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Wind Energy Science Discussion

Date: April 2, 2024
Subject: WES-2023-137 Response to Reviewers

Dear Referees,

We want to thank you for taking the time to review our article, going through it in detail, and providing very helpful comments. We sincerely believe that all your comments have helped us to present our research in its best form and make the article more effective.

We tried to improve article according to your suggestions and we hope to have addressed most of your concerns. Below, you can find our response (AC) to your comments (RC).

On behalf of all Authors,
yours sincerely,

Alessandro Fontanella

Attached documents:

- Response to Anonymous Referee #1
- Response to Anonymous Referee #3

Response to Anonymous Referee #1

Dear Referee,

Thank you for taking the time to review our manuscript a second time and for the valuable comments you made. We appreciated your suggestions regarding the introduction because we think they helped us highlight the main point of the article, and on how to improve the connection between Sect. 3.5 and the rest of the results.

Below you have the point-by-point reply to all your comments.

RC1.1	The introduction section needs to be more clear about the goal and novelty of the paper. I believe the novelty of the paper comes from answering the question "why do FOWTs produce less energy although equation 4 is correct?"
AC1.1	Thank you for this comment, we agree with you it is better to highlight this contribution over the others. We reworked the introduction accordingly.

RC1.2	This should be more emphasized in the result section especially in section 3.5. The results in this section can be connected to the results in Figure 11 by showing the FOWT's motion in the frequency domain. This will prove that the energy gain does not occur because the higher frequency where the energy gain is expected to increase. Currently the section lacks a clear connection with the rest of the results.
AC1.2	We agree that showing motion spectra is a good way to draw connections between results of simulations with prescribed motions and those with stochastic wind and waves. We followed you suggestion and we added the natural frequencies of the platform surge and pitch modes to Figure 11, commenting that meaningful power gains occur at higher frequencies. Moreover, we added one figure in Sect. 3.5 showing the spectra of platform surge and pitch motions obtained in simulations with stochastic wind and waves and we commented the figure saying that the amplitudes of motion are not sufficiently large to produce significant increments of power as demonstrated in the prescribed motion simulations.

RC1.3	Line 44: I still do not clearly understand what the authors mean by a methodology in point 3.
AC1.3	We agree it is not clear and it is not the main contribution of our work. We deleted the sentence in the reworking of the introduction.

RC1.4	Line 49: I do not understand how the work done in this paper can help with grid management.
AC1.4	Since we are not experts of grid management, we recognize this sentence can be speculative and we preferred to remove it.

RC1.5	Table 1: There is typo in the row of Coupled-S.
AC1.5	We found a few typos in Table 1 and we fixed them.

RC1.6	Figure 11: Would be informative to add the pitch and surge natural frequencies to the figure. Or mention it in the description.
AC1.6	We added the surge and pitch natural frequencies to the figure. We also added in Sect. 2.2 one table to summarize the natural frequencies of the platform modes of the four floating wind turbines.

Response to Anonymous Referee #3

Dear Referee,

Thank you for stepping in in the review process of the article. We saw that you read the entire article in detail by providing pointed comments on how to improve its clarity. We are grateful for the time you took to do this.

We saw that you made several comments on the analytical model of Eq. 2-4 and on the simulations with prescribed tower-base motion. We think your comments have helped us clarify the assumptions of these models and how to interpret the results obtained with them. We believe this modelling approach that greatly simplify the platform motion and neglects the response to wave loads is important to understand the aerodynamic response of a floating wind turbine rotor. Prescribed tower-base motion has been considered in recent studies about the performance of floating wind turbines and in the validation of aerodynamic models used in simulation tools for offshore wind turbines.
We agree with the reviewer that prescribed motion in one direction is not realistic. Indeed, we considered more complex motion conditions closer to those faced in normal operation of floating wind turbines in the simulations with regular waves and with stochastic wind and waves.

We truly appreciated your comments because we think they enriched our work and below you have the point-by-point reply to all of them.

<u>Abstract</u>

RC3.1	make sure that the objectives and main findings are clear and concisely presented.
AC3.1	We revised the introduction according to the latest comments of Referee #1 to highlight the main contribution of this work, that is we show the power of a floating wind turbine rotor could in theory increase when due to the rigid-body motions of the structure, however this does not occur in normal operating conditions. We modified the abstract to be aligned to the revised introduction.

RC3.2	"The current technology of spar and....is not suitable to exploit energy of waves..." should be reformulated to avoid misunderstanding related to the concept of integrated (or hybrid) single platform for the energy harnessing of wind and waves using wind turbines and wave energy conversion devices.
AC3.2	Thank you for reminding us of hybrid floating systems. We revised the abstract to make clear that we refer to the rotor power. This is stated in the first sentence when we say that waves can increase the power output forcing the rotor to move against wind and later when we say that large along-wind motions are needed to increase the rotor power.

