Answer to reviewers

Dear Referees,

Thank you for the opportunity to further improve the manuscript based on the feedback provided in this second round of review. In this document, you will find the answers to your questions and comments, and a description of what has been modified in the revised manuscript.

With kind regards,

The authors

Reviewer 1

Dear Authors,

Thank you for considering my previous comments in your revision of the manuscript. I appreciate the effort made to address the issues I raised. Many of them have been answered satisfactorily in the revised text. Overall, the quality of the paper has improved.

In a few instances, while your responses adequately clarify my concerns, the corresponding revisions were not reflected in the manuscript itself. Although the explanations provided in the response document are clear and satisfactory, my intention was that such clarifications be included in the paper to improve its clarity and reproducibility. Therefore, I recommend that you integrate those responses directly into the manuscript text.

Below, I list the comments that remain open, along with my suggestions for addressing them.

Original comment: Abstract: "Numerical design tools have proven to fail... by unsteady effects". Relatively recent research and community efforts have shown that numerical models are ...

Comment after first review: My concern is the "such as the increase in thrust variation caused by unsteady effects". The known unsteady aerodynamic phenomena that are meaningful for floating wind turbines (dynamic wake, returning wake and unsteady airfoil response) cause a decrease of the thrust force and not an increase. To solve this concern, I propose that you replace "unsteady effects" with the description of such unsteady effects that are responsible of the thrust increment. For example, "such as the increase in thrust variation caused by [dynamic wake]".

We agree with changing the statement, however, since the topic of unsteady aerodynamics effect is not central to this work, we have decided to remove "such as the increase in thrust variation caused by unsteady effects" from the abstract.

Original comment: Abstract: "with direct estimation ... with the turbine in operation". Is this damping increment found also in simulations?"

Comment after first review: I suggest adding the observation made by the authors ("An increase in damping of pitch, surge and yaw DOFs is expected, and this setup allows for quantifying it with measured aerodynamic loads") in the text to support the result of the 210% increase.

It has been added in the text in the conclusions. The abstract has also been modified.

Original comment: L31-34: "the results ... numerical models". How likely it is that the unsteadiness is due to the wind turbine aerodynamics and not something related ...

Comment after first review: Can you explain the physics of the phenomenon that brings such an increase in aerodynamic thrust amplitude? This increment of thrust amplitude is not expected to occur in large-scale floating wind turbines (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5375682). If you can explain the physics of the thrust increase, do you expect it is going to happen also in large scale floating wind turbines, and do you think the observation can be generalized to other rotors/scales?

As for a comment above, we believe that this matter is not central to this work but was studied in a previous work by the authors, and serves here only as motivation. The introduction of this paper, in its current form, merely describes the discrepancies found in the literature without going into the details of the discussion. Since the work has another focus, we believe that this level of detail is sufficient.

Original comment: L62: “for specific studies”. Can you be more specific about the kind of studies where prescribed motions are useful?
Comment after first review: I agree, but my request was to explain it in the text (briefly) to support your argument.

An example of such studies has been added to the text

Original comment: L72: “complex interactions” interactions of what? ...
Comment after first review: the response is fine, update the text accordingly.

It was added to the text.

Original comment: Figure 2: in the caption it is said “The experimental points are averaged over two sets.” Two sets of what? Please explain it in the text. ...
Comment after first review: I suggest revising the sentence "proving its suitability to reproduce the full-scale rotor". You are proving the rotor generates a meaningful thrust force.

It has been modified in the text

Original comment: L117: “without tracking errors” How this relates to amplitude of motion? At least you have a limit on the amplitude imposed by the maximum acceleration of 1g.
Comment after first review: Instead of removing "without tracking errors" I suggest saying "with reasonable accuracy".

It has been modified in the text.

Original comment: L118: “most load conditions” Is it true? A wave period of 8s, which is not so uncommon corresponds to a mode-scale frequency of 6.3Hz which is above the range where the hexapod works ok. ...
Comment after first review: Now the text that describes table 5 says "They are limited to the capability of the setup as the harshest waves cannot be tested because the motion response would go beyond what the Hexapod allows in terms of accelerations." Are cases 1-5 within the acceleration limits or not? From the text, it seems they aren't.

