

The brief communication: “A novel wake mixing phenomenon and key parameters for wake recovery of floating wind turbines subjected to surge motions” by Schulz et al., describes the impact of motion conditions and turbine operating conditions on the wake deficit and coherent structures in the wake of a surging two-bladed wind turbine. The work is well structured and presented and definitely represents a valuable contribution to Wind Energy science, especially due to its relevant contribution in extending the current understanding of wake dynamics of floating wind turbines under imposed surge motion. After reviewing the manuscript, I believe that it can be accepted for publication after addressing some concerns from my side. In particular, while most of the comments only require minor editorial changes and further clarification (as explained in detail below), there is a more general comment which is relevant to the whole study and which should be addressed before publication.

General comment

If I am not mistaken, the authors are only showing in this manuscript results under laminar inflow conditions. From some sentences within the manuscript, it is hinted that the authors also performed some tests at higher turbulence levels, even though this is not explicitly stated (and again, no results in turbulent conditions are shown). Nevertheless, the authors make, I believe, some significant statements concerning the extension of the current results to turbulent inflow conditions. Given the current body of literature on floating wind turbine wakes, which has started to show how turbulence may have a significant impact on wake dynamics of floating wind turbines under imposed motion, it is difficult to accept such extension of the results without any data to support it. Additionally, I found some statements “too strong” or too concise, for example in Sect.2, where it is only stated that results can be generalized to turbulent conditions. Additionally, I would say that this statement about the extension of the results to turbulent inflows, contradicts the brief sentence in the conclusion, where it is stated that the impact of motion reduces with inflow turbulence and may disappear for turbulence levels above 5%. In conclusions, I believe that some evidence or reference about results under turbulent inflow could be provided (for example, was the same trend in wake deficit under different Strouhal numbers observed by the authors in some preliminary tests under turbulent inflow?) or this discussion should be further expanded upon, highlighting this possible limitation of the study.

Additional comments:

Line 24: The authors refer to motion-induced unsteady aerodynamics, which is a generic term probably referring to any of the 6DOF. Are the authors commenting on a generic motion (including other DOFs) or only to surge motion? In the latter case maybe this point should be clarified for the reader, as I am not sure if this outcome can be generalized. In general, the scope of this section should be clarified as it complicates also the interpretation of the rest of the section.

Line 25: The authors state that the unsteady impact on the loads can be determined as a function of the motion frequency or Strouhal number. Given the stated objective of the paper, which aims to clarify which parameters govern the aerodynamic response of floating wind turbines, I believe this is not sufficiently clear for the reader and possibly increases confusion. In fact, I wonder if the authors are underestimating the difference between the two parameters. In fact, if I am not mistaken, the aerodynamic response of the turbine could be a function of the Strouhal number but not of the motion frequency. For example, the aerodynamic response of a floating wind turbine could be the same when the frequency of motion and freestream velocity are modified so to keep the Strouhal number constant but be different if the frequency of motion is changed while keeping constant the inflow velocity. The same concept applies the other way around, the aerodynamic response of the turbine might be a function of the frequency of motion but not of the

Strouhal number. The authors should clarify what they mean with this sentence and what is the governing parameter, if one has already been identified in the literature.

Line 26: Would this scaling not apply for example to pitch or yaw motion? In the title the authors clarify that the work concerns the analysis of surge motion, however this limitation is not clarified in the introduction.

Line 30: The list of references is definitely not representative of the full body of literature on floating wind turbines wakes. I am not sure if the authors are analysing only a subset of studies and if so, how are they selected. A small comment should be added at least clarifying that this represent only a subset of current literature.

Line 33: In general, if I am not mistaken, the increase in wake deficit is only visible after a certain distance from the rotor and is not immediately noticeable in the near wake. The authors should clarify where this improvement in wake deficit are noticed, in the mentioned conditions.

Line 45: In the introduction, no discussion is carried out about the impact of the amplitude of motion. Do the authors believe that this has no effect on wake recovery, even for very small amplitudes? In general, I would expect TLP platforms to possibly be characterized by much smaller amplitudes, which could mean that the impact on the wake deficit as a function of reduced frequency might be substantially different than in the case of other more common substructures.

Line 57: I believe there is a typo and “about” can be removed.

