“Low-Frequency Fatigue monitoring using nacelle-mounted accelerometers”

General note

Dear reviewers,

Thank you for your review of our work. We fully acknowledge that after the first iteration our work needed additional love and attention.

In our revised version we have tried to motivate our decisions better, provided more context and describe our methodology in more detail. We have improved the methodology by introducing a yaw offset correction, and improved our results by doing additional analysis including one on all 5 fleet leaders and for one month of data.

We hope you appreciate our efforts. And we would like to thank all 3 of you again for your input, at least in our opinion the work has improved greatly from the previous iteration.

Kind regards,

The authors

Instructions

1. The reviewer comments are in **bold black font**.
2. Author replies are in Red font.
3. The changes made to the manuscript are marked in blue font and referred in this document in blue.

Reviewer 1

presents a practically relevant monitoring strategy in which nacelle-mounted tri-axial MEMS accelerometers are calibrated for sensor mounting offsets and permanent tower tilt and subsequently used to infer fore-aft bending moments and low-frequency fatigue loads.

The fleet-leader concept, the use of operational offshore measurements, and the attempt to obtain a low-maintenance farmwide fatigue-monitoring solution give the work clear engineering relevance.

However, This Reviewer considers that several aspects of the physical assumptions, calibration procedure, data processing, and fatigue validation require substantial clarification and additional evidence before the conclusions can be considered sufficiently robust and reproducible.

In summary, This Reviewer recommends reconsideration after major revision. The following points should be addressed:

1. **This Reviewer considers the extension from 10 min mean acceleration to the time-resolved 0-0.08 Hz signal to be the most important physical assumption requiring validation. A nacelle accelerometer measures both the gravity projection caused by tilt and translational/rotational inertial acceleration. Equation (6) is derived using mean acceleration, whereas Sect. 5 applies a 0.08 Hz low-pass filter and interprets the retained signal as a load-induced tilt proxy. The authors should demonstrate, using spectra, coherence/transfer functions, or an equivalent analysis, that inertial acceleration is negligible or properly accounted for over the retained band, and should state the frequency range over which the quasi-static relation is valid.**

We acknowledge the concern of the reviewer, and within our review we tried to motivate our decision to target to the low frequency content below 0.05Hz better. In this regard we added Figure 2 showing both a 12h timeseries and spectrum to better motivate our decision as well as rewriting our discussion. We acknowledge the 0.05Hz limit remains an arbitrary limit, mostly founded on a 'gut-feeling' this bound excludes the dynamic effects, such as rotor harmonics, first order dynamics and wave loading, while still catching the dominant lower frequency components that were of our interest.

In the manuscript the following explanation/motivation is add on page 4 line 63 to line 74:

"In this study we aim to develop a strategy that provides a quantifiable assessment of fatigue induced due to low-frequency fatigue across a fleet of turbines. As such we adopt a stricter definition of LFFD, focusing on loads with frequencies below 0.05 Hz. This threshold targets loads arising from slowly varying wind conditions and the controller/setpoints variations of interest. While excluding contributions primarily associated with rotor harmonics, with a lower bound of 0.06Hz when assuming a minimum rotor speed of 4 rpm, first-order structural dynamics, typically above 0.2Hz, and waves. This distinction is illustrated in Figure 2, which presents the fore–aft and sideways bending moment time series and their corresponding power spectral densities over 12 hours of continuous operation of one of the turbines used later in this study. As shown in the response spectra, restricting the analysis to frequencies below 0.05 Hz captures the dominant large-amplitude cycles that can persist over several hours, while excluding higher-frequency content associated with rotor harmonics (1P and 3P), the first-order structural dynamics (between 1P and 3P), and wave-induced responses. Although this cutoff is somewhat arbitrary, reflecting the absence of a widely established definition of LFFD, it provides a practical and consistent criterion for identifying low-frequency fatigue dynamics in the present study."

2. **This Reviewer recommends a stronger justification for the definition of low-frequency fatigue dynamics and for the 0.08 Hz cutoff adopted in Sect. 5.1. The statement that this band lies below the lowest cyclic loads, including wave loading, is not sufficiently demonstrated. The authors should show site-specific spectra of measured bending moment/strain and nacelle acceleration, identify the relevant 1P and wave-energy bands, and provide a sensitivity analysis showing how the rainflow spectrum and DEM change for reasonable alternative cutoff frequencies.**

   This aligns with the previous comment, and we fully acknowledge there is no strict definition what comprises low frequency fatigue. But nonetheless we wanted to pursue a method that helps operators to better understand if particular curtailment strategies affect their assets. As such we tried to define it better in a heavily rewritten first section. We hope our efforts resolve the concern of the author.

