Herein the authors compile all comments of the reviewers and the respective answers. We like to express our gratitude to the reviewers for their thorough work. We highly appreciate the time spent on our work and are happy to see it improved by the reviews.

The authors uploaded a new version of the manuscript with all mentioned changes and also a version with highlighted changes for reference.

Anonymous referee #2

General Comments:

- The abstract could be improved by summarizing the methodology for reducing the number of load cases and stating the main results of the case study.

The reviewer is perfectly right, the abstract was missing these two important aspects. They have been added.

- The authors use many short paragraphs. Readability could be improved by rearrangement of paragraphs

Thank you, we have removed a few very short paragraphs from the document.

- At times, the authors refer to results/findings of previous studies and assume knowledge about these findings from the reader. I suggest checking references throughout the manuscript and considering elaborating on explanations.

The authors agree that such unexplained references are quite possible, as this works builds upon several preceding publications. We have re-read the entire manuscript to identify references to previous works:

Section 2.2 refers to two previous publication to explain the inclusion of the pitch angle as an independent signal. The previous explanation read: "who show the load distribution in a blade bearing to be dependent on the non-rotationally symmetric stiffness profile of the rotor blade."

We added an additional explanation " At constant blade root loads, different pitch angles can thus cause different load distributions on its circumference."

Section 2.5 refers to the blade modelling being done according to Menck et al. (2020).

We have expanded this brief mentioning by the following explanation: "with shell elements except for the root section which consists of 3D elements."

Further mentioning of previous works is included in the introductory section. The authors found however the explanations given in this section should be suitable for the purpose of this work.

- *At times, the manuscript reads like a project report, where prior knowledge about methodologies, procedures, and data sets can be assumed. I suggest checking the manuscript for understandability from the perspective of an uninformed reader and adding context where needed.*

The authors note a certain similarity to the previous comment and would like to point to additional explanations included as a reaction to it. Further, the authors agree a certain level of knowledge is required to get a thorough understanding of the findings presented in their work. The authors would argue this is an unavoidable consequence of this work touching several disciplines which on their own can rightfully fill several pages of detailed explanations. Aspects of FE-models of full rotors, the post-processing of these and their use for the fatigue calculations of structures can only be included superficially to keep the entire work within reasonable length.

Specific Comments:

- Line 12: It is unclear what bin counts refer to here.

The authors used 'bin counts' as a synonym for data binning. Data binning refers to a data processing which groups data points into value intervals (bins). We have replaced the first uses of this term with 'data binning' and later introduced the synonym in Section 2.4.

- Line 13: It is not clear what a reasonable degree of confidence means here. Can the accuracy and confidence of in the method be quantified?

A conundrum of the present work lies in the unmanageably high number of simulations necessary to get stress results for each time step of turbine simulation time series. Thus, a full quantification of deviation of the presented method to a detailed simulation is not possible for the authors. A reasonable degree of confidence is thus demonstrated with exemplary results of one rotor rotation of one turbine but cannot be fully quantified.

- Line 27: Do the authors mean at each time step of the aeroelastic simulation?

Yes, the input data are the output of aero-elastic simulations. We have added an additional introductory statement to Section 2.2 on the input data to clarify this.

- Line 29: A short explanation/estimation of the required computational time would be helpful for context.

Computational times heavily depend on the machinery used. Given a similar scenario as used in previous publications, computation times would span several years. Even with more powerful workstations it could take weeks, and such a timespan can hardly be considered acceptable in commercial design processes.

- Line 32: Is it assumed that pitch-bearing loads are influenced by each other or that they are correlated?

The authors wanted to point out how the loads acting on one pitch bearing can be transferred through the hub structure to other pitch bearings and thus influence the load situation of these.

- Section 2.2: In this section, the authors explain the signals used but do not explain the input data sets used. A clear overview of the used data sets and their origin would be helpful.

Section 2.2 is a general explanation on input data, whereas Section 3.1 then contains an explanation on the input data used for the case study.

- Line 99: Input data to FE-Simulations?

That is correct and we added 'FE' for clarity.

- Line 103-106: The meaning of this section is not clear. Are additional signals included or not? Why?