Wave cases 1 to 5 are within the acceleration limits of the hexapod. This was verified in advance with FAST simulations. The text has been reformulated.

Original comment: Section 3: Which is the delay in the measurement-actuation chain? Where it mostly comes from? (e.g. the hexapod motion controller). Which is the transfer function between motion setpoint and actual motion of the hexapod?
Comment after first review: The new analysis is interesting and goes in the right direction for demonstrating the motion tracking capabilities of the setup. From the time series one can see the delay. In the PSD of position shown in the response to reviewers (but not in the manuscript), it is also interesting to see that the set point is amplified at frequencies around the one of platform pitch.
From the bode plot you are showing in the manuscript, it's difficult to identify the delay (linear phase) and the amplification (because amplitude is in dB and reported for frequencies up to 100Hz). I think it would be beneficial to reduce the x axis upper limit to 10Hz to match the rest of the figures in the paper (e.g., Fig 10), to report frequencies at full scale (or add a second x axis with full-scale frequencies), use a linear x scale, use magnitude instead of dB. You can also consider reporting the FRF computed from data instead of the estimated TF.

The figure layout has been modified accordingly.

Original comment: L213: “delays between sensors signals and hexapod feedback”. This sentence is vague, and you should explain here that there is a delay (which is quantified afterwards) and explain which are the consequences of this delay.
Comment after first review: Check the modified text, it has few typos.

Typos have been corrected.

Original comment: L268: “it corresponds to testing in Open-Loop”. It doesn’t. It corresponds only if the robot tracks perfectly the setpoint from the numerical model. ...
Comment after first review: I see the point. Then I suggest stressing in the text that you are verifying only the output of the simulation and not the motion of the Hexapod. You can also say that this is closer to the closed loop since you are running the model on the real-time PC.

It has been added to the text.

Original comment: “L269: “FAST”. Can you be more accurate and specify which version of (Open)FAST you are using?
Comment after first review: Ok, then add the version of the software in the text.

It has been added to the text

Original comment: L276: “stiffness”. I get that you must adjust damping since HydroDyn uses a more

complex model, but why you had to adjust stiffness? ...
Comment after first review: Can you explain it in the text?

It has been explained in the test

Original comment: L305: “sensibly higher for the force sensor”. How much higher?
Comment after first review: explain it briefly in the text.

The explanation has beed added to the text

Original comment: L350: “this is also the region where the effects on wind are visible. The figure only shows the response with wind but not the case of waves without wind (like in Fig. 7-8) so it's impossible to tell at which frequencies the aerodynamic loads have a meaningful effect.
Comment after first review: Can you add in the text references to justify the claim that aerodynamic loads are important at low frequencies?

Reviewer 2

Dear authors,
It was very interesting to review your paper, as I believe in the high relevance of this approach for floating wind hybrid testing and I appreciated the quality of your work.
The paper is well written and I see many improvements have already been made in response to the first review. A significant work has already been carried out. I have two general comments, and a few specific comments.
Two main points to discuss:

1.
I agree with your definition of the total delay was defined as follows in Section 3.3.3: "the total latency between the physical phenomena, i.e. the aerodynamic loads acting on the rotor, and the motion that it induces", however I believe it is underestimated in the same paragraph (50 ms). According to Sec 2.2, the setup should go up to 5 Hz, where the phase shift of the hexapod is almost 90° (fig A2). At 5 Hz, this alone induces a delay of approximately 50 ms. The total delay should be at least the delay of the acquisition and correction process + the duration of the numerical simulation time step + the delay of the actuator (which may depend on the frequency).
So I think this total delay of 50 ms is underestimated and I wonder what is the effect on the estimation of the aerodynamic forces (aerodynamic damping, for instance). The impact is probably limited at low frequencies, where the phase shift of the actuator is lower, but what about the wave frequencies?
I think this matter deserves some discussion, to quantify the effect that the delay can have on the estimation of the dynamic response within the HIL setup and the estimation of the aerodynamic damping at low and wave frequencies.
Similarly, in Figure 5, I think plotting only the PSDs in log scale can be misleading, and showing the time series on a given sample of ~300 s full scale could help visualizing the agreement and the effect of delay.

