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. It is now added in the text that "*\hat{\alpha}_j is a complex number, \omega_j is the smallest angular frequency in the time series and c.c. is the complex conjugate.* The phase information of α is retained in (7)." After (8) is now written that *"By solving the equation in frequency domain the solution \hat{\alpha} can then be solved at once for all time steps".*

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 is rephrased in the paper:
"....the aerodynamic damping can only be added as a viscous linear damping force, where the damping coefficient is a function of the mean wind speed."

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 is 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 is updated:
"The logarithmic decrement has been estimated for the four peaks, both positive and negative, following the largest peak 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
The text is updated, and it is specified whether it is the damping force, damping ratio and so on, which is considered. The logarithmic damping on page 13 is changed to logarithmic decrement.

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 is explained in the text:
"In the present analysis six wave periods from 11.1\sqrt{Hs/g} to 14.3\sqrt{Hs/g} was considered for load case 6.1. It was found that for the present structure the largest load occurred for the smallest wave period, $T=11.1\sqrt{H_s/g}=10.87$s. In load case 1.3 the same ratio, $T=11.1\sqrt{H_s/g}$ is also used."

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 is now 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 Off 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 is rephrased: *"…are based on the assumption that 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.

The following is now added to the text:

"This approach is consistent with the IEC 61400-3 code, clause 7.5.1 for load case 1.3. For load case 6.1a, clause 7.5.1 states that six 1 hour realizations should be considered, unless it can be demonstrated that the extreme response is not affected by application of shorter realizations. Constrained wave methods is mentioned as one way of enabling shorter realizations. This approach has been adopted for the present study. For some realizations, we found that the largest loads occurred at events outside of the embedded constrained wave. This is further discussed in section 4.2.2."

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 is explained in the text:

"In the tower, the energy of the force and moment is located around the first natural frequency and it is clear that QuLA contains most energy at this frequency. This is opposite to the time series, which indicate that the response of Flex5 contains most energy. However, this does not account for the whole time series, which the spectra are based."

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.

Answers to anonymous review

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

General comments

- It is important to highlight the issues and aspects of the "quick" load calculation method that have been adressed here and have not been considered before.

overturning moment include both the

- Which information is missing? The model is presented in the introduction and the following sections.

- Does the overturning moment include both the fore-aft and side-side bending moments.

- No, only fore-aft. This is added in beginning of section 2.2 "The structural model" where the shape function of the model is presented.
  "Only one shape function in the fore-aft direction is considered in QuLA."
  Reason for fore-aft only is to have a simple and fast model.

- please elaborate on how the aerodynamic damping has been precalculated.

- Which information is missing in the presentation of the methods to calculate the aerodynamic damping?

- please consider the use of a list of symbols, since symbols used in the figures are not self explanatory.

- A list of symbols will be added.

- some information about the wind turbine would be useful since some parameters are mentioned many time such as, rated wind speed, rotational speed at rated etc.

- Agree, this will be added, when the wind turbine is introduced on p.12, l.27.

- the damping behavior may change with the turbulence intensity, especially when the wind turbine is operating above rated wind speed, would the computed damping from the different methods change with turbulence intensity.

- Considering the decay tests, it is not expected that the turbulent intensity would change the results much. As can be seen from figure 8, the damping ratio is very similar irrespectively of whether the wind speed is constant or turbulent, which is due to the fact, that the, pitch and rotor speed is kept constant. Considering the standard deviation method, the damping will change if the displacement changes, which is the case if the turbulent intensity is changed.

- the most critical state is usually around rated wind speed. In this case the wind speed fluctuate between below rated (where the pitch is not active) to above rated when the pitch is activated. How would this constant switching behavior affect the calculation of the damping ratio?

- The damping is calculated for an average wind speed of 10 m/s and 12 m/s. For 12 m/s the pitch and rotor speed is kept constant. The average wind speed of 10 m/s did not contain wind speeds above rated, and was therefor not found to cause any problems in decay tests.

- please be more specific on the loads that have been calculated with this method, bending moment in both directions ?

- No only fore-aft direction is considered. This is explained in the beginning of section 4.1: *"Only the deflection in the fore-aft direction is calculated in QuLA and therefore only the forces and moments in the fore-aft direction are considered in the analysis."*

- directions ?
  - for the design of monopile/bucket, pile rotation could be important
- Do you suggest that we should add a mode more? Rotations are included at 'lid' – refer to the spring in figure 2.

Comments inside the paper

All language and layout-issues will be corrected in the paper.

P5:

- No z is here in the horizontal direction, but I agree that this is confusing and will be change.
- The point force is function of \eta_z, and increases therefore as the wave becomes steeper. It is only significant for very large waves, and therefore it can be seen as a slamming force.

P7

1. No only fore-aft
2. Smaller will be replaced with lower.
3. Yes, gravity of both the RNA and the tower. This will be added in the text.

P8

2. Velocity produces damping through the GD term in eq (8). The pile displacement is just used for calibrating the linear damping coefficient. One could also have used standard deviation of tower top velocity. However, we do not expect difference to be large, at least not if the main motion occur at the natural frequency.
3. "Seen" will be changed to "visible".
4. Please see figure 6 and 7. For each decay test a run with and without an initial velocity is performed and afterwards subtracted.

P9

1. Text is updated:
   "The logarithmic decrement is calculated from the difference between the two simulations with and without a starting velocity."

2-3. Text is updated. Blade rotational speed and rotor speed is the same.

4. The choice of a simple damping estimation is part of the models philosophy. We compare the model results to the full aero-elastic model FLEX5 in the paper to quantify how well the approximate steps applied work. This is done in figure 12-17.

P10

5. This might be because the standard deviation puts more weight to the low-amplitude motion. In the decay tests, the damping seems to become smaller for low amplitude motion, see \ref{fig:DecayT} lower plot, for $t>30\,$s.
   The reason there is a large increase in the damping ratio based on the tower top displacements around $W=17$ m/s can be because this method assumes that the tower deflection is the same for Flex5 and QuLA. This is not correct as explained in section \ref{sec:eigen}. As the wind speed increases the tower has a larger deflection and the contribution from the gravity therefore larger. This contribution is not included when QuLA calculates the deflection.

P11

1. A situation with wake could also be considered, as this will change the turbulence intensity, and the aerodynamic forcing. The input to the model is therefore just changed.
2. Yes, soil damping is important to include. The soil damping is part of the viscous damping which represent structural damping, soil damping and hydrodynamic radiation, cf. page 6.

P13

The table text of table 2 is updated.

As explained in the text on page 12 the “+” and “-“ indicates, whether the peak frequency or multiples of the peak frequency are close to the natural frequency.

P17

It is an ultimate limit state, which is considered. This will be corrected in the text.