We have rephrased the leading sentence of this paragraph to "While the aforementioned signals suffice to build finite element simulations, the introduction of additional signals for the selection of load cases might render more meaningful relations between the load signals."

- Line 138: The paragraph lacks clarity and should be revised. What is time series file duration, to which simulations is it referring? The weighting procedure and statements about dependencies are not clear.

The authors have rephrased this paragraph and think it got clearer.

- Table 1: It should be clearly explained what the columns of the tables are referring to and what can be seen from the given numbers. Please also revise the caption.

The authors apologize for the lack of a clear explanation on the table. The updated manuscript version contains the following remarks:

The first signal is split into two and the second signal into three bins, resulting in a total of six bins. The first two columns contain a normalized bin range for the values. The third column contains the number of values within that range. The columns for the dependent *signals then contain their mean values ('mean') and standard deviations ('std'). In the context of the present work, the mean values are the input values of the FE simulations or all values of signal one and two in the respective bin range.*

- Line 145-150. To what data set is the table referring? The bin counting procedure is unclear for the uninformed reader, and explanations should be revised.

See above answer – the data table is just an exemplary output to clarify the data structure. There is no underlying data set for the values in this table.

- Section 2.5: This section could benefit from a brief explanation of the mentioned models to give the uninformed reader some context.

The authors added some explanatory notes on the models:
This work uses a bearing model as described in Graßmann et al. (2023) in a full rotor model. The model uses non-linear springs to represent the rolling bodies and connects these via force-distributed constraints to the raceways. The bearing rings including the bolt holes are represented with 3D-elements. The modelling approach is validated in Graßmann et al. (2023) with experimental results. The deviations of between measured and simulated tangential ring deformations are below 10%. Further validation is not part of the present work.

- Line 157: I'm unsure what "different scenarios" refer to here. A more detailed explanation would be helpful.

The statement on the circumferential position has been removed from this subsection for increased clarity. Further we have added detailed explanations on the purpose of FE simulations within the context of the present work.

- Line 163: Does this mean the above calculation for "missing signals" is not used in this case? Please clarify.

Yes, the data set for the case study already contains the signals of the blade azimuth angle and the hub center bending moments. This also allows for a later verification of the calculation method for the blade azimuth angle. The authors have clarified this in the sentence.

- Line 164, Figure 7: What can be seen from the figure?

This is the power curve of the wind turbine of the case study. The authors did not deem this to be a necessary figure in the first place, but during an initial review by one of the WESJ editors said editor requested this specific Figure to be in the paper.

- Figure 8: Please add legends to the plot. In the top row, which lines are moments and which are forces? A more detailed caption could also be helpful for interpretation of the figure. Are there specific findings the authors want to highlight showing the figure?

Figure 8 serves to give a first impression of the signals this work takes into account for the data binning. In the describing text, we have highlighted the characteristics of the signals. Some of them are more stochastic in nature whereas others follow sine-like behavior. The authors argue a legend to be of little use in the interpretation of the figure, however adding it has been discussed since it s first creation. We have now added it.

- Line 175: More context on the two commercial turbines data sets would be useful. Is it simulated data or measurements? What are the differences in design, rating, onshore/offshore, and the implications for the present study? It is mentioned that the data sets contain different load cases (production vs. extreme loads). What is the implication of that?

The purpose of adding these turbines is merely to shows the conclusions of the case study are not only limited to the reference turbine. Due to their commercial nature and existing confidentiality agreements, details on their design cannot be disclosed. The paragraph has been revised to explain on this situation.

- Figure 9: Please add legends.

The authors have added legends.

- Line 189: Is this an assumption, or is it known? Can a reference support this?

The authors have re-written the entire paragraph because it lacked claritiy and context.

- Figure 11 and explanation: It is hard to visualize which situation is shown here. Could a second plot showing the mentioned axis help?

We have updated the explanations and the plot accordingly.

- Line 201: This statement needs more explanation.

Indeed – the explanations have been extended.

- Section 3.3: I believe this section will become clearer with an elaborate methodology explanation in section 2.4. I suggest carefully revising it, considering an overview of investigated cases, scenarios, and turbines /data sets.

The authors both updated the Section on methodology and the introductory explanations in Section 3.3.

