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 in the revised version of the paper, while a point-to-point response is 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

1. **Please give a clear definition how "vortex core" is defined.**

Thank you for this rightful comment. The vortex core is defined as the inner part of a vortex, where the fluid rotates as a rigid body. This region is characterized by high vorticity values. The vortex core radius is defined in this work as the distance between the velocity maximum of the swirling velocity, following the methodology commonly used in the literature (see for example, van der Wall et al. DOI: 10.1007/s00348-006-0117-x). We have clarified in the manuscript the definition of the vortex core (lines 411-413).

2. **Lines 165: I do not fully understand of AOA, a etc. refers to the tip only or to a unspecified location on the blade. Please improve.**

The authors agree that this part of the manuscript could be improved. In the manuscript the circulation is assumed to be a function of the flow incidence in the last portion of the blade, which is in turn related to the angle of attack at the tip of the blade. The manuscript has been modified to clarify this point (line 171).

3. **Line 252: "blockage ratio". Is it an area-based number? Please clarify. If so, 8% is indeed a (too?) large value.**

The authors agree that the definition of the blockage ratio in the manuscript could be improved. The blockage ratio was calculated as the area ratio, $\beta = A_{rot}/A_{test\ section}$. The definition of the blockage ratio has been clarified in the manuscript (lines 272-274, and Eq. (16)).

We agree that the value of 8% is significant. For this reason the simulations that did not include the wind tunnel walls used a corrected inflow velocity equal to 4.19 m/s to account for the acceleration of the flow in the wind tunnel (lines 276-283).

4. **Line 391 (Figure 4) It is a pity that the \omega scales are not identical. Consider to make them equal (min = -100?).**

The authors agree that modifying the scales can improve the visualization of the results. Figure 4 was modified accordingly, by changing the scale to 0/-100.

5. **Line 458: (Figure 6 right) consider y-scale to start at 0.7 instead of 0. This would male the differences clearer. Why are there no result from SJTU?**

The authors agree that modifying the scale to start from 0.7 improves the clarity of the plot. Figure 6 has been modified accordingly. For the fixed-bottom case (LC1.1) no data from SJTU was available, hence it was not possible to include results in the corresponding figures.

6. **Also, I STRONGLY advice to add ERROR bars to ALL experimental values! This is very important for quantification of meaning of differences from the various simulations.**

The authors agree that adding error bars to the experimental data is valuable to the present work. For PIV data, error bars have been included for all the fixed-bottom cases, as the velocity fields were acquired for 100 rotor revolutions.

A comment has been added in the methodology about how the standard deviation of the experimental data was evaluated (lines 461-466). Hence, the standard deviation of all the investigated metrics was included in the plots. However, it was not possible to include error bars for the surge cases, as the velocity fields were acquired for a single cycle of surge motions. The authors agree that this is a limitation of the current study. For this reason, this point was emphasized in the manuscript to clarify that further experimental tests are required to validate the initial results shown in this work (lines 576-579).

Regarding hotwire data, the standard deviation of the streamwise velocity and the wake deficit was added in the figures. However, it was not possible to estimate the uncertainty of the amplitudes and phase-shifts due to the limited number of experimental data available. A comment has been added to the manuscript to underline this limitation for the reader (lines 488-491 and 900-901).

7. **Lines 280 ff (section 4). Description of CFD models should include mesh size und turbulence models used and if both were varied to estimate the effect on accuracy**

The authors agree that further details about the CFD models could improve the description of the methodologies employed by the participants. Further details have been included in the manuscript and summarized in Table 4. The participants tested different mesh-sizes in order to evaluate the effect of the grid sizing on the results and to guarantee accuracy. Instead, the effect of turbulence models on the results was not evaluated. A comment has been added to the manuscript to clarify this point (lines 350-351 and 344-345).

