WES-2023-107 Response to Editor

Dear Editor,

thank you for the time you spent managing and reviewing our work, providing highly qualified and detailed comments that helped us fix some flaws of our study. Please find below detailed answers to your comments, reported in blue colored text for your convenience.

Best regards,

F. Papi, G. Troise, R. Behrens de Luna, J. Saverin, S. Perez-Becker, D. Marten, M.L. Ducasse, A. Bianchini

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The article is mostly well written.

It seems more suitable as a technical report, but could in principle be published. However, I would ask the authors to consider what readers can actually learn from this work, and to add a few sentences about this to the manuscript.

Dear Editor, we have addressed your relevant comment in two ways:

1. Summarized the objective of this work better in the introduction of the paper: “Ultimately, the objective of this work is to provide wind turbine modelers and practitioners with a quantitative indication of the impact that model fidelity has on FOWT design loads and provide guidance in the selection of the most suitable approach for each task at hand.”
2. Improved the description of the main handouts of this work in the conclusions: “Overall, the main outcomes of this study can be summarized as follows: i) the differences between the compared modelling theories are consistent with the existing body of literature on onshore wind turbines. ii) the greater movement that FOWTs are allowed did not exacerbate the differences to the point that simpler models, such as OpenFAST, are outdated. These tools remain reliable for extreme load estimation. For fatigue loads, underestimation with respect to more physically accurate theories of 2-15% depending on the specific load sensor can be expected. Therefore, within the limitations highlighted in this and other similar works, these models are still relevant for industry and for many research applications.”

That said, there are also a number of minor issues that should still be addressed:

1. Abstract - "Consensus is arising on considering floating offshore wind as the most promising technology to increase renewable energy generation offshore." - Said like that, this does not seem to be true. It is still monopile foundations where we expect the biggest increases in the coming years, in

absolute terms (e.g. see US DoE Offshore Wind Market Report). Either modify or remove the sentence, or provide justification for it.

We agree with the Editor in the fact that this statement, in its current form, could be misleading. We have reformulated as follows:
“Floating offshore wind is widely considered as a promising technology to harvest renewable energy in deep ocean waters and increase clean energy generation offshore.”

2. Abstract - "differences in fatigue loads are larger for tower base loads than for blade root loads, due to the larger influence substructure dynamics have on these loads." - I do not see justification for this claim in the paper, where is this shown exactly? Why is it not simply the larger moment arm, for example, that explains larger differences in loads?

Thank you for pointing this out. Indeed, we did not highlight this aspect properly in the previous version of the paper. Separating the causes of the differences can be challenging in a scenario like this. Notwithstanding this, there are some indications throughout the paper that indeed substructure movement influences the differences between the codes. We have now better highlighted this in two separate parts of the study. First in section 4.1, where standard deviation of platform pitch is shown in Fig. 4: “Platform pitch is remarkably similar in mean value, standard deviation, and minimum/maximum value for the OC4 test-case (Fig. 4 (e)), although higher standard deviation can be noted for wind speed near cut-in and cut-out in OpenFAST. This is interesting because a higher platform-pitch standard deviation indicates increased gravitational and inertial loading variations on the tower. Very good agreement between OpenFAST and QBlade is also shown in Fig. 4 (c). Despite platform pitch standard deviation being lower in QBlade for most wind speeds, at 13 m/s mean wind speed it is higher for QBlade.”.

Then also in section 4.3.1, where tower and mooring lifetime DELs are shown: “However, the differences in fore-aft tower lifetime DELs (TT Fx, TB My) in Fig. 14 (a) and (c) are larger than those in out-of-plane blade root lifetime DELs in Fig. 8 (a) and (c). Such phenomenon can be traced back, at least in part, to the differences in platform pitch that are noted in Fig. 4, which cause larger or smaller variations in gravitational and inertial forces on the tower, increasing the difference in tower lifetime DELs. “

3. line 36: "on-going OC6" - As far as I know, OC6 has been concluded and OC7 is now on-going.

This is correct, thank you for pointing it out. The statement was true at the date of submission of the draft. It has been corrected as follows: “[…] OC6 (Jonkman and Musial, 2010; Robertson et al., 2014b, 2017; Bergua and et. al., 2023) and the on-going OC7.”

4. line 95: "we did not find suitable met-ocean conditions for this analysis" - This is astonishing, given the large effort on this and many other projects dealing with similar issues. For example, the FP7 MARINE project developed such data (DOI: 10.1115/1.4029842). Why are these data not sufficient for this code-to-code comparison?

