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Contact person A. Hegazy
Telephone +31 (0)15 27 89201
E-mail A.R.Hegazy@tudelft.nl
Subject Response to Reviewers

**Delft University of Technology**

Delft Center for Systems and Control

Address
Mekelweg 2
2628 CD Delft
The Netherlands

www.dcsc.tudelft.nl

Anonymous Reviewer #1
Anonymous Reviewer #2
*Reviewers, Wind Energy Science*

Dear Reviewers,

We sincerely thank you for your valuable and constructive feedback on our paper. Your comments have greatly assisted us in enhancing the quality of our work. We have carefully considered all of the points raised and revised the paper accordingly. This letter serves to address your comments and provide an overview of the changes made. Below, we will respond to each of your review comments, and at the end of this document, a color-coded revised version is included showing the changes made to the manuscript.

Yours sincerely,

Amr and co-authors

Enclosure(s): Response to comments of Anonymous Reviewer #1
Response to comments of Anonymous Reviewer #2
Colour-coded revised version

# Response to comments of Reviewer #1

General comments:

Reviewer: The experiment is carried out with the software-in-the-loop (SIL) methodology, which is presented in the article. With SIL you are coupling the physical and numerical domains by means of measurements and actuators. In my experience, this coupling can be affected by issues such as delays and it's not perfectly "transparent" (i.e., it perturbs the wind turbine response compared to a case where all subdomains are physical). In this sense, do you see any effect of the SIL technology itself on the results of the experiment? Maybe you can add some short comments on that in Sect. 2.3 or in the Results section.

Authors: *Thank you for your comment. This is indeed an important point to address, as the delays of the system and the lower limit on the rpm are limitations of the current setup. We have modified the text as follows:*
**Text excerpt:** "A feedback control loop enables the system to reach a satisfactory accuracy, particularly at low and wave frequencies, which are the main focus of the presented analysis. The delays of the system and the minimum rotor speed are limitations of the current setup. The total delay, defined as the delay between measurement of the state of the physical model and the reproduction of the aerodynamic force corresponding to this instant (sum of delays induced by the acquisition, simulation, and force reproduction) is estimated as 100 ms in the worst case. In terms of force magnitude, the error on the reproduction of the axial force is lower than 5% at low and wave frequencies [1]."

Reviewer: The feedforward controller is derived based on a model of the floating wind turbine that is obtained by means of system identification applied to a QBlade model. What did you model in QBlade: the full-scale wind turbine or the scaled system. I assume the scaled floating platform is not a perfect model of the full-scale one (e.g., it has different mass distribution, mooring response, ...). In case you modeled the full-scale (ideal) wind turbine, do you expect that this can affect the results of the experiment? Also in this case, you may add a comment on that in Sect. 2.3 or in the Results section.

Authors: *Thank you for your question. The full-scale model is used in Qblade, and it was able to reproduce the experimental behaviour to a good agreement. The text was modified in Sect. 3.1 instead as follow:*
**Text excerpt:** "To obtain these TFs, which are required for the control design, identification is conducted on the results obtained from QBlade [2], where the FOWT is modelled at full-scale, and was able to reproduce the experimental results [3]."

Reviewer: After the first part of the experiment, it was decided to continue testing on one of the two FF controllers only. I think this choice is not sufficiently motivated (or the motivation is not very clear to me). The FF for platform pitch requires 20% more blade pitch actuation compared to baseline. Is this above some kind of limits of the wind turbine (e.g., maximum pitch rate)? Or did you decide to stop testing the FF for platform pitch because it gave less benefits compared to the FF for generator power but requiring higher control effort? From Fig. 10, it seems the FF for platform pitch is more effective with respect to its control objective ( -20% of platform pitch variation) than the FF for generator power (¡-5%) so I think it's not clear why you decided to continue with the first controller only. If I understood the results correctly, and if you agree with my comments, I think you should also say that you tested both controllers in a baseline scenario, then you decided to explore the performance of wave FF control in a broader range of conditions and, to do so, you focused on one on the two controllers.

