Response to Referee Comments - WES Discussions

Title:
Quantifying the Impact of Modeling Fidelity on Different Substructure Concepts forFloating Offshore Wind Turbines - Part I: Validation of the Hydrodynamic Module QBlade-Ocean

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*All Line and Figure references in the comments refer to the clean version (not LaTex Diff) of the revised manuscript.

Reviewer 1:

General Comments:

Review:
This is a well-written paper describing the verification and validation work for the new QBlade-Ocean features. The example test platforms and load cases are logical and well thought out, covering a range of important physics. The comparisons with OpenFAST and DeepLines Wind show very similar performance in ability to match predictions to experimental results. The low frequency motions that OpenFAST (and other mid-fidelity codes) struggles to accurately predict are shown to be roughly equally undercalculated with QBlade, indicating that this is still a problem that needs to be addressed. The name for the potential flow models is confusing since quadratic transfer functions (or Newman's approximation) are present for some models.

Comment:
We thank the Reviewer for the valuable feedback and comments on how to improve the manuscript. We have considered all the specific comments and each of them has been incorporated in the updated manuscript. In order to respond more specifically to each bullet point in the Reviewer's "Specific Comments" section, we address each of them in detail in the following:

Specific Comments:

Specific Comment 1

Reviewer:
Section 3.1.2: If second order excitation forces are included in the sum and difference frequency, why is the model called linear potential flow?

Comment:
The name linear potential flow combined with Morison drag addressed the linearity of the governing equations required to solve in the boundary element solvers as a preprocessing step (in the BEM code WAMIT). This solver generates the damping and excitation coefficients as well as the quadratic transfer functions. The linear hydrodynamic forces (radiation and excitation) and non-linear hydrodynamic forces (slow drift and sum-frequency) are calculated from these coefficients during the simulation. However, we agree that the naming convention may be confusing and, given that the

boundary condition of the free surface is non-linear in the calculation of the QTF, technically incorrect. We adapted the name for this modeling type in the updated manuscript to “Potential Flow plus Morison Drag” (PFMD).

Change in Manuscript:
Each occurrence of Linear Potential Flow combined with Morison Drag was replaced with Potential Flow combined with Morison Drag. The same was done for all abbreviated occurrences (LPMD to PFMD)

Specific Comment 2

Reviewer:
Section 3.2: What kind of thruster is used to simulate the rotor loads?

Comment:
The brand and model of the thruster (Schübeler HST) have been added to the manuscript as well as a citation to Arnal (2020) for details.

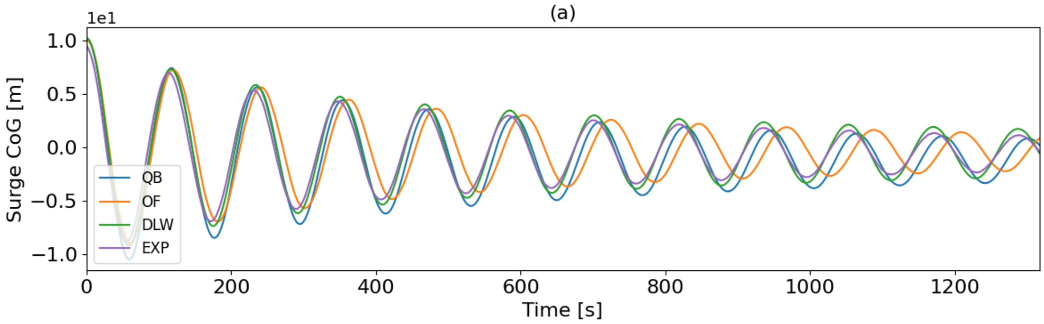
Change in Manuscript:
See comment (Line 213).

Specific Comment 3

Reviewer:
Section 4.2 – last paragraph:
The explanation for the difference in surge frequency focuses on the stiffness from the mooring system, but this was shown in Figure 3 to match very well for both numerical models and the experiment. Is it possible there is some difference in the tuning of the added mass for the two numerical models?