- Section 3.5: I think the azimuth angle calculation could be included in section 2.3, while the results can be shown in the results section. This would also allow a direct comparison between the results in Table 9/6.

The authors beg to differ at this individual point – the calculation of the azimuth angle is a practical side aspect of the methods as we encountered several data sets which did not contain this signal. But it is not inherent to the method itself and while we thought it of *value to any design engineer to have this tool available, we deemed it best to keep it separate from the other results and methods.*

Jonathan Keller

In this paper, the authors present a methodology for selection (or really, downselection) of load cases related to finite element analyses of a pitch bearing design. In general, the paper is well written and structured and presents a nice contribution on the topic of pitch bearing design.

The authors like to thank the reviewer for his kind words.

Lines 4-5: I believe this sentence could be written a bit better, as I'm not sure the "size" of the model or the (magnitude) of the operational loads matters. I think the sentiment being expressed is something like "However, due to the number of contacting bodies and operational load cases involved, it's necessary to..." Then again, in the case of a structure such as a pitch bearing, it's more like the small size and large number of contact patches compared to the large overall size of the bearing itself that is what we mean by "size" of the model. But, I think the provided sentence gives the proper gist of things.

The reviewer is perfectly right, this sentence falls a bbit short of what the authors wanted to explain. We have replaced 'size' with 'complexity' and added a 'number of' in front of the operational load cases.

I will admit the large number of very short paragraphs typically of 1-3 sentences in Sections 1 through 2.1 make things a bit hard to read. I recommend a little effort to group like paragraphs together. For example, the paragraphs in lines 16-43 and then separately 44-56 can all be combined into a single paragraph. Sections 2.2 through 4 are much better.

We have regrouped paragraphs, thank you.

Figures 1 and 2: If possible, I believe it would be helpful for the reader to label each box that corresponds to the descriptions in the text. For example, in Figure 1 it might be "Load time-series", "Rainflow counts", "Rotor model", "Finite element analysis", and "Stress calculations".

This seems a good idea – we have added labels to the boxes.

Lines 72-74: I think the second version of the sentence is written more clearly than the first, honestly, as I don't think we're talking about "signals". It is most easily cleared up by changing the first sentence to "...to which the loads on a pitch bearing are affected by not only the loads on the blade to which it is connected, but also the loads from the other blades as transmitted through the hub.” I’m not sure if the second sentence is then needed, or maybe the first can be deleted and just the second kept.

The reviewer is right in the to sentences having almost identical content. The authors have revised the first and deleted the second.

Although nice to have, I believe Figures 3-5 could be placed side-by-side or otherwise condensed a bit more, as 3 independent figures here seem a bit overkill. I suppose that’s just a preference thing though. I would label the primary and secondary blades in Figure 5 (or some version of it) though.

The authors found the idea of condensing the COS into one Figure very appealing. The azimuth angle is kept in a separate figure.

Lines 103-106: I am curious as to the message intended here. Everything is pretty cut-and-dry to this point, but these sentences beg as may questions as they answer, or maybe they don’t answer anything at all? It almost feels like it’s a description of future work that might be included in a conclusions section. I don’t have any more specific comment here, but rather I’m just generally a bit puzzled as to what to make of these sentences.

We have re-written this description and yes, it was a bit confusing.

Section 2.3: I’m also a bit confused by the title of this section and the description of “missing” pitch angles. I believe what lines 111-115 is saying is that the pitch angles of the secondary blades can simply be assumed the same as the primary blade. In that respect, it’s not missing, so it feels like this statement is better suited in the previous section. Similarly with the hub moments, as these are best derived from the blade root moments. I guess I also don’t understand how the blade azimuth angle would be “missing” from a simulation. Overall, I believe some additional context would be helpful here, or my other suggestion would be to just combine this with Section 2.2. I really must be missing something, as later on I also don’t seem to understand the value of Figure 12.

The entire concept of recalculating signals from time series is thought as a tool for design engineers who have a data set available which does not have all the signals mentioned above in it. This might be time series without the azimuth angle for example, and a realistic scenario might be a turbine manufacturer transmitting aero-elastic output data to a bearing manufacturer with some of the information not contained. In such a case, it might be far more convenient to calculate the signals instead of having another loop of data transfer.
Also, more related to scientific research: For the purpose of this study we have re-used data sets from other projects, partly from commercial turbines not longer in the design phase. It would have been rather impossible to convince anyone in the turbine simulation department to do a new output of the data, so we calculated some missing signals on our own.