Authors: *Thank you for your comment. I guess you misunderstood Fig. 10, as the -20% you are referring to is in the platform yaw DOF, while the pitch DOF is around -5%, which is comparable to the generator power variation reduction, but almost doubles the variation in the control effort. This is in addition to increasing the variation in the tower base bending moment to 10%. The paragraph in lines 274 276 was modified as follows:*

**Text excerpt:** "According to Fig. 10, not only the platform motion alleviation objective requires large actuation effort, but also, it increases the tower-base bending moment, unlike the power regulation objective."

Specific comments:

Reviewer: 12-13. "It was concluded that [...] response than waves.". I suggest removing this sentence and summarizing in a quantitative way the results of the experiment.

Authors:

**Text excerpt: This part was modified as follows:** "It was found that the feedforward controller for the generator power reduces the power fluctuations properly with a fair control effort, while the one for platform pitch motion requires almost double the actuation duty for the same percentage reduction. Furthermore, the feedforward controller was able to counteract the wave disturbance at different wave heights and directions. However, it could not do much with increasing turbulence intensity as wind turbulence was found to have more dominance on the global dynamic response than waves."

Reviewer: 33-35. "As a result, [...] to disturbances.". This sentence should be rephrased. The pitch controller is slower and has limited authority against disturbances of wind and waves.

Authors: *We have modified the text as follows:*
**Text excerpt:** "Consequently, the blade pitch feedback controller has limited control over errors caused by disturbances like wind and waves, leading to a delayed response in control actions to these disturbances."

Reviewer: 77-78. "Whether the scaled-model [...] of the FOWT.". I suggest rephrasing this sentence. It is not a matter of complexity but which domain you want to reproduce with higher accuracy (wind in the wind tunnel, water in the wave basin).

Authors: *We have modified the text as follows:*
**Text excerpt:** "The domain where higher accuracy is required plays an important role in choosing the scaled-model testing environment, whether in a wind tunnel to focus on the aerodynamic aspect of the system, or a wave basin to focus on the hydrodynamic aspect."

Reviewer: 86-89. "When it comes [...] (Al, 2020).". I would skip these sentences. As you say, the FF controller was not tested.

Authors: *This part has been removed as advised.*

Reviewer: 132-134. "This accelerometer [...] thrust forces.". I would remove this sentence because here it is not clear why you have to remove the inertia forces. Explain it in the SIL section.

Authors: *This part has been removed as advised, and the text was modified to become:*
**Text excerpt:** "An accelerometer of model ASC 5525MF-002 is installed on the nacelle, above the load cell at the tower-top. This accelerometer is used to compute the inertial force and the weight terms to obtain the actual thrust forces."

Reviewer: 138-140. "The numerical simulation [...] on the elements of the blades.". Explain this in the SIL section.

Authors: *This part has been explained to the SIL section instead as advised.*

Reviewer: Section 2.3. Even if it seems trivial, I suggest adding a sentence in this section to explain that in the SIL approach the wind turbine rotor is not scaled physically but it is replaced by force actuators that emulate the rotor loads.

Authors: *Thank you for the suggestion, and it helps further the understanding of the reader. The first few lines of the first paragraph became:*
**Text excerpt:** "The main purpose of the Software-In-the-Loop (SIL) system is to take care of reproducing the aerodynamic loads on the FOWT model. It is composed of a real-time loop including acquisition, Blade Element Momentum simulation and force reproduction by means of thrusters to mimic the rotor loads, since the wind turbine rotor is not physically scaled."

Reviewer: Caption of Figure 4. I would remind the reader that the numerical model emulates the wind turbine controller and aerodynamic response.

Authors: *The caption has been modified to:*
**Text excerpt:** "Scheme of the real-time loop for the SoftWind SIL approach, with the numerical model emulating the wind turbine controller and aerodynamic response."

Reviewer: Figure 5. If I understand correctly you use an algorithm to predict wave loads and this is the input of the wave FF controller (not the wave elevation). Can you add one figure next to this one to showcase the wave force prediction?