<u>Introduction</u>

RC3.3	"Results and the methodology of this work can be leveraged in the early phase of floating wind projects to quantify their energy production and reduce the risk

	of investment”. This statement is not clear. Could you please reformulate it? Will your methodology and results leverage or they will be leveraged?
AC3.3	We hope results will be leveraged by others. We rephrased the sentence to make this clear.

Methodology

RC3.4	Eq. 2. Are there any references for this formulation? This expression and the statements above and below referring to it should be carefully revised. If you are interested in the longitudinal motions at the hub level, both surge AND pitch motions of the platform should be considered, not only either surge OR pitch. So, the equations (2), (3) and (4) should be revised. Moreover, the effect of the phases (relative to the wave) of the surge and pitch motions are crucial and MUST be considered for a correct assessment of the motions at the hub level. An important parameter is also the reference frame system in which surge and pitch motions are defined, as well as the coordinates of the hub location. You also need to provide the expression for the wave elevation, which should be used as reference for the platform's motion phases. Eventually, if the hub has an offset relative to the platform's (or tower's) centre, platform's yaw motion also contribute to the longitudinal motions at hub level)
AC3.4	This formulation is often used in recent studies about the aerodynamic response of floating wind turbines, and we added two references to support the equations. Moreover, we revised the text around the equations to clarify their assumptions (the main one is: the rotor is a point in correspondence of the wind turbine hub) Now we talk about hub motion in the along wind direction which is more appropriate than referring to surge and pitch motions for the reasons mentioned by the Referee. As the Referee has noticed, the model of Eq. 2-4 makes use of strong simplifications about the kinematics of a floating wind turbine rotor. The combined surge, pitch and yaw motions, the effect of their phases relative to the wave, and the nacelle geometry are introduced in the analysis with simulations using prescribed motions and regular waves.

RC3.5	“where hh is the hub distance from the tower base”. It seems that for the amplitudes of the motions at the hub level you are considering that surge or pitch occur isolated one from another. In actual conditions, they can occur simultaneously, i.e., they can couple and depending on their respective (wave-induced) phases they can display a constructive or destructive interaction. How/Where are you representing the interaction between surge and pitch? How are you defining the wave elevation that is inducing the surge or pitch motions? How are these motions defined relative to the wave? How are you defining phase lags?
AC3.5	Thank you for this comment. As we said in AC3.4, this comment and the one of RC3.4 helped us clarifying the assumptions of the analytical model. We included, using his words, the comment made by Referee about the coupled surge and pitch motions at the end of Sect. 2 (before Sect. 2.1) where we discuss the limitations of the analytical model, and we start introducing more advanced models.

RC3.6	"surge motion". pitch motion should be considered along with surge motion.
AC3.6	This is true and we now we use harmonic motion (of the hub) instead of making reference to surge and pitch motions that are not appropriate.

RC3.7	"The four models gradually add complexity to the simple analytical model of Eq. 3". The names/abbreviations provided for each of the four models are not standardized, so could you please, provided their names in full extension and relevant references for each of them.
AC3.7	Thank you for this comment, it helped us strengthen Sect. 2.1. In the revised text, we provide names of the simulation model in full extension, and we added relevant references for every modeling approach.

RC3.8	"The rotor aerodynamic response is calculated with a non-linear engineering model". Could you provide references for the aerodynamic model?
AC3.8	We revised the text and we explicitly say that aerodynamic calculations are based on the blade element momentum model.

RC3.9	"platform motion of…directions" Have these motions being applied separately or coupled? How are these motions defined? Amplitudes, frequencies, phases?
AC3.9	We revised the text and we say that motions are applied separately. In Sect. 2.1.1 we added comments on how values of amplitude, frequency and phase were selected.

RC3.10	"hydrodynamic loading and the floater dynamic response to waves". What kind of approach has been used for the hydrodynamic model (potential theory, linear theory, RANS, etc.)?
AC3.10	Part of the sentence has been removed and we leaved "the floater dynamic response to waves".

RC3.11	Table 1. The table just refers to the types of wind and wave used in the simulations. For the better readers' understanding it would better if you actually describe the approaches of modelling adopted in the simulations of: aerodynamics, hydrodynamics, elasticity, etc.
AC3.11	The table has been requested by Referee #2. The modeling approaches for aerodynamics, hydrodynamics and elasticity are summarized in the paragraph starting at line 119. We changed the caption of Table 1 to make it adherent to its content and now it says the table summarized the main assumptions of the four modeling approaches about wind, waves, and floating platform.

