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Interactive comment on "A model for quick load analysis for monopile-type offshore wind turbine substructures" *by* Signe Schløer et al.

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The manuscript proposes a simplified wind turbine load analysis method for windturbines on monopiles that is based on a single degree of freedom model of the substructure. Hydrodynamic loads are integrated with the shape function, resulting in a generalized load. Wheeler stretching has not been applied. Aerodynamic loads are pre-computed on a rigid turbine and are applied as point force and momentum time series. The aerodynamic damping is determined either by matching the standard deviation of tower top displacements, or by a decay test performed. The latter method determines the damping ratio from the observed decay in the difference of the response with an initial tower top velocity and a tower initially at rest. It is considered by the authors to be more accurate. The equation of motion is solved in the frequency domain, and results are presented for the DTU 10MW reference wind turbine on a

novel Mono Bucket concept. Compared with state of the art FLEX5 simulations of the same turbine, fatigue damage is underestimated in the Mono Bucket, with a damage equivalent load of around 70 percent of the FLEX5 result. The ultimate loads under operating conditions are likewise underestimated in the Mono Bucket by 15 percent. Under storm conditions with a parked turbine, the biggest differences occur in the ultimate loads close to the waterline, which are underestimated by around 30 percent. This is presumably due to the missing wave stretching.

The paper is mostly well written, although the authors have been a bit careless and many typos exist that should be corrected. The topic is interesting and suitable for Wind Energy Science journal. However, a number of issues should be addressed before publication.

1. New content: As this paper is part of a Special Issue of papers previously published with IOP (from The Science of Making Torque From Wind conference), I would have expected a footnote explaining this fact. As the authors are probably aware of, publication in Wind Energy Science journal is contingent on 40 percent new content compared to the previously published work. Can the authors explain a bit how they have updated the conference paper and what new results/content is included in the manuscript here?

2. Use of frequency domain: It is unclear if the phase information is retained or not. Are the coefficients \hat{alpha}_j in Eq. 7 complex? The text ("can readily be transposed to the time domain by inverse FFT") suggests this is the case. If so, the solution is completely equivalent to a time domain integration. What is the reason for the use of the frequency domain then? The speedup due to the possibility of using FFT? Please comment and discuss in the text.

3. Aerodynamic damping: p7, l21f: "... it is necessary to simplify the aerodynamic and add the damping [...] as a viscous linear damping force ..." - This seems a bit too suggestive. Why is it "necessary" to model the damping with a linear viscous damper? (In fact, the aerodynamic damping force is definitely not linear)

4. Calculation of standard deviation: Eq. 15 seems to have some issues. First, why the factor of 1/2? What is the summation over? How is the displacement \hat{u} determined from the previous \hat{\alpha} - is it the same? And should it not be an absolute square of the (complex?) displacements?

5. Determination of damping ratio: p9, l9f: "The damping [...] is found by keeping the pitch and rotor speed constant, since it is a very simple method which can be reused several times" - Unclear what the latter part of this sentence refers to. Please explain.

6. Determination of damping ratio: p10, l1f: "The logarithmic decrement is the average of the four peaks ..." - Imprecise formulation. Did you mean: "The logarithmic decrement has been estimated for the four peaks ... and then averaged"?

7. Discussion of damping: In general, the term "damping" is used somewhat ambiguously. Without further explanation, I would assume it stands for a damping force, but the authors seem to use it mostly for the "damping ratio". Please consider a more precise use. Also, p13, l1: It seems unusual to report the logarithmic decrement in percent - percent of what? This is normally used for damping ratios only (percent of critical damping), and therefore misleading here.

8. ULS wave loads: p12, l25: How were the values giving the largest wave loads in the interval from Eq. 20 determined? Values are given in line 25, but how were they found?

9. Focus on speed: The main motivation for the method seems to be that it results in much faster load simulations. However, alternatives exist that should at least be discussed, e.g. the convolution-based approach in the time domain (Schafhirt et al.: "Ultra-Fast Analysis of Offshore Wind Turbine Support Structures using Impulse-Based Substructuring and Massively Parallel Processors", Proc. ISOPE 2015)

10. Single degree of freedom and effective damping: The focus on a single degree of freedom seems to be quite limiting. For idling loadcases (e.g. the storm condition

discussed in the manuscript), side by side motion of the turbine is expected to be important. Especially, if the waves are not aligned with the wind (although the authors assume the waves always to be aligned with the wind loads). It seems quite straightforward to include at least a second mode for the side-by-side motions. Why has this not been done?