Authors: *This has been included in the manuscript as suggested. I would also refer you to this paper [4], where the algorithm is explained together with the wave prediction used in this experiment.*

Reviewer: 202-203. "takes a structure [...] second-order system". How did you choose this structure?

Authors: *Thank you for this comment. Indeed, it was not clear why we choose that structure. The text was modified as follows:*
**Text excerpt:** "The fitting must be ensured to be of the highest accuracy within the wave band enclosed by the dashed vertical lines in the figures, such that the performance is ensured within the wave frequency range of interest without the need for complex higher order control structures."

Reviewer: Figure 7-8. I suggest merging figure 7 and figure 8 in a single figure with two subfigures.

Authors: *The figures have been merged as proposed.*

Reviewer: "the frequency band of interest enclosed by the vertical lines". How did you define this frequency band?

Authors: *For deep waters, where floating turbines are installed, wind waves have frequencies ranging from 0.05 Hz to 0.2 Hz, where FOWTs operate.*

Reviewer: 216-218. Rephrase these sentences. I suggest saying that you test both controllers in one condition, then you proved that one is more effective than the other, hence you carry out all other experiments with the first controller only.

Authors: *Thank you for your suggestion. The text was modified as follows:*
**Text excerpt:** "First, both controllers are tested at the same condition to investigate their effectiveness. Afterwards, we carried out the rest of the experiments with the more effective one, which was then tested at different wind speeds, turbulence intensities, significant wave heights and wave directions."

Reviewer: 222-225. You can remove these sentences if you rephrase the first part of the introduction of Sect. 4 according to the previous comment. I think that what is explained in lines 222-225 must be clarified before Table 3.

Authors: *Those lines were removed as per your suggestion and considered before Table 3.*

Reviewer: "We begin with illustrating the two different control objectives". You are studying the performance of the two controllers and not illustrating their objective (this was done before).

Authors: *The text has been modified as follows:*
**Text excerpt:** "We begin with illustrating the performance of the two different controllers."

Reviewer: 228-229. "At the end [...] such control.". This part is not clear. At the end of the comparison, you understand that one control objective can be reached with reasonable control effort, but not the other. Then you carry out experiments to understand the potential of one of the two controllers considering a wider range of operating conditions. It should be also mentioned that you compare the two controllers to the baseline feedback controller to assess their performance.

Authors: *The text has been modified as follows:*
**Text excerpt:** "We begin with illustrating the performance of the two different FF controllers for power regulation, and platform pitch motion reduction, each one at a time against the baseline feedback controller. At the end, we should be able to determine the effectiveness of the wave feedforward control for each control objective."

Reviewer: 237-238. "which indicates [...] wave signals". It's not clear what you mean. I think you should say that when you add the FF controller to the FB controller, the low-frequency content of the signal remains the same and you have additional blade pitching at the wave frequencies.

Authors: *The text has been modified as follows:*
**Text excerpt:** "This indicates that adding the FF controller to the control loop on top of the FB controller does not have any effect on the low-frequency content that corresponds to the wind turbulence, as it remains unchanged, but only reacts to the higher frequency wave signals, which results in additional blade pitching."

Reviewer: 244-245. "The PSDs [. . . ] other objective.". I don't understand this sentence. I think you mean that reduction of power fluctuations with the FF controller for power is obtained with less blade pitch actuation that reduction of platform pitch motion with the FF controller for platform motion.

Authors: *The text has been modified as follows:*
**Text excerpt:** "The PSDs in Fig. 9 also show that the reduction in the generator power fluctuations with the FF controller for power regulation is achieved with less blade pitch actuation than the reduction resulting from the FF controller for the platform pitch motion."

Reviewer: "the effect of the feedforward controller for both control objectives.". I think you should add "because the two controllers are expected to decrease the variance of power and platform pitch counteracting the effect of the wave disturbance".

