

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.

## Technical comments:

- 1- 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.
- 2- Figure 4: Can you add the blue crosses to the legend as well?
- 3- 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:
  - a. Are the radiation BVPs only solved based on the dry mode shapes? And if so, what is the justification?
  - b. 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?
- 4- Page 7, lines 13-14: Can you elaborate on what is meant by “artificially separated” and “divergence”
- 5- 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.
- 6- 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.

- 7- 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  $\left(\frac{\textit{simulated} - \textit{measured}}{\textit{simulated}}\right) * 100$  instead of  $\left(\frac{\textit{measured} - \textit{simulated}}{\textit{measured}}\right) * 100$ . I think the measured values should be the “reference”.
- 8- Page 10, lines 28-30: Why didn't you use the value from the aforementioned parametric optimization?
- 9- Figure 10: Can you add the measured mode shapes in Figure 10, to show the agreement?

## Syntactic comments:

- 1- 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...”
- 2- May be section 1.1 can be merged with 2.1? (to avoid having a single subsection in the introduction)
- 3- Figure 7 is missing the caption and maybe it would be better to explicitly state the objective value on the y-axis
- 4- Page 11 Lines 15 and 19: shouldn't this be figure 12?