RC3.12	"strip-theory solution; the hydrodynamic coefficients required for the potential-flow solution are obtained with a panel code" Potential flow theory is used together with panel code solutions (such in Wamit) to compute radiation/diffraction effects, but could you please where did you combine strip-theory solution with potential-flow theory?

AC3.12	We rephrased the sentence saying we use "a combination of potential-flow theory, to compute radiation/diffraction effects, and strip-theory, to model viscous drag".

RC3.13	"1.25° in case of rotations". How this value was obtained? Is it a realistic value for actual operational conditions of FOWTs? How the motions are coupled?
AC3.13	We added comments to values of amplitude, frequencies, phase and coupling between motions.

RC3.14	Could you please clarify whether or not the thrust curves shown in Figure 3 include peak shaving. If so, could you please also include the "unshaved" curves for the sake of comparison?
AC3.14	At line 202 (of the latest version of the manuscript) we say that we use the peak shaving functionality and at line 218 we say that curves in Fig. 3 are for the turbine regulated with the control strategies described in the text above (i.e., with peak shaving). We modified Fig. 3 to include curves without peak shaving for the sake of comparison.

RC3.15	line 293, of –> for
AC3.15	We agree that for is more appropriate than of.

Results

RC3.16	"Figure 7 shows how the power curve". How these power curves have been obtained? Do they come from time-domain simulations after averaging the instantaneous power for each wind speed? Please describe/clarify. What was the simulated time? Could you show some typical time series? Have you used OpenFAST for these simulations? If so, compared to the reference case files, what changes in the input files were required?
AC3.16	Thank you for this suggestion. We clarified how we obtained the power curves in OpenFAST. We averaged results after power reaches steady state, so we think there is no value in showing almost constant time series.

RC3.17	"the consequent maximum reduction of generated power is up to 3%". This 3% of maximum reduction of the generated power refers to (is compared to) which reference case? Do these results refers to all four cases with or without shaving? Please clarify.
AC3.17	We added the maximum reduction for the four floating wind turbines and we specified that this reduction is compared to the bottom-fixed case with peak shaving.

RC3.18	In the previous paragraph it is stated that the reduction is up to 8.9% (5 MW) and 11% (11 MW). Do they only refer to the shaving effect? Please clarify.
AC3.18	Thank you for this comment, we agree it wasn't clear in the previous version. Now it is clarified.

RC3.19	"Sensitivity to". Effect of?
AC3.19	We agree "effect of" is better.

RC3.20	"the power curve with steady wind and no waves". What was the procedure used to obtain the power curves: equations, simulations (OpenFAST?)? What was the duration of the simulations?
AC3.20	Thank you for this comment, we agree it helps clarifying our work and how we obtained the results we show. We explain that power curves are obtained with the same procedure used for steady unsheared wind cases (previous section), but changing the wind from spatially uniform to vertically sheared.

RC3.21	"we estimate that uncertainty in wind shear and turbulence intensity". Could you cite some references to support these statements?
AC3.21	We cited one reference that obtained similar results from field data.

RC3.22	to begin the discussion … Fig 10". Eqs. (3) and (4) are questionable so far... Have you used OpenFAST for these simulations? What changes were necessary in the input files compared to the reference ones (examples in NREL repository)?
AC3.22	We hope we have clarified the assumptions and validity of Eq. 3-4. We used the modeling approach named Harm-M, which is based on OpenFAST, and this is clarified in the revised text.

RC3.23	prescribed surge motion of 1.5 m amplitude and 0.2 Hz frequency." The prescribed frequency corresponds to a wave excitation of 5 s, which can be considered a wave-frequency (first-order) motion. Since FOWTs are moored, their predominant motions are expected to be in the low-frequency range (second-order?). So could you please justify why these simulation conditions are of practical interest?
AC3.23	Thank you for this comment. We appreciated it and we added your words about the frequency of motion saying that it can be considered the linear response to a 5s wave. The results of this section are obtained with the Harm-M model. As explained, in the methodology, this model is the first step towards the simulation of the floating wind turbine response in realistic operating conditions. This is also explained at the end of the section when we say "However, with Harm-W, Coupled-S and Coupled-C simulations, which gradually introduce complexity and realism into the modeling of floating wind turbines, we will show that these power increments are not achieved in practice".

RC3.24	Figure 9 "wind shear". Turbulence intensity?
AC3.24	Thank you, we fixed the typo.

RC3.25	"the turbine power with no prescribed motion". fixed wind turbine?