Figure 12 then shows how the calculation is valid as the azimuth angle originally contained in the time series data and the calculated one are very similar.

Table 1: I will admit I only "sorta" get what is being described here, but also sorta not. I was also puzzled by the statement "The results are arbitrarily chosen". It feels like a better use of some text would be to maybe show an example real result for the IWT7.5 in an Appendix, if such a result would be too long in the main body.

We have rewritten the entire description of the Table because it sorta was lacking a lot of content.

Figure 8: It's a bit hard to interpret as is. I see in the second row that the moments are thick lines and the forces thin ones. But I don't see any thick lines in the first row? It also looks like there are two sets of thin lines in the first row, one solid and one dashed, but the text says only one force? It also feels like 30 seconds is more than needed, something like 20 seconds (or 3 revolutions) is probably sufficient. I might also suggest expanding the plot vertically, or plotting on a 2x2 grid rather than a 4x1 grid to make the plots more visible.

There was a slightly confusing choice of markers and lines in the visualization. We have redone this and added a legend.

Figure 9: What do the vertical yellow lines indicate?

120° steps or the distance between two rotor blades. We have added an according description to the text.

Figure 11: Is currently quite small and should be increased in size, especially the font size.

Thanks!

Minor typos:

Line 7: I think it should be "...interface parts" or "...interfacing parts".

Thanks!

Line 34: I believe the citation style here should look like "Chen and Wen (2012) simulated..."

Thanks!

Line 313: I believe the volume, issue, page numbers and doi are missing from Chen and Wen. 134 (4): 041105, https://doi.org/10.1115/1.4007349.

Thanks!

Line 134: Missing space between sentences.

Line 186: I think what is being described here is the “bolt holes”, instead of “bore holes”. Similar in line 187. Maybe this is just me though, and “bore hole” is common in the pitch bearing community.

It might also be something specific to German pitch bearing engineers, but yes, the term bore hole is frequently used in this context. Considering it, bolt hole as more precise and we have changed it throughout the document.

Line 191: I’m not sure what the “As axial location” here indicates.

Neither are we. We have removed it.

Line 222: I believe it should be “Both moments had positive effects…”

Indeed.

Line 292: I believe the citation style here should look like “…based on in Becker and Jorgensen (2023).”

Thanks!

tkre

Line 55: “In particular for comparably weak hub structures, Becker and Jorgensen (2023) stated the need for multi blade bearing FE-simulations in particular for pitch-bearing ring fatigue which may be overestimated when applying 1/3 hub cyclic symmetric boundary conditions. However, the ring damage failure mode may be driven by a combination of (mainly) edgewise, but also flapwise load components so that in general -dependent on turbine size, wind dynamics etc.- any combination of both should be analyzed for a reliable component fatigue assessment.”

Thanks for this detailed explanation on the findings from the 2023 work. We have added the leading statement of weak hub structures and referred to the need of taking into account load combinations for comprehensive assessments.

Line 61: ““Becker, Maier, Wächter et al. (2024) have shown how to apply and modify the FKM guideline for static and cyclic bearing ring strength approval compliant with IEC 61400-1 and DNVGL-St-0361 requirements.”

Thanks for pointing to this additional reference. We have included it in the text.

Line 95 : Becker, D., Burtchen, M., Handreck, T., Lüneburg, B., Müller, P., Neidnicht, M., Rollmann, J., Schlüter, D., Stellmach, S., Volmer, G., Necker, A.: Pitch bearings for multi-MW wind turbine applications – advanced multi-bearing calculation process and product development trends regarding pitch system modularization and hub standardization, FVA BEARING WORLD, Würzburg, 2024

We have added this reference.

Table 11: This section does not clearly indicate whether the load classes (bins) are determined as equidistant with the same class ranges for all rotor angle positions, or if the load class ranges are adjusted based on the current rotor position class. The load component Mx includes the weight component, which is dependent on the rotor orientation. In this context, it may be beneficial to adjust the classes according to the rotor position.