11. Analysis of large turbines: There are indications that for 10MW+ wind turbines also a second tower deflection mode becomes excited. Again, this could be easily implemented. Please comment.

12. Differences in results: p16, l4: "The different results [...] must be because ..." - How can you be so sure? Replace by "seems likely because"?

13. Differences in the results: p16, l5f: "the assumption that Flex5 and QuLa can give the same tower deflections, which does not hold for all wind speeds" - Unclear what this is supposed to mean. Please explain and/or reformulate.

14. Ultimate load assessment: p17, l6: "In each interval the largest load is found and the average of these six loads calculated." - Is this an established procedure from a standard? Please give a reference and/or justification of the procedure.

15. Extreme wave analysis: p19, l21: "In other part of time series, though, the embedded stream function wave results ..." - It sounds as if the extreme wave events was embedded and assessed a number of times - how often?

16. Results in Figure 20: The shown example time series suggest that the response in QuLa is lower than in Flex5. However, the spectra shown suggest exactly the opposite. Please explain this apparent contradiction. Are the time series examples simply badly chosen, i.e., not representative? Or have the colors been mixed up, maybe?

17. Wave stretching: p21, l18f: "This difference could be improved by including Wheeler stretching in the model, which though would decrease the computational speed of the model" - If Wheeler stretching is so important for getting more accurate

results, could you explain a bit more why it would reduce the speed so much? Are the hydrodynamic loads not pre-computed as well (as there are no relative velocities used)? It should be simple to include stretching then, or where am I mistaken?

Minor comments:

- The citation style should be corrected. References should appear in brackets, not directly in the text. Example: "The 10MW DTU reference wind turbine (Back et al. 2013) ..." instead of "The 10MW DTU reference wind turbine Bak et al. (2013) ..."

- Abstract: What do you mean by "a load-based configuration"? Rephrase?

- Abstract: correct "both both"

- Abstract: "Some deviations for ..."

- Abstract: "The differences in ..."

- p1, l17: correct "manufactures"?

- p1, l22: "Furthermore" instead of "Further"?

- p2, l2: "in the time domain"?

- p2, l4: "drive-train, and controller"?

- p2, l14: Web address should be a footnote?

- p2, l23: "two different methods ... are discussed"?

- p2, l30: No commas in section titles

- p4, l16: Consider "Therefore it is the absolute" instead of "not the relative"

- Eq.6 and further: Notation "GM", "GK", "GD", "GF" is highly confusing. Variables should be given one letter only; additional subscripts can be used to differentiate. Change these names to "M_G", etc.?

- Eq.8 "GF" seems to be missing a hat.

- p9, l6: "van der Tempel's method"

- p11, l11: "foundation contributes significantly to the damping"

- p12, l2f: "Since fatigue loads are considered, the wind speed probability of occurrence ..."?

- p12, l6: "and the wind speeds are the same as for load case 1.2"?

- p12, l13: "applies to higher harmonics"?

- p12, l16f: ", which frequency or its multiples are equal..." - please rephrase.

- p12, l17: "However, due to ..."?

- p14, l12f: "exceedance probability curves of the moment peaks" - Consider rephrasing

- p15, l2:", here N_eq = ..."?

- p15, l9: correct "devitation"

- p15, l19: correct "The damping are ..."

- p15, l24: "Since QuLa only has one ..."?

- p15, l29: correct "dampng"

- p16, l1: "ratio of 0.93 relative to those of ..."?

- p17, l1: "it is important to note"?

- p17, l17: correct "ulimate"

- p19, l20: "In other parts of the time series..."?

- p21, l8: replace "most correct results" with "more correct results"

- p21, l9: correct "investiagte"

- p21, l10: rephrase "very close to be identical"

- p21, l14: "the values of QuLa vary"?

- Acknowledgments: missing? No project that financed this research?

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