AC3.25	We agree that "fixed wind turbine" is better and we implemented what you suggest.

RC3.26	"The increment of generated power … a higher maximum power". Since only the imposed motion is considered and it is the same for both FOWTs, why the difference in the generated power. Could you explain? Please also report the difference in %?
AC3.26	We explained the difference between the 5 MW and 15 MW cases, and we added some numbers for one motion scenario.

RC3.27	"Variations in generated … of 9 m/s". Is this conclusion valid for all amplitudes and all excitation frequencies? Please indicate the limitations for this conclusion.
AC3.27	Thank you for pointing this out. In the revised text we say that the variations in generated power are for all motion frequencies and amplitudes we investigated.

RC3.28	"According to the results of the Harm-M … operating conditions". This conclusions here are not meaningful for actual FOWTs and can be misleading for the reader. A FOWT is a 6-DOF body where couplings among degrees of freedom DO play and important role in defining motions of a given point of the platform. Relative phases of the DOFs are crucial and are governed among other parameters by submerged hull geometry, the excitation frequency and wave direction. Moreover, second-order (slowly-varying) effects should be also taken into account. The results presented in this section may be interesting in the context of software verification or as an academic exercise.
AC3.28	Thank you for this comment, we think it helped us clarifying the validity of the results obtained with prescribed motion simulations. We rephrased the sentence to make clear that the Harm-M simulation simplify the platform motion and this is not realistic. We agree with the Referee that motion of floating wind turbines is a lot more complex than the one simulated with the Harm-M modeling, but as we say in the article: "In order to isolate their effect, we introduce gradually [these] parameters in the analysis." We are convinced the model with prescribed motion is useful to understand the physics of the problem and it is useful for aerodynamic and performance considerations. Coupling between motions, their phases, response to wave excitation are introduced in the other simulations we present later.

RC3.29	"Figure 12 shows Pg for the four platforms". Could you please inform the duration of each simulation?. How many frequencies and wave amplitudes were simulated, how was the discretization? Did you use OpenFAST for these simulations? Please also comment on the transients. Have you removed them (how long have you allowed for transients)? How many cycles have you taken to average your results for power.
AC3.29	We followed your suggestion and we explained how simulations were carried out and how we managed transients.

RC3.30	caption of Fig. 11: "no prescribed motion". fixed wind turbine?
AC3.30	We revised the caption and we used what you suggest.

RC3.31	with the presence of waves or without waves?
AC3.31	We agree this sentence wasn't clear and we rephrased it.

RC3.32	page 24. It would be interesting to see the longitudinal motions (velocity of the platform induced by the 6 DOF platforms) at the hub level and compare with the time series of wind speed at the hub as well as with the generated power.
AC3.32	We appreciated this suggestion because it helps strengthening the link between platform motions and rotor response. We added one figure (Fig. 15 in the latest version of the manuscript) where we compare the hub velocity in the longitudinal direction with the time series of wind speed.

RC3.33	450-460. For the sake of reader's understanding of the phenomenon, could you please further explain the low-frequency fluctuations in the generated power. Are there any other potential sources for it (besides the wind turbulence)? For instance, in Fig. 15b, the generated power for fixed turbulent wind follows a different trend compared to the generated power of the FOWTs. These behavior is not that apparent in the other scenarios. Could you please provide an explanation?
AC3.33	We revised Fig. 16 and we discovered and fixed errors in some of the time series that are displayed. We revised the text that discusses results of Fig. 16 and we explained the power generated in the Coupled-S and Coupled-C cases has the same trend of fixed turbulent wind cases and the main difference is the offset in the mean value.

RC3.34	474-475. To further support this argument, it would be interesting to provide the time series of the surge and pitch velocities of the four platforms.
AC3.34	The surge and pitch velocities are in part shown in Fig. 15 (of the latest version of the article) since they compound to give the hub velocity. Moreover, to answer one request of Referee #1 we added one figure with the spectra of platform surge and pitch motions (Fig. 17 in the new version of the manuscript).

RC3.35	This statement is questionable...First, figure 16 shows that depending on the wind speed the variations in the generated power can be positive or negative, there are no (visible) trends. Second, but probably the most important counter-argument is that for the lower wind speeds, the wind thrust is also lower. Thus, the tilt angles are smaller, indeed smaller that at rated wind speed, where the differences in generated wind power are smaller. If your argument about the effect of tilt would be correct, then as the tilt angle increases the reduction in generated power would increase, especially for under-rated wind speeds. Also, how do you explain the increase in the generated power at around 8 - 9 m/s for the spar and semi-submersible (coupled C-scenarios)
AC3.35	Thank you for this comment. To address it, we changed Fig. 18 (previously was Fig. 16) and now it is easier to see trends where they are present. We believe the argument about static tilt is valid and it can be seen in the power curves of the 15 MW floating wind turbines and 5 MW semi-submersible obtained with

	Coupled-S simulations. Variations in generated power around 8-9 m/s are due to the calculation of the power curve and the lack of data in some wind speed bins.

