I have reviewed the paper entitled "Model tests of a 10 MW semi-submersible floating wind turbine under waves and wind using hybrid method to integrate the rotor thrust and moments" by Felipe Vittori et al. and submitted to Wind Engineering Science.

The authors describe an experimental campaign testing a floating offshore wind turbine by means of an hybrid method. They manage to generate wave and wind loadings. The novelty of the setup is that they includes not only the main aerodynamic thrust but also moments in the turbine plane, around the vertical and horizontal axis. As a main result, the authors shows that in order to predict the pitch natural period, the hydrodynamic database should be computed based on the geometry of the floater in its mean position (tilt) due to the average wind loading. This result is surely general to all the floater that have significant change of hydrostatic restoring moment with trim.

The floater they used in the campaign is developed by Saitec. This restricts the data that can be shared like the natural periods, the size of the platform, the description of the mooring lines.

The development of the SiL system is described in previous papers. The main components of the setup are briefly described in the paper (actuators, software for aerodynamic computations, software for actuator command). The performance of the system for the frequency range at aim in this paper (from 0 to 0.16Hz FS, 0 to 1.2Hz MS) is missing (phase, gain, response time, frequency bandwidth and delays); such data should be used in the discussion of the comparisons between experiments and simulations. Is there a load cell at the top of the mast that could provide the measured thrust and moments? That would be nice to compare the aerodynamic loads in the experiments (imposed by the SiL system) and the loads in the simulations, in the time domain and in the frequency.

One main comment is that the authors present many observations of the results shwon in the Figures but they often stop there. They should always try to give possible explanations or ideas that would help understand the origin of such observations.

The authors used confidential restrictions in the decay tests (cf. line 161), while in the other tests, the presentation of results is not so strict and the given results enable the reader to find some data that might be confidential.
For instance, the reader can deduce the natural periods from the time domain Figures 7a, 7c, 8a, 8c (time axis is given explmicitly with figures and unit) and the frequency domain results presented in Figures 7b, 7d and 8b, 8d may be used to confirm those, even if the frequency axis is given without figures. Here are some guess, at FS
Surge natural period is estimated at 86 s (12 mHz).
Sway natural period is estimated at 106 s (9.5 mHz).
Pitch natural period is estimated at 30 s (33 mHz).
Yaw natural period is estimated at 63 s (16 mHz).

Remarks:

paragraph 2.2
The experimental setup matchs the target RNA mass. What about the CoG position (important for trim angle) and moments of inertia (less important, for dynamics).

Regarding the calibration of the numerical model, it seems that it consisted in tuning the stiffness matrix and the damping coefficients. The corresponding paragraphs are however in different sections. Concerning the stiffness that was modified during decay test, how was it obtained in the first place? theoretically?

Yaw decay test shows the worst "good agreement" when compared to the other presented DoF decays. It would worth mentionning the main possible causes, the efforts taken to reduce that and the consequences on the subsequent comparisons between experimental and numerical results such as section 4.3 and 4.4.

Have some decay tests with wind been performed? For instance with constant wind speed, the effect of aerodynamic damping could have observed and compared in simulation and experiments.

The note on the surge and sway definitions (line 160) should be moved earlier when the Figure 4 is described.

Figure 7,
The experimental surge motion is very well reproduced by the numerical model. The relative error on the moment of order zero of the PSD must be very small.
The same agreement is found for pitch motion at low frequency but around pitch natural frequency, the numerical model estimation is lower than the experimental one. What is the performance of the SiL system around that frequency? In other words, is the experimental peak at 33 mHz an expected feature or is it due to the SiL system interacting with the resonant DoF?

Figure 8,
In this Figure, the numerical responses differ from the experimental ones. Possible reasons are missing in the paper.
In sway, the Fy force is not implemented by the SiL system. Is it modeled numerically or is the aerodynamic loads limited to the thrust only, like in the experiments?
The yaw response being larger in the experiments, the projection of the thrust will have a bigger impact on sway, however the difference between the significant yaw angle in the experiments and in the simulation is not large enough to generate such differences on the sway motion.
What about the cable bundle? Is it inline with the surge axis or is it pulling sideway too?
Is the Fy force generated by the multi-propeller system negligible?

Figures 9 and 10,
The effort made to include 2nd hydrodynamic wave forces in the numerical model deserve more than a statement as simple as "certain improvment". The differences between the two numerical models should be quantified. The world "probably" can be removed (line 207). The second-order forces being nonlinear by definition, more severe sea-states would be more interesting if one want to see the effects of such loads with respect to the wind loads, at low frequency.

Results, Figure 11,
Cancellation happens at a wave period of 6.3 s (160 mHz) at FS. It would be meaningfull to compare numerically the corresponding wavelength (61 m) to the length of the floater. The study of the magnitude of the excitation forces given by WAMIT in surge would confirm (if needed) the position of the cancellation frequency. The fact that the numerical model agrees well with the experiment means that the excitation force and the added-mass are correctly computed. I refer here at the added mass since at such high frequency, the RAO is indeed the ratio of excitation force divided by mass and added-mass terms.

Figure 13 and 14,
What is the reason for keeping 2 numerical models here? If any, it is not given in the next where no mention to the hydrodynamic models appears.

The agreement is no doubt very good, in particular when compared to SiL systems that don't offer this capability of generating moments. Looking in more details however, we see both an amplitude mismatch of the yaw amplitude and a delay, at wind speed 7.5 m/s.
If the SiL system is to be used for a FOWT that presents a mooring yaw stiffness, then the effect of the vertical axis moment will have to be quantified.

Section 4.5,
The results presented in this section are of great interest for the reader willing to study floating wind turbines. The hydrodynamic software used in the paper have been developped, validated and used for offshore oil and gas systems where the rest position of the floater is the mean position in waves, and where the effect of wind is mainkly an additional drift force.
For floating wind turbine, the wind has one major contribution, as said by the authors: the mean geometry of the floater is changed due the trim generated by the mean aero thrust and this may have consequences on the hydrodynamic forces such as the hydrostatic stiffness.
The response in pitch is overstimated by the numerical simulation wrt the experiments. What modification of the pitch damping coefficients would be necessary to catch the correct response magnitude at the pitch natural frequency? Would such a modification affects the good agreement observed at periods longer than the pitch natural period?
The SiL performance may also contribute in terms of gain and delay, although the considered periods may be large compared to the response time of the SiL system.