3. **The "zero-load" calibration assumption at 3.6- 3.9 m/s is insufficiently substantiated. The method assumes that horizontal wind loading counteracts the rotor-nacelle assembly eccentricity so that only sensor offset and permanent tower tilt remain. The authors should provide the fitting equations or objective function, the number of samples and yaw coverage per turbine, uncertainty or confidence bounds for the estimated tilt/heading/offset parameters, and sensitivity to the selected wind-speed interval and turbine state. Because Chaar et al. (2026) is central to this step but is cited without complete publication details, the present manuscript should remain sufficiently self-contained to reproduce the calibration**.

   The 'zero-load' calibration assumption at 3.6-3.9m/s has now been better illustrated, by explicitely showing this window in the bending moment curve of Figure 2b. Where the bending moment curve intersects with 0 at these wind speed. We explicitly mention this in the text as well:

Line 110 " As previously illustrated in Figure 2(b), the corresponding bending moment is approximately zero within this wind speed bin. "

For further details on the procedure to compute the components of the sensor calibration. We acknowledge that at the time of the first submission Chaar et al. (2026) wasn't publicly available yet , however now it has been published and is available open access:
https://www.ndt.net/article/ewshm2026/papers/EWSHM_2026_1587.pdf.

In our contribution to Wind Energy Science we wanted to avoid repeating all equations in that paper, which ultimately revolve around axis transformations and in are in a way trivial, and instead focus on the underlying idea and the use of the calibrated results in a context load monitoring. We hope the reviewer agrees with the balance we targeted.

We tried to mitigate the concerns about confidence by conducting the exercise in a second validation period. We quantified the differences we saw between the reference (=original) and validation periods to quantify the possible variability, and conclude that the differences between turbines far exceed the year-by-year variability of the test.

Finally we fully acknowledge the concerns of the reviewer about the the number of samples and yaw coverage per turbine. We judged that neither were a problem for the current site, but e.g. sites where there is little variability in yaw, e.g. Taiwan, might struggle to attain sufficient data in all wind directions. We added this reflection to the discussion.

Line 489- "Another consideration, shared with all purely data-driven methods, is the availability of sufficient data to properly conduct calibrations and model training. At this site, with strong variability in wind conditions, and sufficient data at hand this did not pose an immediate problem, indicated by consistent calibration results between the reference and validation periods. But at sites with lesser environmental variability, primarily in wind direction, the currently proposed strategies might struggle and caution is advised. "

4. **The sensors were installed manually without a dedicated mounting bracket, yet Sect. 2.2 assumes that the relative yaw misalignment between the sensor and nacelle is exactly 0 degrees and states that it cannot be resolved afterwards. Such an azimuthal misalignment would mix the fore-aft and side-side components and could bias the inferred pitch and bending moment. The authors should report the installation/alignment tolerance, quantify the sensitivity of the results to plausible yaw misalignment, and incorporate this third sensor rotation in the calibration if it cannot be shown to be negligible.**
We fully acknowledge this concern and in the review we tried to mitigate this concern directly by introducing a yaw calibration strategy. This was added to Section 4.1: Methodology.

   As this is novel to the current paper we included explicitly how this yaw offset correction improved our results, by including an overview of how the corrected yaw offset reduced the spread in nacelle roll across the farm in the Figure 6. Interestingly we found that the yaw offsets stayed relatively close to our initial assumption of zero mean and relatively small. The impact on sideways roll is significant (no surprise there as it also how the yaw offset is calibrated), but the on fore-aft pitch the effect proofed rather limited.
   We hope this update mitigates the previous concern of the reviewer.

Line 289- 330 "At this stage a methodology to resolve the hereto unresolved sensor yaw offset, θ_{offset} can be introduced. As a result of sensor installation tolerances, θ_{offset} , is location specific and unknown. To estimate θ_{offset} for each location it is assumed that when $\theta_{offset} = 0$, $\psi_{nacelle}(t)$ is purely driven by the horizontal thrust load, while $\emptyset_{nacelle}(t)$ is driven solely by the nacelle responding to rotor torque. At nominal power $P = P_{nom}$, i.e. Region 3 in turbine operation, these two drivers uncouple, with thrust load F_T decreasing with wind

speed v_{ws}: $F_T \propto P_{nom}/v_{ws}$, while rotor torque remains constant and linked to P_{nom}. θ_{offset} is found by isolating samples from Region 3 , $P \approx P_{nom}$, preferably with low variability in power .

