Answers to Michael Muskulus' review

The referee is thanked for the review. Answers and actions to all points are given below (blue text).

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?
Agree, this will we be added to the introduction. The introduction is extended and other models similar to the presented model is presented.
In the conference paper, the aerodynamic damping was only calculated by decay tests, and the method was not described in the paper. Further, in the conference paper, only load case 1.2 and load case 6.1 was considered. In the present paper, the comparison is more thoroughly and includes also load case 1.3.

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.
Yes, the phase information is retained, and yes the reason for the use of the frequency domain is due to the speed-up. This will be explained more thoroughly 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)
Agree, it is not necessary, but the aerodynamic damping can only be added as a linear viscous damper. This will be rephrased in the paper.

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?
The equation will be changed to time domain as this is easier to interpret:
Sigma=sqrt(mean[(u-mean(u))^2])

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.
Bad phrasing. The text is irrelevant here and will be deleted.

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"?
Correct, the text will be updated.

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

The text will be updated, and it will be specified when it is the damping force, damping ratio and so on. The The logarithmic damping on page 13 will be changed.

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?

For load case 6.1 the calculations were made for 6 different wave periods from 11.1\sqrt{Hs/g) to 14.3\sqrt{Hs/g). It was found that the largest load was found for the smallest wave period. This will be explained in the text.

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)

Agree, the paper will be mentioned in the introduction. However, this method still require that the wind turbine manufacture share information about their wind turbines, which is difficult to ensure.

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 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?

It is straight-forward to add a side-side degree of freedom. This could be done for extended analysis. We have here focused on simplest possible and fastest approach.

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.

We agree this can be addded easily. This forexample done in Smilden et al. (2016) “Reduced Order Model for Control Applications in O↵shore Wind Turbines”. A reference to this paper will be added in the text. It will essentially double the complexity of the model. So far our approach has been to make the model simplest possible and quantify how accurate results one gets.

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

This will be corrected.

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. ‘

This will be rephrased: “…are based on the assumption the tower deflection is the same for Flex5 and QuLA, which is not correct for all wind speeds.

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.

It is not a procedure from the standards, but has been recommended by people in the offshore wind sector. We will try to get a reference in revised paper.

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?
A one hour times series is considered, and divided into 6x600 s intervals. The largest wave in each interval is replaced with a stream function wave.

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?
It is correct that for this part of the time series, the response in QuLA is smallest. However, this does account for the whole time series, and the spectra are based on the whole time series. This will be explained in the text.

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?
The wave kinematics and forces are computed 'on the fly' by FFTs. This allows the subsequent force computation to be fast if it is done on the same fixed z-levels. Hence consistent linear integration up to z=0 can be done fast. When you include Wheeler stretching, the grid spacing of the z-vector changes. Therefore, you have to make the integration of the forces for each time step individually. This slows the process down.
We agree that stretching could be included as part of the pre-computation of loads, and afterwards the wave kinematics could be interpolated to fixed z-values. Then one would have to store the wave kinematics up along the pile to allow for later use in Morison with varying design diameters. We chose the simpler approach of no stretching.

Minor comments
The minor comments will be corrected and acknowledgement included.