

Comments and answers REVISION 2 Report 1

Thank you again for taking the time to do a second revision, I really appreciated it.

For Equation 1, the transformed acceleration is expressed as a function of time and yaw angle. I wonder whether the raw acceleration should also be formulated as a function of time and yaw angle. In addition, the variable z in the function requires explanation as well, as it is the first time to show in the paper.

- Yes, you were right. The raw acceleration should be function of time and not of yaw angle, since the rotation matrix is function of the yaw angle. This was corrected in the newest version.

The caption for Figure 7 is missing.

- Caption was added

In the last two paragraphs of section 3.2, it is stated that 'it is apparent that the adjustment primarily affects frequencies below 0.01 Hz'. This statement may need more careful consideration. On the one hand, based on figure 11, the adjustment affects the not only frequencies below 0.01 Hz but also frequencies higher than 0.6 Hz, and these high frequencies range seems to be mainly the system bending modes, where tower stiffness contribute a lot. On the other hand, since the discussion here is about overall system behaviour, it is better to limit this conclusion to where it applicable (which variable). For example, the tower top displacements from figure 12 is also one of the overall system behaviours, but it does not follow this conclusion.

- I have rephrase the last two paragraphs. I have clearly explain that there are two affected zones by the tower stiffness reduction. The first one is in the very low frequency content and in a higher range that affects the tower modes. The initial idea it was to show the same middle image of figure 9 in a logarithmic scale to depicts the variation in a low frequency range.

Regarding the reference format, the journal guidelines provide examples: <https://www.wind-energy-science.net/submission.html#references>. In the current version of manuscript, the journal style requires further check. For example, the journal title of cited journal is missing.

- I have added the information missing for each reference as in the webpage of WES.

My last concern is about the hydro-elastic coupling topic and the paper title. The Hydrostar has been used to solve the boundary value problem while accounting for flexible modes. Have the authors compared the resulting wave loads with and without the inclusion of flexible modes? For this spar, how does the flexibility of the floater influence the wave loads? Since one of the main intentions of this paper is to investigate the hydro-elastic coupling effect, it would be valuable to include these findings and thereby contribute to the community. Such results would provide stronger evidence of the hydro-elastic coupling effect and align more closely with the title. structure. Please provide some more information about how the values in Figure 3 are calculated (in 4-5 lines). Since you use those values you must report how you calculate them (eg using 1 day measurements ? 1 hour? how you make the frequency calculations?). You are providing relevant references but this critical information should be included in this paper too.

- We acknowledge that we did not compare the wave loads in the present work. We agree that such a comparison would be highly relevant when discussing hydro-elastic coupling. For this reason, we have modified the paper title to better align with the scope and content of the study actually performed.
- Concerning the additional information of figure 3. We have now provided further details on the values represented in the figure. In fact, this clarification was already included in our first revision submitted on 21 October 2025. A more detailed explanation is now given on how the reference modes were derived and how they were compared with the continuously estimated modal shapes. To ensure accurate tracking of the modal shapes over time, we employed the Modal Assurance Criterion (MAC) to quantify the correlation between the continuously estimated modes and the reference modes. When the MAC value exceeded 0.9, the corresponding mode shape was retained as a reliably tracked mode. These retained modes are the ones displayed in Figure 3.

Comments and answers REVISION 2 Report 2

Thank you again for taking the time to do a second revision.

This work demonstrates the importance of modeling platform flexibility in coupled simulations of FWTs based on comparisons of numerical models with measured in-situ data from a 2.3 MW spar FWT. The tower modes were evaluated from acceleration measurements and compared to modal analysis of the FWT with different assumptions. Additionally, comparisons with coupled numerical models with and without adjusted tower stiffness to match measured tower frequencies were made. I think further elaboration on the methods might be required for this work. Please see the detailed comments below.