We agree with your comment on about the delay due to the phase shift of the hexapod being 50ms alone at 5Hz. However, this applies to a case in which the motion is, for example, a pure 5Hz sine. Our estimation of 50ms total delay is was bases on signal analysis of position command and position feedback of a wave cases (multiple frequencies involved), and some assumptions (for example "the latency between the command and actual motion is assumed to be half of it"). Based on your observation,

The phase shift of the actuator does not seem to affect the response in the wave frequency, as demonstrated by an addition to the analysis that was done for the previous review cycle. It compares the PSD of a wave case in open-loop and closed-loop and shows a very good match in the wave-frequency range. This plot, reported here below, is not added to the manuscript because it derives from data of a previous campaign in which the modelling is slightly different, and therefore we believe it is not suitable to be included. In the text, the

50ms estimation has been corrected into “less then 100ms” by taking in consideration your reasoning.

Another observation that can be made by looking at this plot is that the open-loop and close-loop no-wind differ in the low-frequency range. This is most likely due to the HIL itself and already seen in literature [Fontanella et al. 2023]. However, the part of this paper about combined wind and waves is not intended to be extensive, as stated in the last paragraph of the introduction. Therefore, extensive tests and analyses have not been performed in this direction. A sentence explaining this has been added to the manuscript.

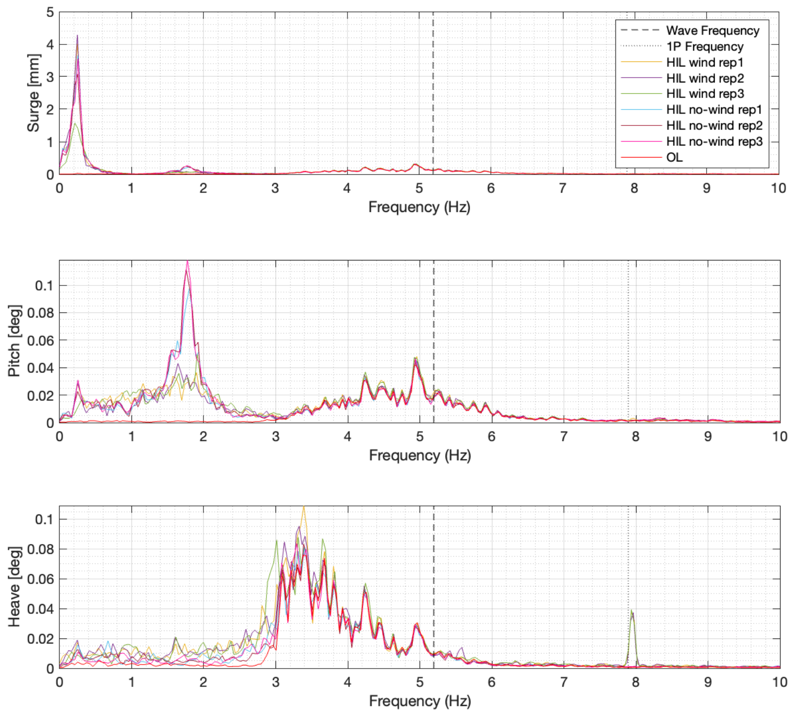
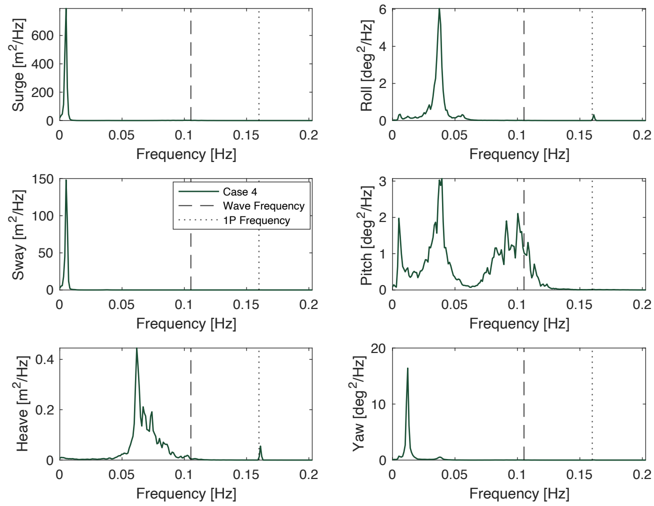
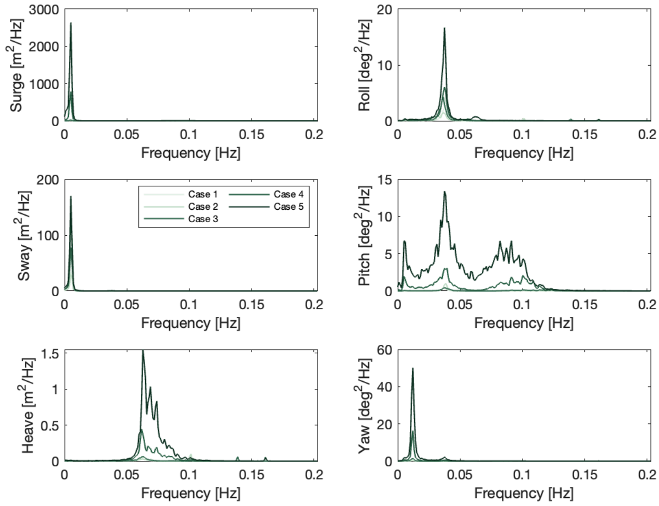