The latter can be obtained by selecting data with a low ten minute standard deviation of power, e.g. $\sigma_P < 0.01P_{nom}$, An estimate θ_{offset} is found by minimizing the standard variation of the nacelle roll within this subset of data $\psi_{nacelle}(P \approx P_{nom}, \sigma_P < 0.01P_{nom})$ for the isolated data , within a realistic band of $\theta_{offset} \in [-10°, 10°]$. The found yaw offset is simply subtracted from the turbine's yaw angle as obtained from the SCADA. Interestingly it is worth pointing out that the found yaw offset does not account for any mismatch between the SCADA's North and the actual North."

5. **Please provide a more explicit assessment of transferability across turbines with different substructure designs, water depths, and boundary conditions. The mapping from nacelle pitch to tower-transition-piece bending moment is expected to depend on structural stiffness and geometry. The authors should report the fitted slopes/intercepts, or an equivalent measure, for the available fleet leaders and examine whether prediction error correlates with structural differences. The manuscript should clearly state the conditions under which a model calibrated on one or two fleet leaders can be transferred to an uninstrumented turbine and whether any turbine-specific calibration remains necessary.**
   We fully acknowledge this comment. To document the differences in the farm we included the new table 1. We also added a more in-depth study on fatigue with all 5 fleet leaders.
   However, there is not immediately a clear relation emerging between the errors observed and the location. This needn't be a surprise considering there is also fairly little spread between the bending moments curves and the pitch curves of the turbines in the fleet.
   Refer to line 132, the following table is added along with minor discussion.

Fleetleader	**Turbine 1**	**Turbine 2**	**Turbine 3**	**Turbine 4**	**Turbine 5**
Design cluster	Shallow - inter	Shallow	Deep inter	Shallow inter	Deep
Water depths [m]	24.1	23.3	27.7	25.0	32.4
Maximum monopile outer diameter [m]	7.4	7.2	7.6	7.4	7.8
Normalized first order sideways mode in idling $[f_{SS1}/f_{SS1,max}]$	0.99	1	0.98	0.98	0.96
Soil	Stiff clay over clayey sand transitioning to very dense sand.	Fine sand over stiff clay and clayey sand	Medium dense to dense sand over stiff clay	Stiff clay	Loose and fine sand over stiff clay
Support structure	Monopile with bolted transition piece				

6. **The fatigue-processing description insufficient for full reproducibility. The manuscript should define the DEM equation, reference number of cycles or equivalent frequency, reference duration, rainflow-counting settings, treatment of half cycles and residues, Wöhler exponents, any mean-stress correction, and the precise normalization applied. The values of the outer radius and second moment of area used in Eq. (8), or the basis for their normalization, should also be stated. In addition, Sect. 5.2 refers to stress range in MPa whereas Figure 9 is labelled "Normalized stress range"; dimensional and normalized quantities should be distinguished consistently.**

For this work we were somewhat restricted in the amount of information we are able to disclose, and as much as possible we wish to stay with normalized results.
Nonetheless we acknowledge the concern of the reviewer and tried to get more clarity in the normalizations employed. The procedure on how long term DEM was computed was also clarified. To make a more widespread analysis we computed the DEM results for m=3,4,5 which covers the typical values in substructure design.
To address this concern eq 14 and 15 were added. please refer to line 453 and 457:

$$\mathrm{DEM}_m = \frac{1}{N_{\mathrm{eq}}}\left(\sum_j n_j \cdot \left(\frac{\Delta\sigma_{Mtn,j} \cdot \frac{\pi}{4}\left({r_o}^4 - {r_i}^4\right)}{r_i}\right)^m\right)^{\frac{1}{m}}$$

$$DEM_{m,Normalized}{}^{SG} = \frac{DEM_m{}^{SG}}{DEM_3{}^{SG}}, DEM_{m,Normalized}{}^{ACC} = \frac{DEM_m{}^{ACC}}{DEM_3{}^{SG}}$$

