Review of manuscript wes-2025-106.R2, 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 making a significant effort in addressing my comments. However, I believe that in this work there are two main criticisms, one from the methodological standpoint and one about the experimental setup:

1) Estimating a reference rotor velocity by assuming that a rotor operating under waked conditions, namely in the presence of both vertical and lateral shear and turbulence different from that of the incoming wind, is conceptually incorrect. Therefore, settings of the rotor tip speed ratio and data analysis can be affected by an erroneous estimate of the rotor velocity.

2) I think the data show the occurrence of wind tunnel blockage due to the excessive confinement of the flow in the vertical direction, due to the presence of the two turbines and the wake of the upstream turbine.

Below, I add my replies in blue to the authors' rebuttal.

R1: 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. … Even though measurements of the incoming wind are prohibitive, a direct measurement of the RPM of the rotor should be needed, in my opinion.

A: … our intention in introducing the rotor-equivalent wind speed (U_{RE}) was not to recreate the real inflow conditions, nor to estimate a power/thrust curve in the IEC sense. Instead, U_{RE} is used to prescribe the operating point of the downstream rotor in a consistent manner across configurations. Specifically, U_{RE} represents the wind speed of a uniform inflow that would generate the same *mean thrust* as the waked inflow… This approach is consistent with common practice in wind-tunnel wake studies, where downstream turbines are operated at a prescribed TSR to isolate wake-induced aerodynamic effects without introducing closed-loop control dynamics.

Regarding the Reviewer's suggestion of a direct RPM measurement, in our experiment the RPM of WT2 is not the unknown to be inferred from inflow conditions, but rather a controlled input, prescribed in open loop to achieve the desired operating point. Since the generator torque is not actively regulated in real time, measuring the RPM would not provide additional knowledge of the inflow. The relevant aerodynamic quantity for our scope is the resulting thrust and torque, which we do measure directly…

RR1: **First, rotor-equivalent wind speed is a well-defined physical parameter in wind energy (Wagner, R., Antoniou, I., Pedersen, S.M., Courtney, M.S. and Jørgensen, H.E., 2009. The influence of the wind speed profile on wind turbine performance measurements. Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology, 12(4), pp.348-362. Wagner, R., Courtney, M., Gottschall, J. and Lindelöw-Marsden, P., 2011. Accounting for the speed shear in wind turbine power performance measurement. Wind Energy, 14(8), pp.993-1004. Wagner, R., Cañadillas, B., Clifton, A., Feeney, S., Nygaard, N., Poodt, M., St Martin, C., Tüxen, E. and Wagenaar, J.W., 2014, June. Rotor equivalent wind speed for power curve measurement–comparative exercise for IEA Wind Annex 32. In Journal of Physics: Conference Series (Vol. 524, No. 1, p. 012108). IOP Publishing.). To avoid confusion for the reader, I would consider assigning another name to the**

this parameter. For instance, as reported in the rebuttals by the authors, thrust-equivalent velocity.
The equivalent velocity is estimated by assuming that the thrust curve under wake conditions is the same as that under a freestream flow. Of course, this is not the case. Therefore, the authors are estimating velocity from a thrust curve that is not representative of the case under testing.
Therefore, I disagree with this statement: "The rotor-effective wind speed is not intended as a reconstruction of the waked heterogeneous inflow; instead, U_{RE}, denotes the speed of a uniform inflow that would generate the same mean thrust on the rotor …" This statement is formally incorrect because you are assuming that a waked rotor and a rotor under freestream conditions have the same thrust curve. If you could estimate the real rotor-equivalent velocity (e.g., with PIV for wind tunnel experiments), you would obtain a different thrust curve. In my opinion, you should perform your analysis only on the basis of the rotor RPM set and the measured thrust force. This approach is not only misleading but also incorrect, for instance, when you set the rotor TRS based on U_{RE} (see discussion in L 244 in the marked-up manuscript).

R1: 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.
A: We thank the Reviewer for raising this general point. In our experiments, the increase in WT2 power output is primarily due to its progressive avoidance of the WT1 wake, as the downstream turbine moves laterally, away from the wake core. At a 1R oNset the rotor is only partially immersed in the wake, and at 1D it is almost fully exposed to the freestream, which explains the observed power increase without invoking large tunnel blockage eNects. This mechanism is now better clarified in Sect. 3.1.2 through small additions to the text.
We acknowledge that some speed-up of the inflow also occurs due to wind-tunnel blockage. The flow accelerates from the 4 m/s freestream to approximately 4.2 m/s at the first rotor (+5%), and a further acceleration is expected in the section where WT2 is located. Although this second speed-up is not directly measured, the estimated increase of about 0.3 m/s based on $U_{\#\$}$ is consistent with the expected blockage trend. As discussed in the Discussion, blockage eNects influence the absolute power levels and prevent a direct one-to-one extrapolation to full scale. However, the dominant wake mechanisms and their trends remain representative, which is consistent with the broader literature on wind-tunnel wake studies. Ultimately, fullscale data will be required in the future to validate wake–interaction studies under realistic oNshore conditions.
RR1: L 244 in the marked-up document: "…in the 3D1D and 5D1D configurations. In these two cases, the URE for WT2 is higher than both the free-stream wind speed and the wind speed measured by the lateral Pitot system, which accounts for the blockage effect of WT1. The additional increase in velocity observed at WT2 is attributed to the local blockage caused by the WT2 rotor itself." A single rotor does not generate blockage. A single rotor has an induction, namely, a velocity reduction upstream of the rotor and a slight increase of the velocity in the near wake at lateral locations. A velocity increase at the rotor plane is a sign of wind tunnel blockage, thus due to the large blockage ratio for the experiment and the confinement of the ceiling of the test section.