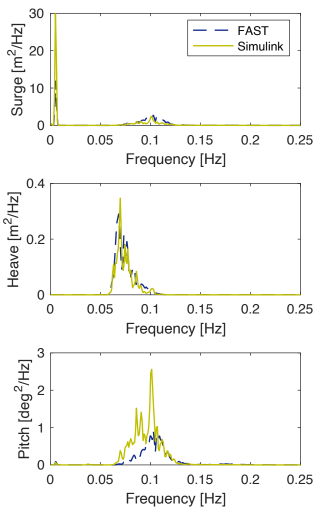
Figure 5, instead, shows the motion response to wave loading of the Simulink floater model in comparison with FAST (benchmark). Both are pure simulations performed in a standalone modality (i.e. the experimental setup was not in use), and therefore, the reasoning on delays is not relevant to this result, neither with regard to delays induced by the actuation of the hexapod nor those induced by the HIL loop. However, the latter can influence the results shown in Figures 10 and 11.

Figure A7 (previously A8), added in the previous review round, can be useful for this matter, by showing a time history of position together with measured forces and accelerations and estimated aerodynamic force. It shows a 250s sample at full scale (5s at model scale).

PDSs in Figures 5, 10 and 11 have been kept in log scale because, upon trying, the linear scale was not found suitable to show the result. However, for completeness, the PSDs in linear scale are reported here (figures 10, 11 and 5 in this order):







2.

In section 3.1, I understand equation (3) and the connected appendix A1 is written in the

frame linked to the load cell, it should be specified.
In the equation of the torque in appendix A1, aren't there terms ("omega x I omega") that are omited from equation (A2)? (see Fossen (2011), Handbook of marine craft hydrodynamics and motion control, for example)
Also this equation doesn't show how the dynamics of the rotor (high RPM) affects the measured force (gyroscopic moments) on the tower. If I am correct, these terms should make gyroscopic moments appear and possibly induce a correction on the yaw moment.
Also in appendix A1, in the equation of the force, the mass (scalar) is denoted M, which is confusing with the mass matrix also written M in other equations. Please clarify.

