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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "An efficient frequency-domain model for quick load analysis of floating offshore wind turbines" *by* Antonio Pegalajar-Jurado et al.

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The authors thank the reviewer for the feedback provided. Please find below the reviewer's comments (RC) and corresponding author's comments (AC). PXLY refers to page X and line Y in the manuscript.

RC: I enjoyed reading this article. It is easy to read, has a complete set of equations, and explains the results very well.

RC: This work is relevant. Floating wind turbine evaluations with State-of-the-Art (SoA) time-domain integrated models require significant resources in terms of computations and post-processing of the results. The load case matrix is large, and usually each load case is computed with several realizations of irregular waves and turbulent wind. In floating wind turbine research, the focus has mostly been on time-domain models, due to concern about large motions, nonlinearities and coupling. As these models mature, and experience is gained with different floating platforms, it seems like many cases can be properly linearized and solved in the frequency domain. The impact of this work could be extension of time-domain integrated models to allow efficient computations of some of the load cases in the frequency domain. I think the key to application of methods like the one presented in this article (QuLAF) is automation of the input. If a SoA model is set up for input preparation to QuLAF, the choice is then to just run the SoA model for all the load cases by cloud computing, or invest in some additional work setting up QuLAF, which hopefully then will be recovered by the very fast execution of QuLAF.

AC: Agree. Although the focus of this study has been on assessing the simplified approach and identifying potential improvements, and therefore many things have been done manually (e.g. linearization of mooring system and extraction of aerodynamic damping), the authors believe that most of this work can be automatized if needed. A comment on this has been added to the end of Section 7.

RC: The quality of the article is very good. In my opinion, it lacks only a few clarifications to be ready for publication.

RC: The description of QuLAF, section 5 is quite complete, but I think it would benefit from a few statements right away, on the forcing term on the right-hand-side (RHS) of eq. 5. This information is given later in the paper, but it would be easier to understand the mass matrix, eq. 4, with this information upfront.

AC: As suggested, Sections 5.4 Dynamic response vector and 5.5 Dynamic load vector have been moved to the beginning of Section 5, right after the equation of motion (eq.3) and before the matrices are introduced.

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RC: From eq. 15, we can see that the external forces are transformed to forces and moments at the water line, component 1 -3 in the RHS F. The physical interpretation of component 4 is not mentioned in the article; to me it looks like it represents the part of the external force/moment (component 1 -3) performing work on tower deflection.

AC: That is correct, the last component of Faero represents the effect of aerodynamic loads on the tower modal deflection, and includes aerodynamic thrust force and tilt torque at the shaft. This information has now been added to the text at the end of Section 5.5.

RC: Instead of just defining the mass matrix, I suggest a few sentences on how it is derived (energy method?). All components of the mass matrix except (4,4) can be understood directly by looking at which forces are required to produce unit accelerations along DOF 1 -3. For example, column 1 (and row 1) is the forcing required to produce a unit horizontal acceleration, with no tower bending. Column 4 represents the external (component 1 -3, already known from symmetry) and internal (component 4) forcing required to obtain a tower top acceleration of phi_hub.

AC: The mass matrix was derived from a free body diagram where all the forces were included using D'Alembert principle. A note on this was added to the text in Section 5.1.

RC: Consider moving the sections 5.4, dynamic response vector and 5.5, dynamic load vector to the beginning of section 5; this would probably solve the issues mentioned above.

AC: As suggested, Sections 5.4 Dynamic response vector and 5.5 Dynamic load vector have been moved to the beginning of Section 5, right after the equation of motion (eq.3) and before the matrices are introduced.

RC: For a floating wind turbine with a catenary mooring system, mean drift and current can be important for the mooring line characteristics at the mean platform position. The way I understand the model, this can be taken into account when evaluating mooring line and other mean position characteristics with the SoA model. If this is the case, I suggest mentioning explicitly that mean drift (along the wave direction) and current from any direction can be taken into account in QuLAF.

AC: True. Although only wind has been considered in this paper, the position-dependent mooring stiffness matrix can include effects from other mean forces such as mean drift and current. This has now been stated in the text in Section 5.8.3.

RC: Misalignment of wind, waves and current can be important for fatigue calculations. I think the article would benefit from a few comments on extension of QuLAF to include sway and roll. Do the authors think this would be straightforward, or are there issues with coupling terms etc.?

AC: The extension of QuLAF to out-of-plane degrees of freedom is on the list of future improvements. The authors do not foresee major issues in doing so, and perhaps the aerodynamic loads and damping is where one should be more careful. A paragraph on future improvements has been added to the end of Section 7.

RC: A separate file contains the article, with highlights in yellow and sticky notes with minor questions/comments and edits for consideration.

AC: The suggested text edits have been implemented, and the questions/comments are addressed below.

P5L17: Although this section mainly serves as motivation, I think the mathematically straightforward switch from time to frequency domain deserves a more precise comparison than 'practically identical'. After the initial transient, the differences, if any, should be due to the finite time step in the time stepping scheme, and the corresponding finite number of frequencies in the FFT/iFFT, right? I assume you selected the time step based on a sensitivity study. For example, how much do the maximum deflections computed with the time domain approach change when doubling the time step ? How much do the corresponding solutions in the frequency domain change?. What is the maximum difference between deflections computed with time and frequency domain models differ (making sure the comparison is done after the initial transient is gone) . The time step of 0.01s is typical for time domain-simulations of full-scale floating wind turbines. Does the selected values for mass, damping and stiffness in this example reflect one of the DOFs for the platform in this paper?

AC: Yes, the error between time- and frequency-domain solutions is mainly due to discretization. The example presented in Section 3 was obtained with a 1-DoF model of a lab-scale spar subjected to linear hydrodynamic forcing, used for teaching. Since the only purpose of this section is to illustrate the two methods to solve the equation of motion and to compare the execution time for the same time step, there is no relation between the properties of that spar and the semisub used in the paper. The time step in QuLAF is the same as in the SoA model, which was chosen based on a sensitivity study.

P10L16: is the nacelle total velocity (caused by platform surge, pitch and tower deflection) taken into account when computing the aerodynamic damping?

AC: The aerodynamic damping for each DoF (surge, pitch, tower) is extracted from a separate simulation where only the relevant DoF is active and all the other DoFs are restrained (see P16L21-22).

P23: This is small in a printed version

AC: All results plots have been enlarged.

P25L4: Gumbel distributions are often used in extreme value statistics. Would that be relevant here ?

AC: We believe that in this case the Rayleigh distribution is more adequate to predict the distribution of peaks in one realization. Gumbel distributions, on the other hand, are useful to predict extreme values of many realizations (e.g. estimation of 50-year wave height from annual maxima).

P27L6: Did you try running FAST with rigid blades here? That would be an interesting comparison

AC: Yes, and for the case "Waves + wind 3" (the case with "worst" results in wind and waves) the DEL error changes from -6.87% to -5.56% when the blades are rigid in FAST, hence the blade flexibility plays a role here, but it is not the only cause. A comment on this has been added to Section 6.4.

P27L7: why ? is it more linear in this region than below rated ?

AC: We believe it has to do with the thrust curve being more "flat" above rated than below. This comment has been added to the end of Section 6.4.

P29: ok with web address as reference ?

AC: We prefer to reference the software's website rather than a specific version of the manual. The same is done with FAST, MoorDyn, etc.

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