Authors: *The text has been modified as follows:*
**Text excerpt:** "In Fig. 10, the standard deviation, as a statistical metric illustrating the variation of a signal about its mean, is used to demonstrate the effect of the feedforward controllers, since they are expected to counteract the effect of the wave disturbance, and thus, reduce the variance of the generator power and the platform pitch."

Reviewer: "Once you define one symbol, it's not necessary to define it again. I would avoid defining symbols more than once to improve readability.".

Authors: *The manuscript has been carefully reread to avoid redundancy regarding defining the symbols.*

Reviewer: 255-256. "and we even [. . . ] reduction FF control". This sentence is not clear but I don't have a suggestion to improve it. Please double check it.

Authors: *The text has been modified as follows:*
**Text excerpt:** "The variation in $\theta_c$ increases for both FF controllers relative to the baseline case. This indicates that indeed the FF controller is active for both objectives. However, in order for FBFF$_{\omega_r}$ to achieve its control objective, it requires almost half the blade pitch actuation needed by FBFF$_{\theta_p}$ to achieve its control objective. As for FBFF$_{\theta_p}$, the blade pitch variation about the mean value increases by almost $20\%$ above the baseline case to achieve its control objective."

Reviewer: "the thrust force varies". Is this the aerodynamic thrust or the total thrust force (aerodynamic + structural)?

Authors: *This is the total thrust force. We also added 'total' to the text for distinction.*

Reviewer: 272-274. "a huge actuation effort [...] for the rest of this study". See the comment in the General comments.

Authors: *This has been addressed and clarified in the general comments.*

Reviewer: 301-302. "the peak-to-peak value of the signal is decreasing as the turbulence intensity increases". I'm not sure about this comment. With TI=0%, the peak-to-peak is about $2°$, with TI=13.8%, the peak-to-peak is about $8°$.

Authors: *This is correct for the blue curve corresponding to the FB controller only, while we refer to the red curve of the FBFF. Thanks for pointing this out as it was not clear, and has been clarified in the text.*
**Text excerpt:** "the peak-to-peak value of the FBFF control signal is decreasing as the turbulence intensity increases"

Reviewer: 229-330. "as there is no clear [...] for every DOF separately". These two sentences are not clear to me.

Authors: *The text has been modified as follows:*
**Text excerpt:** "Regarding the platform motion, each DOF has a different sensitivity to the wave height variation."

Reviewer: 346-347. "we can not [...] the generator power signal". Do you have an explanation for that? If yes, maybe you can add a short comment.

Authors: *An explanation was added to the text has been modified as follows:*
**Text excerpt:** "This is an expected result, which is attributed to the fact that the system identification was performed with the wave excitation moment as a disturbance input instead of the surface wave elevation. This was done for the purpose of eliminating the effect of wave directionality from the control problem. Therefore, there is no reason to expect the FF controller to be less effective in wave conditions with different directional spreading, since the directional spreading does not have a very significant effect on the distribution of the wave excitation forces over the frequencies."

Reviewer: Figure 19. If Fig. 19 does not add value to the discussion you can remove it and just explain by words that blade pitching is the same in all cases you analyzed.

Authors: *It is definitely a valid point, but we thought it was better to stay consistent with the rest of the figures in the manuscript.*

Reviewer: Through the result section, we see the response of the platform DOFs is not coherent, and they react differently to changes in the environmental conditions. Do you have an explanation for this behavior? In case, you may add some short comments in the results and in the conclusion where you discuss the platform response.

Authors: *Unfortunately, we do not have a clear explanation for that behaviour, but we suspect it to be due to the platform dynamics. Besides, the main objective of the controller is reducing the generator power fluctuations, but not the platform motions. Therefore, we can not directly relate each motion to be proportional to a change in a certain condition.*

Reviewer: 351-355. "This is because . . . of the wave fluctuations". Can you rephrase these sentences? I think they are not clear.