Table 1 Main simulation parameters for CFD simulations

Participant	POLIMI	SJTU	TUD	UNIFI
Simulation approach	ALM URANS	Blade resolved DES	ALM LES	ALM URANS
Turbulence model	k-ω SST	Spalart-Allmaras	Dynamic Smagorinsky	k-ε RNG
Rotor region [D]	0.26	0.11	2.5	0.22
Rotor region cell size	$1.7 \cdot 10^{-2} m$	$3 \cdot 10^{-3} m$	$1.3 - 2.4 \cdot 10^{-2} m$	$1.56 \cdot 10^{-2} m$
Near wake region [D]	0.63	6.26	N.A.	0.84
Near wake element size	$2 \cdot 10^{-2} m$	$1.2 \cdot 10^{-2} m$	N.A.	$3.13 \cdot 10^{-2} m$
Far wake element size	$4.5 \cdot 10^{-2} m$	$4.8 \cdot 10^{-2} m$	$5 \cdot 10^{-2}$	$6.25 \cdot 10^{-2} m$

8. **Line 502: Please explain in more detail HOW "the effect of blockage" was considered.**

The authors agree that the correction of blockage in the simulations could be improved in the manuscript. Additional comments have been added to the manuscript (lines 276-283). Since the wind tunnel blockage affects the results, the simulations run by POLIMI, TUD and UNIFI included the wind tunnel walls. The remaining participants, which could not include the walls in their simulations, corrected the free stream velocity in order to account for the flow acceleration in the wind tunnel. The corrected free stream velocity was calculated using the correction proposed by Glauert for moderate blockage ratios,

$$U'_\infty = U_\infty \left(1 + \frac{\beta C_t}{4\sqrt{1 - C_t}}\right)^{-1}$$

where β is the area-based blockage ratio, and U'_∞ and U_∞ are the corrected and actual free-stream velocities. The parameter C_t is the thrust coefficient, calculated as:

$$C_t = \frac{T}{0.5 \rho A_d U_\infty^2}$$

For the present case C_t is about 0.88, U_∞ is 4m/s and the air density is 1.177 kg/m^3 resulting in a corrected wind speed of 4.19 m/s.

9. **In summary, the authors should be more courageous and draw even stronger conclusions, if possible.**

From the analysis of the numerical and experimental results no clear trend has been identified that suggests a consistent limitation of one of the methodologies employed. Moreover, it has to be remembered that current data are unique but limited in terms of acquisition length. The numerical results show better agreement for the fixed-bottom case than in the unsteady cases, where significant differences arise among the participants especially at high frequencies of motion. This suggests that the currently available methods require further tuning in order to capture the wake behavior in these conditions as different methodologies could show significant discrepancies and lead to different conclusions concerning the wake response of a floating wind turbine. Further analysis and especially experimental tests are required to identify the sources of the observed limitations and to validate the currently available results. We have modified the conclusions to further clarify these points.

Minor

10. **I wonder if the "large" in the title (and elsewhere) isn't too un-determined.**

    The reviewer is right. Of course, it is difficult to indicate thresholds in these applications but the inclusion of "large" in the title is intended to emphasize that the investigated amplitudes of motion correspond to significant ones when translated to full scale. Indeed, for the surge motion, the maximum amplitude is 0.125m which corresponds to 9.375m at full scale and a total displacement in the wind direction of 18.75m. For the pitch motion an amplitude of 3° is considered corresponding to a maximum displacement of 6°.

11. **"OC6". I wonder, if IEAwind Task 30 should be added or a more detailed explanation of the abbreviation.**

The manuscript has been modified to explain the abbreviation and specify that the project was carried out under the IEA wind Task 30 (lines 115-116).

12. **Line 21: typo: Shangai -> Shanghai**

Thank you for your comment. We have corrected the manuscript (line314).

13. **Line 874 (and probably also elsewhere) change "good agreement" to "agreement of xx % or so. As I always say: "good" and "bad" are terms for fairy-tales not from science.**

Thank you for your comment. The manuscript has been corrected to provide a better quantification of the results, wherever possible.

Other Changes:

Due to a post-processing issue Figures 11, 12 and 13 have been modified, resulting in some small differences for some of the participants. The text has been modified to reflect these changes.