7. **There seems to be an important numerical inconsistency between Table 2 and the conclusion. Table 2 reports MAPE values of 8.61-9.89 %, whereas the conclusion states that the total DEM is predicted with a margin of error below 3 %. These statements cannot both describe the same error measure. The authors should reconcile them and define precisely what "margin of error" represents. If MAPE and RMSE were computed over multiple subintervals rather than from the single aggregate DEM values shown in Table 2, the sample basis and equations should be reported; otherwise the use of "mean" and RMSE is unclear.**
This was indeed an inconsistency that had risen over various iterations of the draft paper. In this current paper the results were updated and the error metrics were more consistently defined. The error metric used for bending moment and stresses timeseries is MAE expressed according to the maximum value recorded by strain system. Hence, the metric is this form would express the mean absolute error relative to the maximum recorded value expressed in %. For the normalized DEM the error used is a simple percentage error between the measured and estimated value.
Line 345

$$\mathrm{MAE} = \sum_{i=1}^{n} \frac{\left|{\mathrm{M_{tn}}}^{\mathrm{SG,i}} - {\mathrm{M_{tn}}}^{\mathrm{ACC,i}}\right|}{{\mathrm{M_{tn,max}}}^{\mathrm{SG}}} \times 100$$

8. **The fatigue validation in Sect. 5 is limited to approximately one week and one fleet-leader turbine, whereas the broader motivation concerns long-term and farmwide fatigue monitoring. If additional data are available, a longer and/or multi-turbine fatigue validation would considerably strengthen the work. At minimum, the authors should report the number and duration of curtailment events, the distribution of operating states and wind speeds, and the missing-data fraction in the validation week. If broader validation is not available, the abstract and conclusions should explicitly frame the evidence as a short-term proof of feasibility rather than a fully validated farmwide fatigue-monitoring solution**.
We fully agree but were bound to some practical constraints. However, for the updated version we were able to extend the validation period to 1 month and this for all fleetleaders. The results were added to Table 4. The results also made us nuance a bit our findings, concluding the method shows potential but is not 'guarantee' for a perfect proxy for strain gauges.
Refer to table 4 on line 470

9. **This Reviewer recommends adding the sensor and acquisition details needed to assess low-frequency/DC performance. The manuscript should identify the accelerometer model or relevant specifications, sampling frequency, measurement range, noise density, bias stability and temperature**

sensitivity, together with strain-gauge sampling, time synchronization, resampling, and preprocessing. The implementation of the Butterworth filter should be specified, including whether it is causal or zero-phase and how edge transients are handled. These details are particularly important because low-frequency tilt estimates are sensitive to bias, drift, and timing.

We have made every effort to address this concern and improve the clarity of the methodology. In response to the reviewer's comment, we have also added Table 2 to the manuscript. While we acknowledge that not all of the reviewer's requests could be fully addressed, we hope that the revisions adequately mitigate the main concerns raised.

The main text has also been revised to clarify that the filtfilt function was used to achieve zero-phase distortion when applying the Butterworth filter. In addition, we have included a reference covering the general principles and design of the filter, as well as a reference illustrating its application in similar structural engineering contexts to obtain low frequency content.

10. **Structural health monitoring, condition monitoring, and fault detection have paramount importance in the growing wind industry. The proposed methodology can be most likely applied to wind turbine generators or other mechanical components. In this context, https://doi.org/10.3390/s22041627 recently reported on the current state of the art in this field. This can be added to the introduction of the paper to provide further context to this research.**

Thank you for the suggestion, we have updated our references accordingly

Line 22-30 "Wind turbines, particularly offshore installations, operate in harsh environments, making structural health monitoring (SHM) essential. In the past years, various strategies have emerged to continuously monitor key structural parameters to facilitate the management of these structures while reducing inspection costs, avoiding unnecessary maintenance, predicting potential failures, and ensuring reliable operation, thereby maximizing the return on investment (Civera and Surace, 2022; Martinez-Luengo et al., 2016; Martinez-Luengo and Shafiee, 2019). The technologies are diverse, encompassing acoustic emission, thermal imaging, accelerometers, and strain gauges, each with specific objectives, limitations, and applications. With guidelines such as VDI 4551(Verein Deutscher Ingenieure, 2020) providing a framework for monitoring and assessing the structural integrity of wind turbines and offshore structures. This research particularly focuses on investigating whether accelerometers, installed in the nacelle can be employed to monitor low-frequency fatigue of the structure."

11. **A final technical and language proofreading is strongly recommended. Examples include the reference to Figure 8 instead of Figure 9 for the rainflow histograms, "medium value" in the Figure 6 caption where "median value" appears intended, inconsistent "DEM/Dem" capitalization, "not to different" in the Figure 1 caption, the "0. 98" entry in Table 1, and the description of R^2 as correlation rather than coefficient of determination. The notation around Eqs. (2)-(6) should also be checked; in particular, the statement that only yaw needs to be known should be reconciled with Eq. (6), which still uses the previously estimated tower-tilt parameters.**

We've reviewed the manuscript thoroughly in the current iteration and hope the concerns of the reviewers have been mitigated.