Comment:
We agree that this explanation is misleading as stated in the manuscript since the fairlead tensions indeed are verified in Figure 3, albeit at static conditions. We can rule out the added mass modeling as the cause for the seen deviation since both QBlade and OpenFAST rely on the added mass coefficients (at infinite frequency) that are calculated in a preprocessing step within the BEM tool WAMIT instead of modeling it explicitly (as would be done in a Morison only approach) for both Softwind and OC5.
The observed difference in frequency also cannot be attributed to masses and inertias since the overall masses and inertias of the Softwind and OC5 structures were defined in accordance with the rigid body masses and inertias specified in the experimental definition. Additionally, consistent mass distribution was closely adhered to for the turbine and RNA, as well as for the assembled system CoG, between the numerical codes.
Hence, the dynamics from the mooring lines are the probable cause. MoorDyn (OpenFAST) models the lines as rigid cable segments that are connected with joints that neglect bending stiffness in a lumped mass approach. QBlade-Ocean instead models the mooring system using the absolute nodal coordinate transformation (ANCF) in CHRONO, a nonlinear finite element formulation that includes bending, torsion and shear deformation. As a possible result, both codes required different amounts of additional stiffness in the surge DoF of the OC5 model to tune it towards the experimental result (1e+04 N/m in QBlade compared to 5e+04N/m in OpenFAST).
The close agreement seen between QBlade and DeepLines Wind on the Hexafloat model (see figure below), with no additional stiffness included, further supports this thesis. DeepLines Wind, like QBlade, relies on cable elements to capture the mooring dynamics.

The deviation in natural frequency is small and we updated the manuscript to focus less on the small difference. Moreover, it is updated to state that a possible explanation could be found in the modeling of the mooring dynamics (lumped mass and no-linear cable elements).



Change in Manuscript (line 354 - 362):
A short summary of the explanation laid out in the comment that cites differences in the mooring modeling approach as a possible cause is now written in the corresponding section.

<u>Specific Comment 4</u>

Reviewer:
Figure 4: It would be nice if there was some difference in the markers for 2.1 and 2.2 and if the corresponding wind speeds and blade pitch values were included in the caption

Comment:
We thank the Reviewer for the suggestion.

Change in Manuscript (Figures 4 and 5):
The markers were modified for TC 2.2 and the caption has been completed with the missing information (both for Softwind and OC5).

<u>Specific Comment 5:</u>

Reviewer:
Section 4.4: With these very large wave heights, the viscous forces will also be important, especially for the more slender members of the Hexafloat.

Comment:
We agree with the Reviewer.

Change in Manuscript (line 411):
Viscous forces influence all three models and this information was missing. Two sentences stating this have been added to section 4.4 with the addition of a reference to Lemmer 2018.

<u>Specific Comment 6:</u>

Reviewer:
Figure 7, The amplitude of tower loads at the main wave frequency is significantly smaller for the experiment compared to both numerical models, is there any comment on the cause of this?

Comment:
In contrast to what was our initial answer to this comment, the experimental result could be matched

with an enhanced treatment of the vicious drag on the heave plates. This indicates that the reduced amplitude is the result of non-linear wave interaction with the tower modes. The enhancement originally was aimed at improving the alignment in the non-linear response of irregular wave fields but also showed improvement in the present test case.

Change in Manuscript:
The modifications to the model that lead to this improvement are stated in the manuscript (Sect. 3.4) and it is pointed out that these lead to better alignment with the experimental amplitudes of the tower loads (line 440).

