Text in black: Reviewer comments

Text in gray: reply

Reviewer 1

- Essentially, for a floating wind turbine, the wave-frequency induced motions impose additional effects on the wake generation. one may check the rotor plane fore-aft and side-to-side motion effect in separate analyses. that means it would be good to look at 0 and 90 wind-wave misalignment.

  That would be an interesting analysis that would be useful to further verify FAST.Farm for floating applications. In this case, however, we are running cases for comparison to Johlas et al and are limited to cases that they ran. With the 30-degree wind-wave misalignment included, here, we feel this is a reasonable compromise between the extremities you propose.

- There is a significant difference in the wind speed STDs from the two models. however, if it is only related to the low-frequency variation (low as compared to the rotor rotation frequency), from the wake and motion analysis of the floating wind turbine, one might just compare the rotor-averaged or the height-averaged STDs.

  Thank you for the suggestion of comparing the rotor-averaged std, we added that in the text. And indeed, the difference in std is a consequence of using TurbSim instead of the same inflow as Johlas et al., and the figure showing the vertical dependence of u (mean, std, and TI) is useful to evidence that. We realized after the fact that we could have generated TurbSim output with a std that more closely matched Johlas et al, but decided that what we had was close enough and it would not change the key conclusions, so, not worth changing.

- good to present the spectral comparison as well, if available.

  This would indeed be an interesting comparison, but unfortunately, we do not have the frequency (or time) response obtained by Johlas et al.

- it might be good to use some kind of period ratio to address this issue, for example the main wave oscillating period of the floater and the rotor rotation period. or one may check the velocity ratio in wind direction, for example between the wave induced rotor velocity and the wind speed.

  Interesting suggestion. In the present case, the problem was related to the displacement of the floater, and not much to the velocity. We had to use a value below the natural frequency of surge, which is a frequency induced by the mooring system. We could say, then, that $f_c/f_{n,surge}$ should be close to 1.

- could you please explain why the heave resonant motions could contribute to the fore-aft tower-base moment for the rated conditions?

Sure. For the severe sea state, the wave energy spectrum encompasses the heave natural frequency, leading to relatively large heave motions. The vertical displacement changes the vertical rotor position within the wind shear profile, causing a variation in thrust. The vertical velocity changes the relative wind velocity at the blades (both magnitude and direction), thus also inducing a variation in thrust. And because the center of gravity of the tower and RNA are not aligned with the tower axis (due to the CG of the RNA and due to mean platform pitch), motions along the vertical direction introduce an inertial bending moment at the tower base. We added a sentence in the text to summarize these effects.

These effects are smaller than other effects such as turbulent wind fluctuation or pitch motion, but they are large enough to appear in the PSD plot for the case shown in yellow because of the large heave motion at the resonance frequency of heave.

Reviewer 2

General Comments/Summary

This manuscript provides a comprehensive analysis for studying wake effects for floating wind turbines in FAST.Farm. The authors begin their study by simulating a single turbine case validating FAST.Farm with an LES Study (Johlas et al. 2020). Importantly, they find merit in using the newly implemented curled wake model as it outperforms the standard polar wake model, especially when it comes to the vertical wake deflection. Secondly, the authors analyze power, tower base loads, and blade loading for a three turbine array in a comprehensive range of environmental conditions. Additionally, they compare their results with fixed turbines as a reference, which provides a helpful baseline. They find that their results largely depend on a balance between the mean pitch angle, the floater motions, and the vertical wake deflection, which depend on the environmental conditions. Their structural analysis is comprehensive by analyzing damage equivalent loading and power spectral densities. Additionally, the authors do a good job of stating the limitations of their work/model, which is appreciated. Overall, this is great work that can be improved with a few minor revisions as suggested below.

Specific suggestions and questions

- Starting in line 13: I do not think the word compliance is being used correctly throughout the manuscript and I have not seen it used in this manner in the literature. Typically, compliance is used with respect to a standard or specific criteria, which is not the case in this study. I believe the authors can simply reference to the "impact of the floating substructure", "impact of a floating turbine compared to fixed" or the "impact of the motions of the floating substructure". Please double-check the use of the word "compliance" throughout the manuscript.

Thank you, we rephrased the text in the parts where "compliance" was used.

- Line 15: The power and loading of the array are indeed affected by the waves, change in tower frequency, and vertical deflection of the wake. It may seem obvious, but the velocity deficit itself because of the wake has a primary effect that should be stated where appropriate. While the vertical wake deflection is important, ultimately the magnitude of how much the pitch changes for downwind turbines depends on the strength of the wake or velocity deficit (which your results show).

