

Review No 1

Major comments:

1) The article is the combination of two fairly distinct investigations. In particular, the tuning of the model described in the first part is likely quite irrelevant to the second part where the lifetime of the bearings is estimated. In the first part of the article you tune the elastic properties of blades and tower. These do not seem to be the key drivers of loads in the pitch bearings. Is it a good idea to combine the two stories together? Please discuss.

Following the feedback of the reviews, the paper will be shortened to maintain sole focus on the tuning and model description. The damage calculation is covered in another paper.

2) Related to point #1, the power curve comparison presented in section 2.4.1 is not really a validation. If you showed the initial basic model in Figure 8, I believe it would overlap with the red markers generated by the tuned model. The power curve is only mildly influenced by the natural frequencies of the system (except if major instabilities are present, but that's probably not your case). So I don't think this section should be part of section 2.4 Validation. The only real validation is presented in Section 2.4.2.

The power curves will no longer be part of the validation. The validation will instead be extended by comparison of simulation results to field data during operational conditions. Specifically the root mean square of vibration signals over the operational regime of the turbine are compared to the simulated vibration at the tower top.

3) The paper only has 4 sections. I think readability would improve significantly if you could split the narrative into more sections. One idea could be to do: Section 2 Model Generation Section 3 System Identification Section 4 Model Tuning Section 5 Model Validation. If you do so, remember to update the text at line 78.

The sectioning will be revised following this suggestion.

4) The whole scaling process is pretty crude, but, more importantly, it is not well documented. At line 107, what does it mean that "tower bending stiffness is scaled based on rated thrust"? Same for line 113: what does it mean "scaled accordingly"? Section 2.1.3 is also nebulous. I understand that data is confidential, but why do you use such a complex tool like QBLade to do a Viterna extension of the polars? What does it mean that "material properties are linearly scaled"? Or "thickness ratio is equal to 1"? It seems that you simply scaled blade mass by the cube of the length. Am I missing anything? Overall, I would recommend a substantial rewriting of these sections, adding some rigor to the description of the scaling process.

The scaling processes will be described in clearer detail, providing utilized formulars and rigor information.

The section about the monopile will be revised completely, as the design data of the monopile was made accessible since the submission of the paper, such that a scaling of the monopile is no longer necessary.

For the blades (section 2.1.3) design details, such as the blade dimensions, cross sections and weight, are available to the authors. The availability of this information is already confidential. The authors would like to keep it that way, if possible. In any case, the section itself will be revised to provide a more clear procedure of the scaling approach and how one could reproduce this process.

5) Table 1: at 0 rpm you should not have rotor whirling modes. Why do you have 3 distinct natural frequencies for flap and 3 for edge? Also, I don't think you discuss how you've estimated these numbers. The comment also applies to line 426: I don't think you've obtained the first 13 eigenfrequencies. I think it's 7.

The found eigenfrequencies are indeed not from whirling rotor modes. The nomenclature of the modes might be misleading and thus the observed modes will be renamed to follow the nomenclature here:

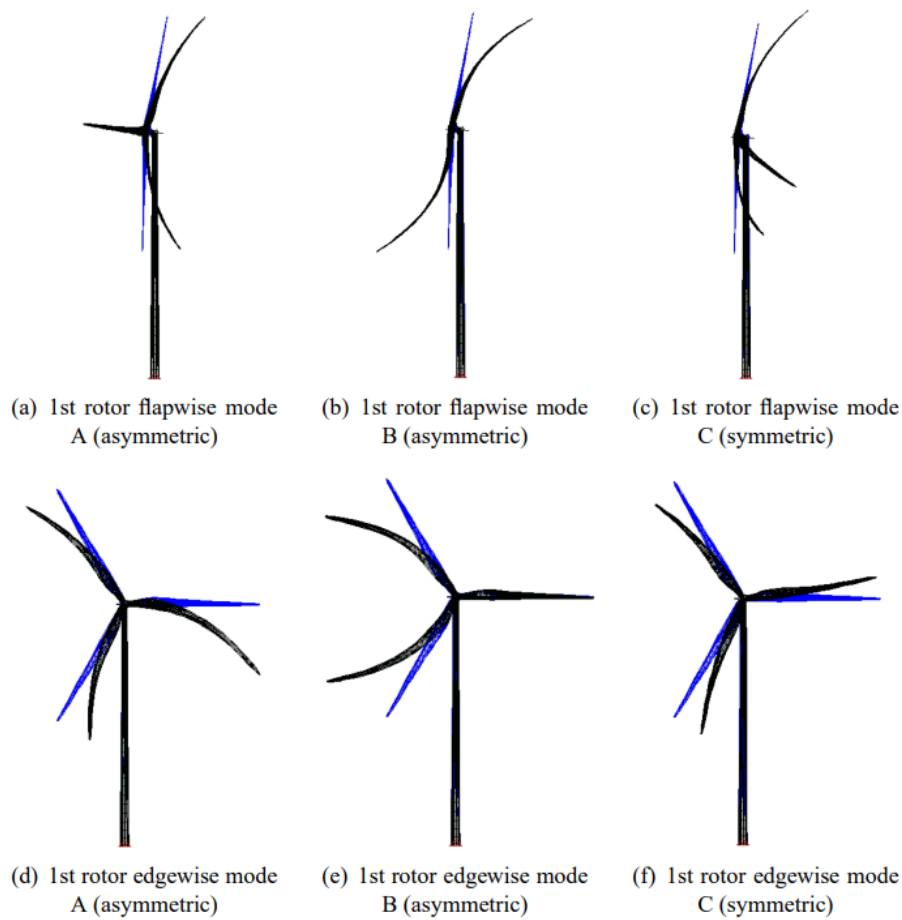


Figure 3.5: The 1st rotor modes of a wind turbine: side view for the flapwise modes and front view for the edgewise modes (undeformed and deformed models are shown).