Authors: *The text has been modified as follows:*
**Text excerpt:** "Directional spreading results in a reduction of the pitch and surge excitation forces. So in terms of the wave excitation force, an increased directional spreading has an effect similar to a decreased wave height. In that sense, the result in Fig. 19 are consistent with those in Fig. 16."

Reviewer: Section 4.5. all wind speeds are above the rated wind speed. Can you add a short comment to explain why you did not consider below rated wind speeds?

Authors: *There is no scientific reason for choosing above rated wind speeds. It was rather due to the short time available for the experiment, that we decided to focus on the above rated.*

Reviewer: Data availability. I think it is mandatory to have this section. Is there any data/model made available for readers (also by contacting you)?

Authors: *This section has been added to the manuscript.*

## Response to comments of Anonymous Reviewer #2

Reviewer: Page 6. "where kp and ki are the proportional and integral gains respectively, which were properly tuned using the loop-shaping technique." Could the authors elaborate more? How did the baseline controller get tuned? Did you reduce the gain crossover frequency to overcome the negative damping problem compared to onshore cases?

Authors: *Thank you for your question. The gain crossover frequency was indeed reduced below the platform pitch eigenfrequency to avoid the negative damping instability. This has also been reflected on in the results section. Also, a sentence was added for clarification as follows:*

**Text excerpt:**"It is important to mention that the gain crossover frequency was kept below the platform pitch eigenfrequency to avoid the negative damping instability."

Reviewer: Page 11. "In order to obtain the TFs; Gg,(s) and Gp,(s), a chirp signal, logarithmically distributed over the experiment's duration," As I understood, these transfer functions are not physical in the experiment. What is the reason you didn't compute them directly using aeroelastic code instead of using the system identification method?

Authors: *Thank you for your question. This is indeed a fair point. We could have followed a model-based control method, but the reason for our approach here is twofold; 1) While Qblade is a very flexible aeroelastic tool it is unfortunately not equipped with a numerical linearisation capability. 2) We wanted to explore the data-driven approach in a way that would allow us to further apply more sophisticated data-driven control methods in the future.*

Reviewer: Also, did you update your feedforward controller design in different operating conditions, as the frequency responses are different?

Authors: *No, the controller was only synthesised at one operating point, but it was proven to be robust at other operating points as was discussed in the wind speed variation subsection in the results section.*

Reviewer: Figure 6. I understand that the feedforward controller maps the wave-induced moment to blade pitch. Could you elaborate on how the wave LiDAR information is translated into the wave-induced moment?

Authors: *Thank you for your question. Just for clarification, this technology already exists in the offshore industry and is based on RADAR measurements, not LiDAR. Unfortunately, this is out of the scope of this paper. However, some references were cited in the text that would definitely be helpful with this matter. We refer you to [4, 5, 6, 7]*

Reviewer: Figure 6. Did you use the Constant Torque strategy in the experiment? Can you justify it?

Authors: *Thank you for your question. Yes, we did, as the focus was on the performance of the wave FF control rather than the feedback. We did not modify anything within the feedback control loop. We only added an extra FF controller in the above-rated region. This is apart from the benefits of using the constant torque strategy above rated wind speeds according to [8], which was added to the text as follows:*

**Text excerpt:**"Moreover, constant torque strategy was adopted, which on one hand limits the rotor speed variations resulting from reducing the natural frequency of the blade pitch controller, and on the other hand, reduces the drive-train loads and the pitch activity [8]."

Reviewer: Figure 7 & 8. The phase response of your design (red) looks quite different to the actual frequency response (blue). A phase difference would cause a time delay in the FF control action, derived from the disturbance, which might affect the disturbance rejection performance. Could you comment on this?

Authors: *Thank you for noticing this, and it is definitely correct. However, it was only a matter of phase wrapping which was causing the phase to look differently. It is also important to bear in mind that the phase should only be accurate within the frequency range of interest. This has been modified in the manuscript.*

# References


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[8] T J Larsen and T D Hanson. A method to avoid negative damped low frequent tower vibrations for a floating, pitch controlled wind turbine. 75:012073, 2007.