- Page 4, lines 46-45: I am unfamiliar with the SSI-COV method, and wanted to check the work you cited (Masjedian and Keshmiri, 2009), but I couldn't find it. The reference list doesn't include where it was published. I think a brief elaboration on the method is necessary, if the reference is not accessible.
 - We apologize for the reference formatting in the previous version. All references have now been revised to follow the format specified on the WES submission webpage (<https://www.wind-energy-science.net/submission.html#references>). Consequently, the cited works now include the corresponding DOI and URL. In addition, we have included an extra reference to provide a more comprehensive overview of the method. Regarding the request for further elaboration, we believe that adding more details would not significantly improve the manuscript. A full description of the method would be quite extensive and falls outside the main scope of this article.
- Figure 4: Can you add the blue crosses to the legend as well?
 - Done
- Section 2.2.1: In the modal approach, the radiation coefficients are usually found iteratively (as in Borg et al. 2016) by updating the mass matrix in the finite-element model by the infinite frequency added mass and solving the radiation BVP with the updated modes until the modes converge. This is not stated explicitly in your implementation. This raises the following questions:

- Are the radiation BVPs only solved based on the dry mode shapes? And if so, what is the justification?
 - Yes, the radiation BVP are only solved based on the dry mode shapes. For the vibrations modes we are looking at, which correspond to global deformation of the floater (in opposition to local deformations of parts of the structure), using wet or dry modes will in our experience result in the same (or almost the same) results because the dry and wet mode shapes are quite similar.
- It is stated that the vibration frequency is obtained when the eigen frequency matches the frequency of the added mass. But that would likely be the infinite frequency added mass limit for the structural modes. For example, the second tower natural frequency is around 1.6 Hz which corresponds to a very short wave. Did you use the infinite frequency limit in this case? And can you elaborate on the details of the radiation analysis stating the mesh size and the oscillation frequencies for which the radiation BVPs were solved?
 - We agree, for these frequencies (1.6 Hz) we could have considered infinite added mass, and it would not have changed the results too much; for the first modes though, we thought the frequencies were closer to the upper limit we had chosen (3 rad/s) and that is what we used. In our computation, there is a 2% difference between added masses at 3 rad/s and at infinite frequency. The mesh size was approximately 0.5m, which is fine enough for the corresponding wave lengths.
- Page 7, lines 13-14: Can you elaborate on what is meant by “artificially separated” and “divergence”
 - We have replaced the term “artificially separated” with “manually separated” for clarity. In the work of Guignier, the floater is divided into multiple rigid bodies, with additional lateral surfaces introduced to close each body. A small manual separation is introduced between these bodies to avoid numerical singularities when solving the boundary value problem (BVP), since boundary conditions are applied to all wetted surfaces. Singularities may arise when the distance between panels approaches zero.
- Page 7, lines 15-25: So, the difference between the this and the study mentioned in the paragraph before is that the radiation problem is solved for the rigid modes of the entire platform, rather than 6 DOFs for each “compartment”? If this is the case, I think this needs to be stated more clearly. May be change the phrasing a little, since saying it’s “a similar but different approach” is bit vague.
 - Yes, this is correct. In Guignier’s work, the hull is divided into several rigid bodies, and a hydrodynamic database is computed for each body (compartment). In contrast, Luan divides the structure into several linear elements, but the hydrodynamic coefficients are determined assuming the entire hull behaves as a single rigid body. We have rephrased this part of the manuscript to make this distinction clearer.
- Page 9, lines 39-40: I think this statement needs to be specific to the studied platform, since rigid body modes can be influenced by modeling platform flexibility for other platforms.
 - We have been more specific by adding the platform type.

- Section 3.1: The relative percentage errors in this section need to be checked. For example, on line 44, the maximum error is +37% which seems to correspond to $(\text{simulated} - \text{measured simulated}) * 100$ instead of $(\text{measured} - \text{simulated measured} / \text{measured values should be the "reference"}.) * 100$. I think the measured values should be the "reference".
 - We agree that measure data should be the reference values. The corrections were done in the paper. The formula used $(\text{simulated} - \text{measured} / \text{measured}) * 100$
- Page 10, lines 28-30: Why didn't you use the value from the aforementioned parametric optimization?
 - Yes we did. It was my error that I hadn't corrected the sentence in page 10 during the first revision.
- Figure 10: Can you add the measured mode shapes in Figure 10, to show the agreement?
 - I do not have access anymore to the data. However I upload the image of measurement shapes next to the simulated shapes to compare.

Syntactic comments:

- Page 3, lines 26-27: I think a clearer phrasing would be "In Section 2.2.3, the potential simulation model adjustments are presented, discussing their limitations..."
 - Corrected
- May be section 1.1 can be merged with 2.1? (to avoid having a single subsection in the introduction)
 - We liked the proposition. We have merge the sections.
- Figure 7 is missing the caption and maybe it would be better to explicitly state the objective value on the y-axis
 - Caption added
- Page 11 Lines 15 and 19: shouldn't this be figure 12?
 - Yes indeed, the text was corrected.