The reference frame has been specified in the text in section 3.1.

Indeed, the gyroscopic effect given by the kinematics (omega x I omega) is neglected in this formulation. A comment about that has been added in the text, and in the future, we will investigate whether it is relevant to include it or not in the real-time estimation of the aero forces. Similar for the gyroscopic effect given by the rotor: here the gyroscopic moment is measured, but not corrected, i.e. the fact that the rotor inertia about its axis is higher than the scaled one have not be taken into account in the present formulation.

The mistake about M and J naming in equations 3, A1 and A2 has been corrected.

Other comments:

Line 77 : I believe it is worth citing more recent works on this topic (i.e. from the past 5 years) as many ongoing research is made in this field and probably face similar challenges.

The references provided are intended to show the main techniques for hybrid testing in wave basins, which have been developed before, and this justifies the choice of reference. This is confirmed by this review on the topic from 2023 https://doi.org/10.1016/j.apor.2023.103796. After an additional literature review, no additional reference has been found to be relevant to be included or substitute a present one. Moreover, the challenges of wave basin hybrid testing are not central in this work, which focuses on wind tunnel testing. Citing wave basin works, fully-physical and hybrid, serves as a storyline to get to the wind tunnel tests. However, in case you have any reference in mind, we are happy to include them.

Line 135: This is only true if you focus on low and wave frequencies only (which can be slightly higher than ~0.1 Hz at full scale).
It is important to specify if this is your window of interest, because the aerodynamic damping can also play a significant role on the tower bending mode, which is typically at higher frequencies.
The tower or blade modes are not objects of study of this setup at this stage, considering that they are modelled as rigid. The window of interest towards low- and wave-frequencies have been specified in the text

Figure 3: I'm not sure "balance" is the best word in english. I would rather write "load cell".

The sketch has been updated

Line 231: typo “r” added after a point
It has been corrected in the text.

Line 235: typo “is lays”
It has been corrected in the text.

Line 250: typo “details”
It has been corrected in the text.

Line 298: can you specify how the damping ratio is estimated?

The damping ratio was estimated with the logarithmic decrement, which was found to give more consistent results with respect to Hilbert method for the present case. The cycle is used for the estimation. This information, already present in section 5, has been added to the text also in section 3.31.

Figure 5: plotting only the PSD in log can be misleading (particularly on the Surge and Pitch). Could you add a time series comparison, on a sample of ~300 s? (several low frequency oscillations)
For the current topic, a quantification of the error on the wave frequency amplitude and on the pitch and surge response amplitude at the natural frequency would be relevant to add.
One can refer to Figure A7 (previously A8) for a visualisation. The PSDs in linear scale have been added to this document (see above), but were not found suitable for the manuscript because the linear scale leads to a poorer visualisation of the result, for the present case.

About the error quantification, see the reply to a previous comment.

Line 318 / Figure 6: There is a visible alteration of the pitch motion when closing the loop in fig. 6, particularly for troughs no. 2 and 3. Can this be commented?

We agree with the comment, and we have provided an explanation for the mismatch in the manuscript, which we deepen here.

The response of the pitch with the loop closed appears more damped, in the first cycles (more precisely, with respect to the open-loop. Considering that the initial condition is on the high side (5deg), and so also the pitch velocity, and consequently the hub velocity, our explanation is that the drag of the non-rotating rotor (so far assumed null) plays a role here. This could explain this mismatch in the pitch decay, which is not visible in the roll decay, where the match is very good. Pitch an roll have almost identical dynamics and the force correction operates in the same way in the two DOFs, therefore, the drag of the rotor is the most plausible explanation.

Line 331: unfinished sentence “…acceleration term, the two are almost indistinguishable because.”

It has been corrected in the text.

Line 348: see main comment no.1. I think the delays should be quantified with more details.

The matter has been discussed in reply to a previous comment

Line 354: "for investigating"?

It has been corrected in the text.

Line 417: “does not affect roll and pitch responses”: I disagree (see comment for Line 318 / Figure 6)

See the reply to a previous comment