<u>Specific Comment 7:</u>

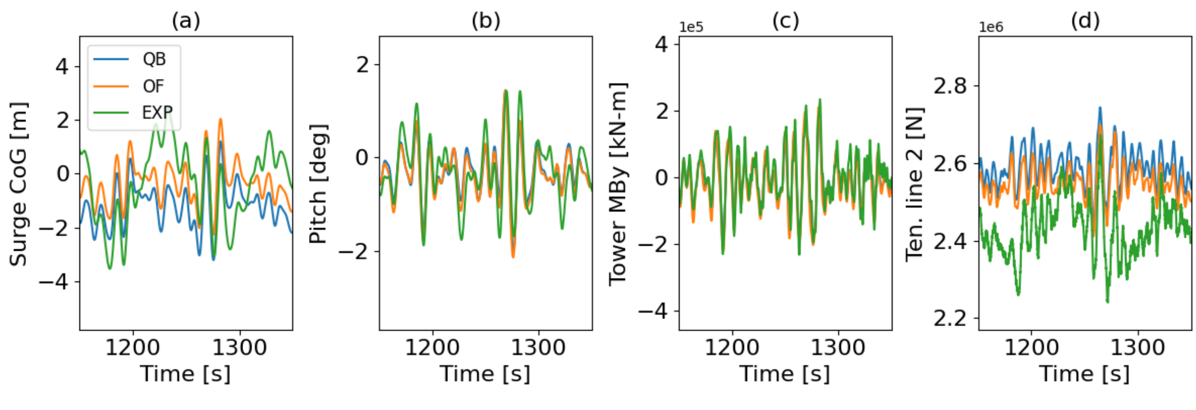
Reviewer:
Section 4.4.2: As mentioned by the authors, a key issue in the surge / pitch frequency motions is the application of the damping from the decay tests to all conditions.

Comment: We thank the Reviewer for the feedback. The manuscript continues to identify this as a key issue and considering this effect together with viscous excitation could lead to a significant improvement in the prediction of motions and loads outside the linear excitation range.

<u>Specific Comment 8:</u>

Reviewer:
Figure 13 (now Fig 10 in the revised manuscript): It is mentioned that the difference in the wave frequency pitch motions between the numerical models and the experiment is on the order of a tenth of a degree, but the difference in range shown in the box-whisker plot appears larger than this. The difference doesn't seem negligible, and may also be related to the tuning of the viscous force coefficients from the decay tests.

Comment:
We agree that the formulation in this paragraph is inviting misinterpretation, and we thank the Reviewer for pointing this out. The description that is mentioned by the Reviewer is supposed to address the comparison between QBlade and OpenFAST to each other and not, as was interpreted, between the numerical codes and the experiment. It is true that the IQR in the box-plot that corresponds to the experiment is wider compared to the numerical codes, this is a result of the non-linear motions too. This is stated just below the sentence that is addressed in the review with QBlade and OpenFAST underestimating the IQR by 16% and 10% respectively (Line 520). A time series of the corresponding test case that proves the statement concerning the difference being in the order of tenths of a degree between QBlade and OpenFAST is shown below.



Change in Manuscript (line 511):
The paragraph is slightly modified in the revised manuscript to clarify this point and to split the discussion between responses within the linear wave range and below it.

Specific Comment 9:

Reviewer:
Line 485: It is mentioned that the lower mean mooring tension in the experiment results in larger motions, but I would expect this to be dependent on the change in tension with translation rather than the mean tension.

Comment:
After revisiting Figure 13d (Figure 10d in the revised manuscript), we agree that the discussion in line 485 (of the original manuscript) is not sufficient to explain the increased response towards the lower end of the linear wave frequency range seen in the PSD of the fairlead tension in the experiment. After consultation with the ECN researchers involved in the Softwind SIL setup, it is believed that the cause could be an incorrect estimation of the hydrodynamic damping on the mooring lines, which is applied by the Morison drag. This damping term often is sea-state dependent and can cause these discrepancies. The reason why this difference in the fairlead tension is not visible to the same degree in the surge PSD could be argued by the fact that the linear wave frequency range can be considered as a high frequency for the surge motion compared to the natural frequency in this DoF, which is well below this range. As a consequence, the response of the platform at wave frequencies is dominated by wave forcing and inertia.

Change in Manuscript (line 526 - 532):
The paragraph in which Figure 10 (formerly Figure 13) (d & h) is discussed, has been modified stating the explanation provided in the comment above together with a reference.

Technical Corrections:

Reviewer: Line 35: “to a large extend” should be “to a large extent”
Comment: The manuscript was updated accordingly (line 36)

Reviewer: Line 65: water plane area instead of water surface area?
Comment: The manuscript was updated accordingly (line 87)

Reviewer: Line 82: Section is abbreviated here but not earlier in the paragraph
Comment: According to the WES guidelines: https://www.wind-energy-science.net/submission.html , “Section” should be abbreviated in running text as “Sect.” unless it comes at the beginning of a sentence. The word “Section” appears twice in Line 99 but only once at the beginning of a sentence. Hence, one instance is abbreviated.