Reviewer 2

Study titled “Low-Frequency Fatigue monitoring using nacelle-mounted accelerometers” presents a concept for low-maintenance farm wide fatigue-monitoring.
Due to its importance and relevance, this work should provide more clarification and verification on several points. In the text, the authors state in more than one place that there is some approximation and a lot of error included, but still, they still assess their model(s) as a success with “below 3%” error. I’m not sure I agree with that. Also, some of the equations need netter and more detailed explanation (like Equation 6 and 8. Finally, Conclusion section should address all shortcomings included in this work and mention all assumptions and rooms for error before addressing “error remaining below 3%”. I think the manuscript needs proofreading and fixing tiny typos as well.

Thank you for comment, in this iteration we have aimed to improve both the methodology, by including a yaw offset correction. Aimed to improve the description of our methodology, and the extended the study on the errors we were able to achieve. We also nuanced our responses a bit more, indeed sharing the sentiment that there is still room for improvement.

Some specific lines to clarify:
Line 119: is it Figure 2 or Figure 3? Please check.

We have reviewed that segment**.**

Figure 5: blue color should be less intense since it masks information (the mean)

This was updated in the figure 6 (previously 5).

Line 240: it would be nice to include a graphical representation of locations of all 5 turbines (and their position on Figure 5a)

Figure 6a (previously 5a) was updated with an explicit highlighting of the fleetleaders and the turbines used for illustration.

Line 255: “which could be attributed to similar boundary conditions” this needs more explanation (what is considered as a part of “boundary conditions”? This is why more info about location of 5 disused turbines would be useful).

We have added Table 1 to provide more details about the substructures and the differences between the fleetleaders.

Line 278: “ we adopt a fourth-order Butterworth low-pass filter at 0.08 Hz.” This part needs some reference and more explanation

We have rewritten this segment and provided more context on how the filter was applied. Highlighted in blue in the updated document.

Line 355: “with the margin of error remaining below 3%” I’m not sure I found anywhere in the text that errors are below 3%. Where is this coming from?

We acknowledge this inconstancy that originated from different iterations of the draft document. The error is coming from evaluating the currently figure 9 (a) timeseries.

Reviewer 3

The manuscript presents an approach for estimating low-frequency fatigue loads in wind turbines using nacelle-mounted acceleration measurements. It combines correction for sensor mounting offsets and permanent tower tilt with a calibrated relationship between estimated nacelle tilt and fore-aft bending moments for subsequent fatigue assessment. The use of operational measurements and a fleet-leader strategy gives the study practical relevance, particularly for monitoring turbines without extensive load instrumentation.

However, substantial revision is needed to clarity the methodology, improve the presentation of results, and align the conclusions with the support evidence. The current description does not sufficiently distinguish physical assumptions from validated relationships or demonstrated performance from potential applicability. This makes it difficult to assess the reproducibility of the approach and the extent to which the results support the conclusions.

The following points should be addressed.

1. **Improve the organization and focus of the introduction**

The introduction provides relevant background, but the proregression from the practical monitoring problem to the research gap and proposed contribution could be clearer. This discussion of non-uniform sensor installation is repeated across several paragraphs and should be consolidated to distinguish installation-related calibration issues from the broader challenges of farm-wide fatigue monitoring. The introduction should also clarify which challenges the proposed approach addresses and how its contribution differs from previous work.

We have reviewed the introduction significantly. Keeping your comment in mind we hope these updates resolved this concern.

2. Provide a complete description of the methodology

The processing chain is not sufficiently clear to allow readers to follow to reproduce the approach. Essential information is dispersed across references to previous work and the discussion sections. Please present the complete workflow in the methodology section, including the conversion of acceleration measurements to estimated tilt, the relationship between tilt and bending moment, and the subsequent fatigue calculations. The underlying physical assumptions should also be stated and justified, particularly the used of mean acceleration and the conditions under which a quasi-static interpretation is valid.

In the reviewed work we tried to clarify the procedure throughout the entire process. In particular we aimed to provide a better overview of the entire workflow at the end of Section 2.

The physical motivation to use the quasi-static assumption was worked out in more detail, mostly by the introduction of Figure 2 that shows in its spectrum how the filter of 0.05Hz isolates the low frequency effects from the dynamic effects residing higher.

