Answer to Reviewers

Answer to Referee #1:

1. *The authors need to cite paper doi:10.1088/1742-6596/2265/4/042008, also authored by them. The paper has some portions which are very similar to those of the one under consideration. Some figures are also identical. The authors need to clearly state which content is novel and which is not. As a general recommendation, results need to be repeated only when they bring added value to the new publication in terms of clarity or completeness. Too similar figures should be avoided.*

   The citation to the referred paper is added in the introduction, specifying that the part of the analysis concerning the thrust curve is completely new, and the methodology adopted even to process already presented data is improved and specified in the body of the paper. Figure 9, 14 and 15 are reporting parameters already analysed in a previous work, but the processing of data behind the figures is improved. First of all, more operative points (i.e. more data) are taken into account in the present work; then, the pre-processing of data is improved to more effectively remove non-significant entries.

2. *Please state which version of FAST (or OpenFAST?) has been used.*

   The version is FAST v8.16.

3. *Uncertainty estimation on experimental data could bring some added value.*

   The uncertainty estimation on experimental data is more complicated in the context of this tests, given the natural environment and the consequent uncontrollability of the inputs (e.g. wind speed and direction). For this reason the analysis was not carried out. However, graphs including dispersion were added in the manuscript (Figure 11 and 12) regarding the evaluation of thrust force.

4. *It seems that the authors did not use a yaw control (is this the case?). If so, I would suggest plotting the wind direction trend vs time to demonstrate to the reader that yawed functioning conditions are not affecting the results.*

   The yaw control wasn't activated during the tests, mainly because of concerns on the wind direction measurements, on which the controller relies, that shows fluctuations during transients. The yaw was aligned to the wind direction before each startup of the turbine. However, if the wind direction changes during the tests the rotor faces a misaligned wind that can alter the performance. For this reason, the misaligned cases are excluded in the analysis: in the evaluation of the regime points the points that present a wind misalignment outside a threshold of 5 deg are discarded.

5. *The English form has some minor flaws. The most apparent one is "performances" that should be singular in technical writing. A revision by a native English speaker is suggested.*

   Mistakes are corrected in the text.

6. *The explanation for the discrepancies seen in Figure 14 is a little bit vague. Please try to better motivate the loss of performance in the below-rated region. Could this be associated with aerodynamics?*

The discrepancies between the design curves and the measurements observed can be motivated with two main causes. The first apply for the below-rated region only, where the discrepancies are higher, and is related to the modification of the demand curve of the torque controller. This modification was introduced during the setup tests performed on-site and it was introduced to overcome problems on the startup of the turbine, that was found to be problematic in specific cases and in particular for high wind speeds. The modification consists of the introduction of a motoring torque for low rotational speed and on the shift of the speed thresholds. This directly leads to the modification of the operating points (rotor speed and torque) in the region. The modified curve can be seen in doi:10.1088/1742-6596/2265/4/042008. The second reason is related to the uncertainty that persist in the wind speed measurements also after the correction is applied, also given the complexity of the procedure. This can slightly affect the measurements both in below- and above-rated region. However, also a better-than-expected efficiency of the profile could explain the slight upward shift in the pitch angle curve in above-rated. The latter could also be explained by the lower above-rated torque (and thus power) found in above-rated and derived by the oscillation of the rotor speed causing a non-constant torque demand from the controller that results, in average, lower than the maximum torque. This would cause the increase of the pitch angle that is seen. The whole explanation is also summarized in the text.

7. *As a general impression, the paper is very "descriptive", i.e., shows good results, but probably lacks a little bit of a critical perspective. I would recommend revising the final part of the paper in a way that could provide to the reader a more critical insight for example on the validity of the methods used for future studies, on the source of uncertainties in the real environment or in some prescriptions to manage wind turbines in multi-purpose platforms.*

   Considerations about procedures for tower dynamics inspection, thrust estimation, and controller adaptation are added in the conclusions

<u>Answer to Referee #2:</u>

1. *The paper could say a few more words on the overall advantages/disadvantages of the multiplatform concept. For example, are there any studies that showed the potential benefits of combining floating wind and wave energy converters from a system point of view? And also with aquaculture?*

   A reference about the economical convenience of multi-purpose platform is added, with a focus on mooring systems and electrical dispatch system sharing. Two references about studies on favourable effects on combined power generation from wind and waves is added. No reference was found on fish farming dedicated multipurpose platforms.

2. *Line 39: The paper mentions that wave-tank tests have been performed at ECN. Can the authors comment on how the present 1:15 model scale results compared to the small- scale results? This would help support the statement that "large-scale models are technologically very similar to prototypes and can reduce scaling effects". Alternatively, the authors can point to a reference showing this.*

   In Ruzzo et al. "On the arrangement of two experimental activities on a novel multipurpose floating structure concept" is shown the comparison between the two experimental setup. The reference is cited in the text

3. *Page 3 Line 82: the choice of the airfoil is justified by the fact that it is suitable for low Reynolds number applications. However, the environment here is more realistic (i.e. higher*

Re) than in a wind-tunnel environment. Can you give the typical Re values encountered here and explain why the low-Re airfoil was still a good choice?

Even though the environment of this application is realistic, the model faces a Reynolds number mismatch of around 60 with respect to the full-scale. This is way less if compared with wind tunnel values but justifies the choice of the profile. The profile thickness results an intermediate choice between the full-scale and a wind tunnel model. More information, including the analogy between the selection of the profile for outdoor and wind tunnel applications and values of the Reynolds number along the blade span, can be read in doi:10.3390/en14082119 (Figure 3). This reference is cited in the text.

4. *Page 4 L 122. The goal is to investigate the effect of floating motions on the turbine power production and structural health. However, because the platform is quite large compared to conventional floating wind turbines, the motions are expected to have a much smaller impact on the turbine. Can the authors comment on how the floating motions of this multiplatform differ from those of a floating wind turbine without aquaculture/WEC? Also, are scaling effects, i.e. between full scale and a 1:15 model, expected to be larger/smaller/identical for a floating wind turbine when the floating platform is smaller?*

   The necessary premise to be done here is that the platform dynamics is still to be investigated properly. What can be said is that the platform built during this project is surely bigger (in relative terms) than a conventional floating wind platform, and then in general terms less prone to intense motions. However, by inspecting the natural periods of the platform, it is found that pitch and roll motions are located in a frequency range where also longer waves of the first order spectrum are acting. This is triggering time by time a dynamic amplification effect on these motions, that was clearly visible during the experimental campaign. It is to be understood if a full-scale platform in a full-scale sea would suffer from the same issue in a more or less relevant way. Concerning the scaling effects, the most critical issue to be handled during the scaling procedure, that is not present in traditional floaters, is the moonpool effect that is dependent from viscous phenomena.

5. *Pages 7-8. Can the authors provide more information on the post-processing, i.e. filter used, decrement procedure? Alternatively, the scripts could be openly shared so that the procedures can be reproduced.*

   Specification about the adopted filter is added in the manuscript. A reference about the logarithmic decrement procedure is added, together with the specification of assumptions that stand behind this procedure.

6. *Fig 15: What is the peak at around 3.7Hz?*

   The peak is at the double of the 1P frequency (1P is 1.8Hz for above rated operating conditions).

7. *Line 348: Once that --> Once*

   It is corrected in the text.

8. *Line 371 & 427: punctuation missing*

   It is corrected in the text.