Reviewer: Line 116: Is implantation the correct word?
Comment: Thank you for pointing this out. No, implementation was indeed the word we wanted to use. The manuscript was updated accordingly (Line 131)

Reviewer: Line 352: Should include in this sentence that the discussed load cases are with the DTU 10 MW RWT
Comment: Thank you for pointing this out. The information has been added to the manuscript (Line 393).

Reviewer: Figure 11: It would be great to move the legend so that the mentioned surge discrepancy is visible.

Comment: Thank you for pointing this out. The figure has been updated so the legend doesn't interfere with the data (Figure 8).

Reviewer: Figure 15: Again it would be better to move the legend away from the data
Comment: Thanks again. The figure has been updated so the legend doesn't interfere with the data (Figure 12).

Reviewer: Line 689: Sentence should be restructured with subject and verb
Comment: The sentence was restructured in the manuscript (line 736).

Reviewer 2:

Review:
The paper presents a code-comparison of the newly implemented hydrodynamic module in QBlade which is compared to model test data and simulations with openFAST and DeepLines. Three different cases are considered: OC5, Softwind and Hexafloat where both comparison with OpenFast and experimental data was performed for the first two cases, and only code comparison was done for the latter case. The paper is very well written with clear objectives and discussions**, but maybe a bit too long**. The validation is performed in a structured and stepwise manner where the complexity is increased for each step. **However, the paper itself is just another code-comparison paper and does, as such, not bring any new scientific value to the community**. Methodologies and models implemented and discussed are well established, and any shortcomings of these methods have been well-known for decades. For instance, it is well-known that second order models obtained from potential flow codes tends to fail predicting the low frequency motions and that this phenomena goes further back than the OC5 studies. There are several papers already addressing this issue. **To increase the scientific significance of the paper, a minimum should be to make an attempt to improve the prediction of slow drift motions using state-of-art approach or introduce a new methodology - not only doing a pure code-to-code comparison**. Applying LLFVW in global analysis of FOWTs is to some extend of newer relevance, but not really the main objective of the paper.

Comment:
We thank the Reviewer for the constructive review and suggestions to improve the manuscript. From a general standpoint, in the author's opinion, the scientific value lies in the fact that even though QBlade makes use of several advanced modeling techniques compared to DeepLines Wind and OpenFAST, the overall influence on global loads and motions in the very controlled and idealized environment that we investigated, is only limited. This, while opposing our expectation going into this project, is also a result that is worth communicating with the community. Having this in mind, the validation and verification of the hydrodynamic module of QBlade is a necessity to continue in this direction of research and to have a more detailed look into the possible advantages that the increased fidelity of the structural and aerodynamic methods contribute in more realistic environmental scenarios that should increasingly pose challenges for the BEM method. A subsequent study to the present manuscript analyzes the influence of the increased fidelity in more realistic met-ocean conditions. The considered conditions include misalignment, shut-down, ETM, etc. in a set of DLCs (760 simulations). The models that were used in this subsequent study rely on the QBlade (with LLFVW), OpenFAST and DeepLines Wind models that are presented and validated in detail in the present manuscript and hence builds on the results and findings of this work. A manuscript of this subsequent study in realistic met-ocean conditions is also currently in the discussion phase of WES (https://doi.org/10.5194/wes-2023-107). To emphasize the interdependence of the manuscripts, the titles are linked as companion papers (see (iii) below for further details).

We appreciate the suggestion made by the Reviewer that the scientific significance of this work can be increased by applying recently published methods to increase the accuracy in the prediction of slow drift motions. Following this recommendation, studies on improving the accuracy of slow-drift motions were considered for QBlade. The approaches introduced by Li and Bachynski-Polic (https://doi.org/10.1016/j.oceaneng.2021.109165) and Wang et al. (https://doi.org/10.1016/j.renene.2022.01.053) were investigated and the latter approach was identified as the one with a physically sound and applicable solution to the problem. Hence, the OC5 QBlade model in this study was updated concerning its drag coefficients close to the sea surface level. Moreover, the code itself was modified to optionally apply the axial drag to act on heave plates only on the faces experiencing negative flow with a filter that is applied to the vertical velocity component, allowing to partly separate the damping caused by the heave plate on the pitch and heave DoFs. The same approach is then partially (due to the lack of heave plates) applied to the Softwind model to investigate the transferability of the approach to spar-type floating structures. The improvement on the slow drift response for the OC5 model can be seen in the revised manuscript.