Found at page 62 of:

Peeters, Joris. "[Simulation of dynamic drive train loads in a wind turbine.](#)" *Katholieke Universiteit Leuven* (2006).

Minor comments:

- The introduction is too verbose. You submitted your manuscript to a wind energy journal, there is no need to talk about global deployment goals. Also because these get very quickly outdated: the USA have just pivoted away from offshore wind (your citation at line 16 is outdated). Please rethink the text between lines 13 and 40 and make it more specific to your manuscript. Please focus on your contributions that are of interest to your readers.

The introduction will be reworked following this comment to make the introduction more terse and relevant to the presented work.

- Line 68: there is a major difference between damage equivalent loads and lifetime estimates, which are usually based on stress metrics. Aero-servo-hydro-elastic models estimate DELs, but for lifetime more is needed. Please highlight this critical difference.

Will The difference between DELs and lifetime estimations will be adressed

- Line 85: "around" 8MW? I imagine rated power is not proprietary...

Will be changed to more specific 8.4MW

- Line 86: This is maybe not of critical importance, but it's surprising to see an expensive commercial tool such as Simpack being used solely for visualization and modal characterization... Although in a clunkier fashion, but NREL tools do both things.

The work indeed could have been carried out mainly in openFAST, but the user interface of Simpack made the process more comprehensible. Especially the calculation of the mode shape coefficients and the visualization of the modes, as tools like ACDC for openFAST were not as developed as they are now, when the work was carried out.

- Line 104: the LEANWIND model could not be validated, when validation means compared to real-world data, since the LEANWIND model was purely theoretical. Note that the citation Desmond et al. 2013 is misleading, because it points to a DNV deliverable that I cannot find online (is it even publicly available?). I would recommend rephrasing this paragraph.

The paragraph will be rephrased and an accessible reference will be provided.

- Line 116: what does "fixity" mean?

The term "apparent fixity" describes a modelling method for the soil-structure interactions. It will be made clear that the apparent fixity model describes the monopile to have 0 degrees of freedom at a given depth below the sea bed, as it currently reads as two separate statements.

- Line 117: define DOF. Also, I think you can better explain that 0 degrees of freedom mean rigid clamping.

The term rigid clamping will be added as well as a definition of the DoF.

- Line 170: it's not entirely clear why you used 4 turbines and not just one. What is the reasoning and value of using 4? For example, the results do not clear characterize turbine to turbine variability.

Blade strain measurements are available for three turbines, whereas none of them have vibration measurements. To minimize the influence of turbine-to-turbine variability, the average modal frequencies derived from the strain measurements are used. It will be clarified in the paper that the applied methodology does not allow an exact match with the frequencies of the individual deployed turbines. However, it enables the development of a tuned model whose modal frequencies fall within the range of variability.

- Line 175: I find confusing that you interchange out-of-plane with edgewise and in-plane with flapwise. I understand that the blades are parked, but I would stick to the words "edgewise" and "flapwise".

The modes will be called flapwise and edgewise consistently now, giving a note at the beginning that the blades are at 90deg, such that, contrary to an operating turbine, out-of-plane bending modes are not flapwise, but edgewise modes.

- Line 191: typo "Utilizing"

- Line 207: what kind of sensors are you using? did you install them, or did they come installed from the manufacturer? These are important details for replicability. For example, can your approach be replicated on any turbine, or does it require the installation of specific instrumentation?

For the vibration data, we use an IPC accelerometers which were installed by us. For the blade strain measurements we obtained data from strain gauges installed by the operator. More information of the utilized sensors and their installation will be provided

concerning the type of sensor as well as their exact location on the drivetrain and the blades.

- Line 209: typo "Hz"

- Line 214: typo "measurements"

- Figure 6: I don't see where you've defined how you've normalized Frequency

The frequency is normalized with a random value to keep confidentiality promises. This normalization will be pointed out in the revised paper to avoid misunderstandings. Or the normalization can be based on the frequency of the highest modes instead, if this is preferred.

- Line 229: I think I understand what a "yaw-inducing" mode is, but I am less sure about the "pitch-inducing" mode? Maybe better to link it back to Figure 5?

While this part will be removed, the yaw, and pitch, inducing modes will be explained a bit more to avoid misunderstanding.

- Line 236: typo "strain"

- Line 328: "as well"? Where else are these parameters specified?

This part will be removed

- Line 334: "The movement is a slow oscillation, which makes the periodicity that the classical calculation approach builds on disappear." What does this mean?

This part will be removed

- Line 405: "Due to turbulence, the wind speed varies over time, when it surpasses the rated speed, the pitch angle is constant at 0 deg, thus reducing the number of pitch movements." What does this mean? Same for "where the thrust is still high but the rated wind speed is seldom surpassed."

This part will be removed