RC3.36	Could you please provide references to this statement? Please also provide further explanation on how the nacelle-velocity feedback controller works. What are the input parameters for that controller? Is the main parameter the nacelle velocity? If so, in order to proof your statement you could show the time series of that parameter for the 4 FOWTs.
AC3.36	This result is not fundamental for the main goal of the article, and, at the same time, it requires additional information to be justified. Thus, we preferred to remove it.

RC3.37	"to offset the power loss due to platform tilt caused by thrust and platform compliance" as earlier mentioned, this justification is questionable. Could you be more specific on what do you mean by platform compliance?
AC3.37	We clarified that we refer to platform static tilt.

<u>Conclusions</u>

RC3.38	General comment: You should be more succinct and explicit in some statements, avoiding long explanations. Explanations and discussion should be provided in the results and discussion sections, not here. Please, revise the findings.
AC3.38	The structure of the Conclusions was suggested by the Editor before opening the discussion of the manuscript. The Editor commented: "Can the conclusions be arranged in a similar manner [to the introduction], i.e. an initial bullet list with the main conclusions, and then expanding a bit each bullet? Also, usually the conclusions should be self-sufficient, i.e. a generic reader read the title, then the abstract, then the conclusions, so the conclusion should (very briefly) reintroduce the context and the problem statement, together with the main aim and the overall methodology, before focusing on the conclusions." If there are no mistakes, we prefer to maintain the structure that you can find in the latest version of the article.

RC3.39	which motions? Here, the reader may understand that increasing the platform motions (in all DOFs?), the generated power of the wind turbine will increase. This have not been clearly/explicitly proven along the paper. Indeed, since the platform motions are harmonic, during part of its motion cycle, the platform moves against the wind (increasing wind generated power), but then, it will move in the same wind direction (reducing the wind generated power). Could you further discuss (in the previous sections of the paper, not in the conclusions)?
AC3.39	At the beginning of the sentence it is said "along-wind motions". Concerning the Referee comment "but then, it will move in the same wind direction (reducing the wind generated power)" we think it is wrong because, as it is proved in the article with: 1) the analytical model; 2) simulations with prescribed platform motion; 3) simulations with regular waves, when the rotor moves cyclically against wind its power increases.

RC3.40	perhaps you can be more explicit and mention which of the platform's DOFs contribute more.
AC3.40	Thank you for this comment, we revised the text to be more specific.

RC3.41	This statement has not been proven along the paper. Indeed, even eq. (4), which is not rigorously correct, show that the increment in the AVERAGE power is not (linearly) proportional to the wave amplitude.
AC3.41	We clarified the assumptions of Eq. 4. Indeed, the increment of average power is not linearly proportional to wave amplitude, but we never claimed it is.

RC3.42	This statement is not rigorously correct. Typical spars and semi-submersibles with catenary mooring systems move at low-frequency (below wave frequency range) in surge, sway and yaw modes, but for heave, roll and pitch they typically oscillate at the wave frequency range. What do you mena by high and low frequency here. In dynamics of moored floating structures, high frequency and low frequency refer to frequencies above and below the wave frequency range, respectively. As far as the reviewer understands, the turbine controller responds to low-frequency actions rather than to high-frequency perturbations. Then, what would be the mechanism for generating more power from high frequency platform motions?
AC3.42	We agree with the Referee and we reworked this part of the conclusion accordingly.

RC3.43	Which modes of motion?
AC3.43	rigid-body motion modes, we revised the text to make it clear.

RC3.44	where (columns, pontoons, tower, mooring lines, etc?)
AC3.44	In the revised manuscript we say it may result in high structural loads for the floating wind turbine components.

RC3.45	This statement has not been proven or adequately referenced/explored/discussed along the paper. Please see comments in section 3.5.2 (pag. 27) concerning the nacelle-velocity feedback controller.
AC3.45	We removed the statement.

RC3.46	As mentioned in the abstract comments, this statement could be misleading.
AC3.46	We corrected the statement to exclude the option of wave energy converters.

RC3.47	remove “obtained for spar-buoy and semi-submersible wind turbines”
AC3.47	Removed.

Appendix

RC3.48	times?
AC3.48	Thanks, we fixed the typo.