Changes in Manuscript addressing the general comment by the Reviewer:

“but maybe a bit too long”

We agree that the manuscript could be shortened. To streamline the conveyed message, some paragraphs containing non-essential information were shortened (mostly from the static, decay and regular wave load cases). To further cut some length, the frequency analysis from the regular wave section (Sect. 4.4) was removed and only the corresponding time series of the OC5 model remains in the manuscript as the frequency information mostly relates to the importance of using the exact wave from the experiment as an input.

ii) “To increase the scientific significance of the paper, a minimum should be to make an attempt to improve the prediction of slow drift motions using state-of-art approach or introduce a new methodology - not only doing a pure code-to-code comparison”

As indicated in the general comment above, with the implementation of the approach laid out by Wang et al. (https://doi.org/10.1016/j.renene.2022.01.053), a recent proposal to improve predicted response at low frequencies in engineering tools is applied on the OC5 semi-submersible in QBlade. We see that a different parameter setting of the cut-off frequency and blending parameter as compared to the cited reference are required in our cases. The method and the process to find the parameter settings are described in the revised manuscript. The latter information provides valuable input for the applicability of this method to other tools and environmental conditions. Furthermore, the approach is partially transferred to the spar-type Softwind model. The enhanced results are added to the existing figures (irregular wind and wave cases) and show the improvement of this methodology with respect to the experimental result.

iii) “Applying LLFVW in global analysis of FOWTs is to some extend of newer relevance, but not really the main objective of the paper.”

As indicated in the general comment, a study on this topic has been carried out by the same group of authors which is also in the preprint phase within WES (https://doi.org/10.5194/wes-2023-107). An exhaustive amount of simulations of the three FOWTs presented in the current manuscript were carried out in QBlade, DeepLines and OpenFAST in realistic met-ocean conditions. DELs and ultimate loads are extracted allowing a comparison between the BEM and LLFVW methods under such conditions as well as the analysis of their influence on these metrics. The present manuscript and the abovementioned preprint were renamed after consultation with the handling editors to parts I and II that rely on each other’s findings. The common title for both papers is “*Quantifying the Impact of*

Modeling Fidelity on Different Substructure Concepts forFloating Offshore Wind Turbines" with Part I: Validation of the Hydrodynamic Module QBlade-Ocean and Part II: Code-to-Code Comparison in Realistic Environmental Conditions. Both can be seen as sequential steps towards the common goal of analyzing the influence of increased modeling fidelity on design driving loads. Of course, both manuscripts are revisited to emphasize the connection between them.

Additional Comments of Reviewer 2:

Bullet Point 1:

Reviewer: Does the paper address relevant scientific questions within the scope of WES?

- Only partly, as they do not attempt to solve any shortcomings with the methodologies, e.g. improved prediction of slow drift motions.

Comment: We followed this suggestion and the manuscript now includes a method aimed at improving the slow drift motions and loads. As in the original paper, significant improvement in isolated hydrodynamic cases could be achieved. Furthermore, we tested the method on a different floater type. Moreover, we advanced the complexity beyond what was tested by the authors of the method by including aerodynamic loads to test the approach. In combined aero- and hydrodynamic excitation cases, this approach showed to be less efficient in the surge motions and related loads.

Change in Manuscript: Additional Simulation results (indicated by QB2) were added for the figures of relevant test cases and included in the discussion throughout the paper. An additional section "Section 3.4 - Treatment of Non-Linear Excitation" has been added to detail the approach, while parameter studies that help to better understand the approach's sensitivity to them can be found in the Appendix. The introduction contains an additional paragraph discussing non-linear excitation (Line 58).

Bullet Point 2:

Reviewer: Does the paper present novel concepts, ideas, tools, or data?

- No

Comment: This paper represents part I of a two-part work that has the objective to understand the influence of model fidelity on the simulation on FOWT. This, in our opinion, indeed presents novel data and information for the community. We agree with the Reviewer that this message has not been conveyed sufficiently before the revision. Additionally, in the revised manuscript, a method to improve the slowly varying drift forces has been tested in various scenarios that, to the information of the authors, hasn't been done before to the same extent.