We have included an explanation on the bins being equidistant and an outlook to future works which might include bins of different lengths. Thanks for this interesting idea!

Nikolay Dimitrov

I would like to complement the existing reviews of the paper by several additional comments (so-called review by editor):

General comments

- It is positive that the authors consider the potential interaction between multiple blades. However, I believe in their current setup the authors have omitted the discussion of a potentially important part of the problem, which is the way the hub is modelled in the aeroelastic simulations. This model is very often simplistic with little or no flexibility in the hub assembly. Please discuss the way hub deformation is modelled in the aeroelastic simulations and if that may have an effect on your calculations and conclusions.

The reviewer is right about possible interactions between deformations of the uptower structures, specifically the hub, and the blade aerodynamics and loads. Deformations of the hub can change the orientation of the blade root and then entire blade which in turn can change the inflow conditions and also the lever arms for bending moments. The authors see this issue as relevant and worth a visit in future works. With respect to the present work, the authors would suggest refraining from mentioning said aspects in the paper for two reasons:

1. *The method itself does not rely on the way the aero-elastic model is set up. It can use any input data in form of time series. Even if significant changes in behavior occurred, re-doing the analysis for different signals will render results. But the authors do not expect any significant changes in the general behavior of the loads, because basic relations such as the*

gravitational loads influencing the edgewise bending moments will still exist even with a more detailed aero-elastic model.

2. *The paper already covers several aspects from data analysis to finite elements simulations which are strictly necessary to give a good picture of the idea behind it. But it feels already like a lot to digest, especially for engineers and scientists not familiar with the topics. Adding another aspect will inflate the structure of subsection even further and might confuse readers more than spreading additional knowledge.*

- It seems to me that with their selection algorithm, the authors are doing a sort of ad-hoc analysis of variance or variance sensitivity analysis (i.e., considering how much the variance of a given input variable contributes to the variance of the target value). Maybe this can be formalized by linking the theory with some sort of statistical test (F-test may be relevant?) or with variance-based sensitivity analysis?

The limited understanding of the authors of statistical tests is that these serve to verify a hypothesis, i.e. a mathematical function. The presented methods are able to function without the need of a prior hypothesis (besides the idea of which signals to use). The authors however agree using hypotheses and statistical tests might be an equivalent approach for signals dominated by gravitational forces such as M_x. They are, however, not helpful for more stochastic signals like M_y because they would need a Fourier transformation with many components as a functional expression, and it is not certain that such a function can be defined for an entire set of aero-elastic simulations.

Specific comments

- Page 4, line 83: The authors state that "the pitch angle is positive in a mathematically negative sense". This is unclear. Maybe the authors mean that the angle convention is opposite to the typical right-hand rule, or opposite to the trigonometric convention (anti-clockwise positive)?

Thank you! Yes, the authors meant to say it is opposite to the right-hand rule. We have altered the explanations

- Page 4, line 85: Authors point out that the flapwise/edgewise coordinate system rotates with the pitch angle. One way to refer to the moments which do not rotate with the pitch angle is by calling them an "in-plane blade bending moment" and "out-of-plane blade bending moment" with reference to the rotor plane. I can see the authors use the in- and out-of-plane notation later in the paper, so why not already here?

*What we tried to point out is a small laps in wording commonly used in description of blade root loads: While the pitch bearing coordinate system does *not* rotate with the blade, loads in this COS are still called as flapwise and edgewise although strictly* *speaking they are when the pitch angle is different from zero degrees. Does this make sense?*

- Page 7, line 135: Multipliers: this is normally referred to as probability weights?

We have added an additional explanation on the term in Section 2.4

- Page 8, section 2.5: It does not get clear what part of the structure is modelled by FE simulations. Is it only the bearing, or also the hub/blade? The reference Graßmann et al. (2023) only considers bearing modelled in a test rig. This brings the discussion back to my general comment on how and if hub deformation plays a role, and if the modelling approaches used in the paper can capture that.

The authors have added several additional explanations to Section 2.5.

- Page 11, line 191: Unfinished (or redundant) sentence: “As axial location”.

The authors have revised the entire Section 3.2.