Line 63: The TUHH model is characterized by a two-bladed configuration. Could this impact the outcomes of this study in comparison to a tree-bladed rotor, which represents the most common configuration?

Line 63: What is the blockage of the experimental setup? Could this affect the wake response of the turbine?

Line 67: I may be mistaken, but it feels like this statement about laminar results possibly being generalized to turbulent conditions directly contradicts the introduction and the findings of some of the same authors in previous work concerning wake dynamics of floating wind turbines under imposed motion and increased inflow turbulence. Actually, the choice of laminar input, while understandable in the framework of this work, still represents a significant limitation of this work. I may be mistaken, but I believe that higher turbulence not only could reduce the overall impact of motion on the wake but could also alter the wake response to changes in reduced frequency and motion characteristics more in general. The authors should clarify this point.

Line 99: I am not sure that the wake deficit induced by the nacelle falls at the center of the wake for all operational cases. Additionally, do the authors have any explanation for why the near wake at $x=1D$ is not symmetric?

Line 100: Previous studies have shown that the position of the transition point for the wake is usually affected by upstream turbulence. In particular, no significant differences were observed in the transition position with higher turbulence intensity when motion of the turbine is introduced. This represents one of the conclusions of this work which, in my opinion, cannot be easily generalized to a turbulent inflow case.

Line 103: the authors state that the wake “returns to fixed-bottom performance” for $St= 5.75$. However, there are significant differences between the fixed-bottom and motion case in terms of

velocity profile. I believe this sentence is due to the velocity profile returning (slightly) to a top-hat shape; however the difference from fixed-bottom case should still be clarified.

Line 105: “after Eq (1)”. Please rephrase.

Figure 1, b.1 and b.2 the authors should consider changing the colours of the lines to have a match between the two figures at a Strouhal number of 0.6. In particular the similarity between colours for $St=0.3$ on the left side and 0.6 on the right side can cause a lot of confusion at first glance.

Line 114 and Line Line 117. I believe that the manuscript may overstate the impact of the induced velocity on the wake deficit. In fact, it is true that the current results confirm the previous study by Garcia et al., where increases in recovery were observed, but in that work, such differences were quite small (ignoring for a moment the limitations of the numerical methodology employed in that work when assessing the wake response up to 20D downstream).

Regardless, also in the current study the impact of the induced velocity on the wake deficit is limited at high Strouhal numbers, even though larger differences are observed for lower frequencies. For this reason, I wonder if this trend should be highlighted in the discussion of the results.

Figure 2b. If possible I would recommend including the fixed-bottom wake deficit as a horizontal line to highlight any increases in wake recovery.

Line 132: I wonder if there were some issues in the final draft of the manuscript concerning the writing of this sentence “[...] whereas the wake in the fixed case ($St_p = 0$) exhibits broadband energy distribution over $f D/U_\infty \in [0.2, 0.6]$, characteristic of natural wake meandering (Figure 2b)”. In fact, as shown in the figure and as I would expect, the wake oscillations in the shear layer should have a broadband spectrum in the fixed-bottom case and a significant response at the frequency of motion for the motion cases. Please clarify or correct this section of the manuscript.

Line 132: If I am not mistaken, the fact that the velocity spectrum is obtained in the shear layer of the wake is reported only in the caption of Figure 2. I think it might improve clarity for the reader if this detail is also reported in the main text.

Line 174: The authors state that it is interesting how surge motion and rotor rotational frequencies interact at large distances from the rotor. While I agree that this outcome may be in some way unexpected and definitely interesting (especially in the comparison drawn to the helix method), I believe that the current results are strongly affected by the use of a laminar inflow during the tests. In fact, under laminar inflow, the vortex structures in the wake are much stronger and more persistent (as the turbulent fluctuations alter the stable location of the tip vortices, promoting vortex interaction and collapse, hence aiding mixing), and periodic harmonic perturbations as induced by a sinusoidal surge motion may have very interesting effects on wake recovery. For this reason, it could be possible that these coherent structures and wake dynamics are practically not observable under realistic inflow conditions (and probably already for low turbulence levels).

Line 232: The authors state that the trends deduced by this study are persistent over increased turbulence levels up to 5%. However, unless I have not missed something substantial in the manuscript, no experimental data is provided in the manuscript to support this statement. As explained in the general comment, the authors should further expand the discussion on this point.