Change in Manuscript: The title and the introduction of the manuscript have been modified to emphasize the context of this work in relation to a second paper that builds on the result of this manuscript. Additionally, as indicated below bullet point 1 (BP1), a new method to better capture non-linear motion response has been added, tested and discussed.

Bullet Point 3:

Reviewer: Is the paper of broad international interest?

- Only partly, there is some interest for code-comparison even if new methodologies are not introduced.

Comment: As indicated below BP1 and BP2, we have modified the manuscript to further increase the appeal of the paper.

Change in Manuscript: Reference to BP1 and BP2**.**

Bullet Point 4:

Reviewer: Are clear objectives and/or hypotheses put forward?

- yes

Bullet Point 5:

Reviewer: Are the scientific methods valid and clear outlined to be reproduced?

- Yes

Bullet Point 6:

Reviewer: Are analyses and assumptions valid?

- Yes

Bullet Point 7:

Reviewer: Are the presented results sufficient to support the interpretations and associated discussion?

- yes

Bullet Point 8:

Reviewer: Is the discussion relevant and backed up?

- Yes, but does not bring any new knowledge

Comment: We thank the Reviewer for the feedback. The revised manuscript indeed now brings additional knowledge regarding the applicability of enhanced viscous drag modeling to capture non-linear excitation under irregular wave combined with aerodynamic loads.

Change in Manuscript: Reference to BP1 and BP2**.**

Bullet Point 9:

Reviewer: Are accurate conclusions reached based on the presented results and discussion?

- yes

Bullet Point 10:

Reviewer: Do the authors give proper credit to related and relevant work and clearly indicate their own original contribution?

- No, additional references (outside of the OC-community) where challenges with slowly varying motions should be given

Comment: We thank the Reviewer for pointing this out. Indeed, additional references could have been added.

Change in Manuscript: Various other studies on this matter are cited in the introduction (Lines 58 - 76) and in Section 3.4 "Treatment of Non-Linear Excitation".

Bullet Point 11:

Reviewer: Does the title clearly reflect the contents of the paper and is it informative?

- Yes

Comment: The title has been modified (See BP2) to emphasize the context of this work in relation to a second paper. We still consider the title to correspond to the Reviewer's comment.

Bullet Point 12:

Reviewer: Does the abstract provide a concise and complete summary, including quantitative results?

- Yes

Bullet Point 13:

Reviewer: Is the overall presentation well structured?

- yes

Bullet Point 14:

Reviewer: Is the paper written concisely and to the point?

- The paper is very comprehensive and well written, but is considered to be a bit too long to be concise in this respect.

Comment: We thank the Reviewer for the kind feedback. We have reduced some portions of the written text and excluded several figures, mainly corresponding to the topic of regular wave excitation (the frequency analysis). We also streamlined the main body of text throughout the paper. With these modifications we were able to maintain the same length of the paper while additional information has been added to the discussion (see BP1 and BP2).

Bullet Point 15:

Reviewer: Is the language fluent, precise, and grammatically correct?

- Language is fluent

Bullet Point 16:

Reviewer: Are the figures and tables useful and all necessary?

- Yes, but some figures could be larger to be easier to read.

Comment: Considering the length of the manuscript and the requirements defined in the WES template regarding maximum allowed figure width, it has indeed been difficult to increase the figure size. Axis limits, legend positions were modified to increase the readability and figure sizes were increased whenever possible.

Bullet Point 17:

Reviewer: Are mathematical formulas, symbols, abbreviations, and units correctly defined and used according to the author guidelines?

- Not checked

Bullet Point 18:

Reviewer: Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

- Paper should be shortened

Comment: See BP14

Bullet Point 19:

Reviewer: Are the number and quality of references appropriate?

- Some additional (classical) reference should be added

Comment: Classical references were added to the manuscript to the corresponding sections that describe the basic theory behind a given modeling approach when they appeared (Newman's approximation, Morison's equation, Potential Flow solutions, Froude Krylov Forces etc.).

Bullet Point 20:

Reviewer: Is the amount and quality of supplementary material appropriate and of added value?

- N/A