – Please add the resulting Reynolds number.
4. L 196 – You do not need a new paragraph for this sentence.
5. L 208 – Does this information on wind velocity contradict what already reported in L 184? Please clarify.
6. Line 210: At 2.4 m/s, tip-speed ratio is 7.78 (not 7.5) using the values mentioned in the study. It may be a good idea to represent the uncertainty or range bound on the tip-speed ratio and wind speed measurements.

7. L 214 – The estimate of the rotor effective velocity from the thrust coefficient sounds a bit unusual considering the typical accuracy in the thrust measurements and the presence of the incoming wake. Did you measure at least the rotational velocity, e.g., with a tachometer?

8. Table 2 – Is it possible to add other 2 columns with the thrust coefficients of the two turbines? This information is crucial for the reproducibility of the experiment.

9. Line 215: TI is higher in the wake and there is a radial (lateral) variation or gradient of the incoming velocity. As a result, the turbine in the wake experience higher inflow at rotor tip and lower inflow in the wake. However, the BEM model used for calibration uses freestream conditions. How do radial variations of wind speed and TI affect accuracy on the estimation of U_{RE} from the thrust measurements for the cases with an upstream wake?

10. Line 221: It is mentioned previously that the blockage (speed up) effects of WT1 makes the wind speed equivalent to 4.2 m/s. U_{RE} presented in the table 2 shows value up to 4.5 m/s. Is this the error from the calibration or the blockage of the second turbine? It needs to be justified.

11. L 231 – What do you mean for unidirectional sinusoidal motion? Please clarify.

12. L 310 – Can the authors comment on why they ignored completely in this work the discussions on the power performance of the turbine rotors, in terms of RPM and power coefficient? In literature, it has been shown that Cp, specifically, can be a more realistic parameter than Ct due to the non-similarity of many system components, e.g., tower and nacelle.

13. Line 345: The shear in the wake usually leads to higher flow in the outer region of the rotor (which is more sensitive to the overall operation of the wind farm). Hence, how a performance of a wind turbine for the wake and freestream (average of 2.4 m/s) cases can be compared?

14. L 355 – the title of this section is a bit ambiguous. What do you mean for energy recovery? Please consider providing a clearer title.

15. L 363 – I am not sure you described so far how you estimate power capture for this experiment. Please address this point or add required information.

16. Caption of Figure 5 – “maximum time-averaged…”, this seems a contradiction, maximum or average? Please clarify.

17. L 367 – 379. Can you summarize these results in a figure or a table for better interpretation of the results. You should provide these results for all the configurations investigated, not only for a subset.

18. L 367 – 377 – Can you assess/compare these values from previous works or predictions from engineering wake models?

19. L 378 – 380 – How do you justify this speedup leading to a 20% extra power at 3D and becomes even larger at 5D?

20. L 385 -387 – “power increase of 26%...24%”. I believe that presenting the results in this way is misleading. The percentage difference for the case 3D is large just because the initial value for fixed conditions is very low compared to the other cases. In my opinion, a more rigorous way to present these results is to calculate the respective power coefficients, which is a universal meaningful parameter. Then, the results should be presented as difference from the Cp of WT1, and for the dynamic cases as difference with the fixed case. At the end, it is obvious from the data

that the effects due to the WT1 motion are negligible with respect to the fixed wake interaction for all cases. Therefore, mentioning “26%” is only misleading.

21. Line 489: How do the thrust and torque amplitudes for the fixed case are extracted from the load spectra evaluated at a reduced frequency of 0.6?

22. Line 507: Usually, meandering is observed in the far wake. However, the downstream turbine is kept in the near wake. How the effect of meandering in the near wake is justified?

23. Line 758: Is the blade-element-model of the rotor is experimentally validated?