3. **Clarify how differences between turbines are addressed**

The authors stated that 'the turbines are from the 7 to 10 MW generation and are installed on XL-monopiles at moderate water depths', suggesting that their tower structures may differ. Please provide the relevant tower and support-structure characteristics.

Where differences exist, discuss their implications for the calibrated tilt-bending moment relationship and how they are accommodated when transferring the calibration between turbines.

We provided extra context regarding the structural differences of the turbines in the fleet through the use of Table 1. Despite having some variability between our five turbines of study we didn't spot a one-to one link between the variability in our results and the differences between turbines. Section 2.1.

4. Improve the figures, tables, and definitions of reported quantities

Several figures and tables are difficult to interpret because the plotted elements, normalization procedures, or reported quantities are insufficiently defined.

> **Figure 1: Please revise the schematic's legibility and ensure that the relevant components and their relationships are clearly labelled and explained.**

We've replaced Figure one with a more cleaned up version made using a simple computation.

> **Figure 4: Please clarify what the dots represent and why the curve extends beyond the values they indicated.**

(Now figure 5) The caption of the figure was updated.

> Figures 7-9: Please define the residual in Figure 7 and identify the reference quantity used for normalization in Figure 7-9.

We fully acknowledge that the requirement to normalize our results introduces some confusion. In our update the normalizations were made more explicit. In general the normalization is primarily done with respect to the maximum value. For exception for the DEM.

> **Table 1: Please provide units for all appliable quantities.**

(Now table 3) This was update along with a more detailed discussion of the definition of these error metrics.

5. Present the validation results more comprehensively

The discussion considered only one of the 33 turbines, whereas the broader motivation concerns farm-wide monitoring. Comparisons across turbines and time periods are therefore needed to demonstrate the applicability of the approach at the farm level.

The results distinguish between different operational stages, but provide insufficient information on the operating conditions or their influence on the proposed approach. These aspects should be described and discussed

The current results target only operating conditions and we fully acknowledge the error is highly dependent on the exact conditions. In general this multi-dimensionality will make it hard to ever come to an all-enveloping conclusion.

However, we have extended our validation dataset to all 5 fleetleaders and extended the dataset to a full month. We hope that the extended results in Table 4 provide more context to our exercise.

The fatigue damage index appears to be calculated using a single-slope S-N curve. Using a realistic S-N curve, such as one recommended in DNC-RP-C203, would provide more fare comparison on tower fatigue behavior

than slope-sensitivity analysis alone, especially considering the substantial discrepancies in the high-cycle portion of the results.

The reviewer concludes correctly that we framed our exercise through the use of Damage Equivalent Loads/Moments rather than damages which indeed imply a single constant Wöhler slope. The use of damage equivalent moments is fairly common practice in the context of fatigue modelling of offshore wind turbines. To emphasize the possible impact of using different slopes we included in Table 4 the results for three different slopes m=3,4 and 5, which cover the range of values typically associated with primary steel design of monopiles. A methodology we employed also in some of our preceding work; Sadeghi et.al.

We acknowledge that for damage calculation DNV-RP-C203 often relies on a bi-linear SN curve. However, this also depends on the exact conditions, e.g. free corrosion is a single sloped curve. And through the use of a bi-linear curve results will vary depending on employed stress concentration factors and the exact point of the intercept (logA) of the specific SN curve used. In our experience framing the results in terms of damage does not particularly yield additional insight over the use of the damage equivalent moments for varying slopes. Meanwhile using a bisloped SN curve adding additional complexity and variability in the results.

For these reasons we hope the reviewer agrees with our preference to keep working with damage equivalent moment.

6. Clarify the limitations and revise the conclusions accordingly

The manuscript would benefit from a dedicated discussion of the assumptions, uncertainties, and applicability limits of the proposed approach. This should cover the calibration assumptions, the validity range of the quasi-static interpretation, the restriction to the retained low-frequency band, and the transfer of the calibrated relationship between turbines.

Throughout the work we tried to motivate our decisions better as well as providing additional context in the measurement data, instrumented locations, and our results. In the discussion segment we do repeat some of the concerns raised by the reviewers and we have dialed down our final conclusion.

Finally, a thorough revision of the manuscript's organization and scientific writing is needed, with particular attention to consistent terminology, complete definitions, and accurate references to figures and equations.

We have revised the document thoroughly as a whole. We hope the reviewer agrees with our amendments.