

Review No 2

Major Comments

1) The manuscript would benefit from clearer boundaries between the two main topics (model development and bearing lifetime assessment). Each could stand alone as independent research. Please make this distinction explicit in the abstract and introduction.

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) In the second part, the wind speed distribution of the site is used while keeping TI of the IEC class to define the rolling contact fatigue (RCF) lifetime of the pitch bearing. The rationale for this choice is unclear.

- If the aim is site-specific assessment, why not use measured turbulence?

- If the aim is to explore input classes, why not fully commit to class input levels?

Please justify explicitly.

The chapter on fatigue life calculations will be removed from the paper

3) The study of environmental effects on bearing fatigue life requires a stronger basis and elaboration. While the results are valuable, they do not flow naturally from the earlier sections. Please improve the narrative link (site-specific vs design assessment) or justify clearly based on the response to Comment 2.

The chapter on fatigue life calculations will be removed from the paper

4) Since full environmental measurements are available, responses in standstill states could be filtered to include mostly low-turbulence periods. This would better approximate the white-noise assumption in OMA. Please either apply or comment.

The standstill period was explicitly chosen for a duration of high wind speeds, during low wind speeds the excitation of the system is very small, which results in difficulties to separate modes from background noise. The 24-hour period which was used now, is the only long period with varying but high wind speeds, for which the turbine was in standstill.

5) Validation should not rely solely on the power curve. Although pitch and rpm are mentioned, only presenting the power curve adds little value. Please consider:

- Validating against other outputs not directly used in scaling.

- Normalizing/tabulating percentage differences if confidentiality is a concern.

- Investigating scatter of outputs (variance) vs scatter of inputs (e.g., turbulence extremes).

- Validating against other outputs not directly used in scaling:

- The power curves will no longer be part of the validation. The validation will instead be extended by comparison to field data of vibration signals during operational conditions.

Specifically the root mean square of the vibration signals over the range of operational wind

speeds will be compared between the model and field measurements.

- Normalizing/tabulating percentage differences if confidentiality is a concern.
- It will be discussed with the operator if we can publish pitch and rpm curves as normalized curves.

- Investigating scatter of outputs (variance) vs scatter of inputs (e.g., turbulence extremes).

- Is this referring to the study on bearing lifetime assessment or model development ?

In either case, this can be provided as a series of simulations at the end of the respective paper.

Suggestion: Even with uncalibrated strain gauges, tracking fatigue load responses under two operational conditions and comparing with the tuned model can demonstrate validity for fatigue load comparisons. If not implemented, please mention this as a limitation and suggestion for future work.

It will be explored if it is feasible to compare load responses or frequency spectra of the strain gauges during operation with simulation results to obtain fatigue load comparison. This is most probably a topic for future work.

6) Tuning generic models involves uncertainty due to assumptions and missing information. A dedicated discussion is needed on scaling methods, assumptions, measurement limitations, and their effects.

Suggestion: Robertson et al. (2019), *Sensitivity analysis of the effect of wind characteristics and turbine properties on wind turbine loads* (*Wind Energy Science*, 4:479-513), could serve as a base to highlight potential biases. This would strengthen the applicability for site-specific fatigue assessments.

Thank you for already suggesting literature. A discussion on this will be added in the revised paper, as there is room for more targeted discussion about model tuning after the bearing fatigue was removed.

7) Please expand the discussion: more turbulence often induces more pitch activity, increasing RCF. Explicitly address this link.

The chapter on fatigue life calculations will be removed from the paper, but the feedback will be taken in mind for the work about bearing fatigue.

Minor Comments

1) The manuscript is overly wordy, especially in the introduction. Restructuring into a clear Methodology → Results flow would improve readability and may also help with Comment 1. The introduction will be revised, being more terse and clear on the aim of the paper as well as its structure.

2) The absence of the 1st in-plane blade mode in high-frequency measurements (while out-of-plane is detected) should be explained. Could it relate to sensor orientation, turbulence characteristics, or other reasons?

The link between sensor placement and visibility of this mode will be discussed. The 1st in-plane mode induces drivetrain torsion, which does not result in a distinct frequency peak in the sensors, due to their orientation. No high frequency rpm measurements are available.

3) List all design parameters at the beginning of Section 2, ideally in a table.

A table will be added for a clear overview.

4) Section 2.1.3: Please clarify whether the actual turbine profile is available.

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, however, is already confidential itself. We see that this availability should be disclosed for the paper to be reproducible and will discuss with the corresponding parties.

5) Avoid “edgewise/flapwise”; use “in-plane/out-of-plane” consistently.

To follow the nomenclature advised in:

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

The paper will use edgewise and flapwise exclusively and clarify that the blades are at a 90 deg pitch angle at all times at the beginning of the section, to avoid misunderstanding.

6) Provide more detail on environmental measurement tools and placement relative to the turbines.

For the vibration data, IPC accelerometers are used 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 location on the drivetrain and the blades.

7) Line 439: The effect of mean wind speed should be mentioned.

The chapter on fatigue life calculations will be removed from the paper, but the feedback will be taken in mind for the work about bearing fatigue.

Technical Corrections

- 1) Line 92: "eingenfrequencies" → "eigenfrequencies"
- 2) Line 191: "Utlising" → "Utilising"
- 3) Line 221: "Similary" → "Similarly"
- 4) Line 278: "baldes" → "blades"
- 5) Line 288: "suDyn" → "SubDyn"
- 6) Line 384: "charachtersitics" → "characteristics"
- 7) In several places: "asymetric" → "asymmetric" (please search/replace throughout).
- 8) Line 292: delete extra 'the'.
- 9) Line 231: Please reword for clarity: 'The mode at 5a is identified as the 2nd symmetric in-plane rotor mode, while 5b and 5c are identified as the 2nd asymmetric in-plane rotor modes 1 and 2, respectively.'
- 10) Line 213: Clarify the phrase 'at least three poles are identified as stable in consecutive model orders.'