The Editor comment was again right, as the text was too concise to be accurate. The analysis required a large amount of specific environmental data, and, as the Editor is surely aware, international

standards are fairly specific regarding the requirements. In addition to the long-term statistical descriptions of the installation sites we also need environmental contours of wind speed and significant wave height. We were aware of the work that the Editor mentions, but, as far as we are aware it does not contain environmental contours in the form that we needed in this study. Moreover, wind-wave misalignment is not considered in the statistical representations in (DOI: 10.1115/1.4029842). Therefore, we decided to follow the route highlighted in the paper. We have amended the paper as follows:

"Some databases containing such met-ocean data can be found in previous work – for a comprehensive literature review see (Papi and Bianchini, 2023) – however if we restrict our research to Europe, we did not find met-ocean conditions that were completely suitable for this analysis. In fact, although conditions for some reference European sites can be found in the open literature, such as in (Li et al., 2015), specific environmental contours are required to perform ultimate load calculations according to the prescriptions of International Standards. Therefore, an open-source procedure to obtain and prepare long-term environmental data so it can be used in a design load calculation of an offshore wind turbine was developed. The procedure that is detailed in (Papi et al., 2022c) and is available open-access for others to use and improve upon (10.5281/zenodo.6972014). A highlight of the procedure is the fact that the statistical description of the installation site includes wind-wave misalignment, which has been shown to have a significant effect on loading (Stewart, 2016)."

5. line 138: Are half-hour simulations long enough to allow for low-frequency responses to build up to realistic values? The usual simulation times used, mainly because of this issue, are three hours. Please explain why 30 minutes is sufficient here!

Thank you for pointing out this important point. We have debated this internally a lot during the project. Some of the numerical models we ran, such as the LLFVW in QBlade, although manageable with modern hardware, are computationally more expensive than most engineering tools that are currently being used for this sort of application, such as OpenFAST. To keep computations reasonable within this project, we tried to reduce the simulation burden as much as possible. The choice of running half-hour simulations instead of the more common one-hour or three-hour intervals was made for this reason. However, this choice is backed-up by existing research. Based on the Editor's observation, we have included the following explanation in the paper: "The half-hour simulation length differs from the more commonly used one or three-hour simulation lengths. The rationale for such long simulations is to allow for enough time for low-frequency response, typical of FOWT systems, to build-up. Existing research (Stewart, 2016) however, indicates that the total time that is simulated within each environmental bin is the most important factor for fatigue-load estimation, rather than the length of each simulation. Moreover, based on the results in (Stewart, 2016), increasing simulation time beyond half-hour for each environmental bin does not appear to yield improved fatigue estimations in most cases. Therefore, considering the comparative nature of the study, two half-hour simulation for each environmental bin were considered sufficient for fatigue load comparison."

6. Table 1: Explain abbreviations (ws, sims) in title.

Thank you for pointing this out. We have added these abbreviations to the nomenclature list and amended the description of table 1.

7. line 294: "QBlade is able to model Wheeler wave stretching" - This should also be available in all codes that implement linear wave theory! And was it used, or not?

To the best of our knowledge, Wheeler wave stretching is currently not available in OpenFAST. On the other hand, after double checking with the project member that ran the DeepLines simulations, we can confirm that Wheeler wave stretching was included in those simulations. The paper was corrected to reflect this (L308).

8. Fig. 7: Why are the blades pitched differently? And why is there no turbulence in the DeepLines model, but in the other two simulations?

Thank you for pointing this out. Simulations in Fig. 7 feature wind speed above cut-out. The controller is not active in these simulations, and the rotor is left to idle with the blades feathered. When the controller is not active, we could not export rotor speed, blade pitch and wind speed from DeepLines. Placeholder values were added to the dataset instead: constant values of 0 for blade pitch and rotor speed and constant value equal to the mean wind speed value for wind speed. We have removed the timeseries in question, as they add confusion to the figure, and cleared this in the figure description.

9. References, line 852: This is incomplete. Year is missing, type of publication and lead institution is missing. And what is "n.d." supposed to mean?

Thank you again for the thorough review. The reference was indeed incomplete. We have added the missing items.

Please also correct a number of tpyos:

1. line 119: "selected process" -> "selection process"
2. line 298: "hydrodunamic" -> "hydrodynamic"
3. line 332: "DeepLines respect to" -> "DeepLines with respect to"
4. line 355: "shown in Fig. 4 (b)" -> "shown in Fig. 4 (d)" ?
5. line 416: "platform roll" -> "platform pitch" ?
6. line 523: "are shown for the in Figure 12" -> "are shown in Figure 12"
7. Figs. 15 and 18: "flier values" -> "outlier values"
8. line 630: "Fig. 18 (b,c)" -> "Fig. 18 (a, b)" ? (Fig. 18 does not have a panel c)
9. line 678: "for extreme loads.. In fact, " -> "for extreme loads. In fact, "

Thank you for pointing all these points out. They all have been corrected. In the case of typo #5, an additional clarification was also added to the text. In case of typo #8, the label “c” was added to the figure where it was missing.