Indeed, when we say wake deflection we are referring to the vertical deflection of the wake deficit, which changes the velocity deficit at the downwind rotor. We rephrased the first two occurrences of "vertical wake deflection to "vertical deflection of the wake deficit" to clarify.

- Section 3.2 and 3.3: Why is C_meander changed for the Curled wake model? Please state if this is to be consistent with the polar model. From Fig. 7, it seems that the C_meander has a negligible effect on the wake dynamics for the curled wake model. Additionally, in line 328, please explicitly state that swirl is used (I'm assuming).

You are correct, we used this value of C_meander for consistency with the polar model. We added a sentence in section 3.3 to clarify that the same results can be obtained with the default value C_meander=1.9. We also specified that swirl is used.

- Line 334 and Fig. 9: This figure is only briefly discussed; however, I believe it is important to your work. I suggest moving the sentence beginning on line 334 and starting a new paragraph at the end of Section 3.2. I recommend including a sentence or two describing the “good” agreement. For example, you could mention the transition from the bimodal distribution in the near wake to a more gaussian distribution further downwind is well-captured and so on.

Thank you, we added a sentence following your suggestion.

- Line 387 and throughout: Please double-check your use of the word “oscillation”, which is used a lot. Oscillation describes repetitive motion usually at a specific frequency. Other words that might be a better choice are: variability, fluctuation, energy, motion (depending on the sentence).

Good suggestion, we replaced the use of the word “oscillation” throughout the text.

- Line 417: The fact that the tower-base DELs increase for fixed and not floating is quite striking. I would at least say here that it has to do with the difference in the tower frequencies and then state that you show/expand on this later. It takes a while to get to when this is finally stated.

Thank you, we added a sentence about that following your suggestion.

- Section 4.3: I would move this subsection to the appendix. The discussion on blade loading is much smaller compared to the tower-base loading. There is really no difference in the results here, but it is nice to see that. In line 502, what are the cases where there are large relative differences in the standard deviation? I don’t see this in Fig. 20. I also think line 508 does not need to be a separate paragraph.

This indeed looks better. We moved it to the appendix and removed the part about the “large relative differences” which was not true – we do see some relative differences but saying that they are large was quite a stretch.

Minor suggestions

- Line 11: please state the separation distance for the three-unit array in the abstract.

Done

- Line 15: remove “in a nutshell”

Done

- Line 20: I think a more upbeat word than interesting could get readers excited about this work

Changed to “promising”

- Line 24: how about Hywind Tampen?

We added Hywind Tampen to the list

- Line 49: remove “a”

Done

- Line 71: What are the limitations of previous implementations? Please state one or two.

Added two examples and a reference to a paper with a more complete list.

- Line 134: I don’t think you can technically say “floating boundary condition”. It’s a fixed boundary condition attached to a floating substructure. Suggest rewording

Indeed, “floating boundary condition” is not a technically correct term. We rephrased to say that the tower is clamped to a floating substructure.

- Line 148: state what the z0 used was

We included the value ($z_0 = 5.62 \cdot 10^{-5}$)

- Line 214: a little odd to have a single sentence paragraph. I think you can move this sentence to the end of the first paragraph in this subsection.

Indeed, we fixed that.

- Line 277: need a period after Fig

Done

- Line 281: no comma needed after velocity

We removed that comma

- Line 301: parentheses for citation are incorrect

We removed the parentheses

- Line 337: remove “in a nutshell”

Done

- Line 336: please state the cases

As that would be about 25 different cases, it is not very practical to state them in the text. There is a horizontal line marking y=0 in the plot, so all the cases above that line correspond to the floating turbine yielding more power than the fixed turbine.

- Line 455: A rough estimate of the “wide” range in Hz would be helpful here

We added a statement saying that it is “about 0.10 Hz slightly depending on the sea condition”

- Line 461: remove besides

Done

- Line 472: what is “this” frequency range? Please state it

We clarified that it is the frequency range around the natural frequency of platform pitch

- Line 556: add the hyphen between above rated. Double check that you are consistent for below-rated, at-rated, and above-rated.

The current version of the paper was revised by a professional editor, so the problems related to that have been fixed.

Other changes

- Edited Fig. 19 to indicate the first natural frequency of the tower
- Added Fig. 20 to make it easier to see what is happening