We thank the reviewer for the thorough and constructive feedback. Below, we address each point in detail and indicate the corresponding changes in the revised manuscript.

In this paper, the authors present a methodology for calculation of the probability of static overload of a wind turbine pitch bearing based on exceedance of the static safety factor as determined by the recently published pitch bearing design guide (Stammler et al 2024). Appreciable revisions have been made based on reviewer comments to the initial submission, but my biggest trouble is with understanding the comparison between IEC classes and the 13 real sites. A close second to that is the importance (or not?) of the treatment of uncertainties, which comprise a major portion of the methodology (5 pages or 25% of the manuscript) but almost none of the discussion of results. I offer the following comments for consideration that could still improve the paper.

Title and terminology

- I still believe there are a number of terms that are used loosely and vaguely throughout the title and text, including "reliability assessment", "probability of failure", and "probability of damage" (all 3 are used in the text). I understand the authors' perspective coming from a structural reliability standpoint; however, I still feel that the most accurate description of the work (and a suggested Title that is truly reflective of the work) is "Assessing the probability of static overload of wind turbine blade bearings considering turbulence, design, and manufacturing characteristics". To me, a "reliability assessment" would examine all possible failure modes, including rolling contact fatigue, static overload, core crushing, and wear. "Probability of failure" and "probability of damage" of course are closer to the described work, but a static overload that causes an indent in the bearing is by no means assured to cause a failure of the bearing. If the authors would like to explain in the text that the methodology for this probability assessment is based on structural reliability assessment methodologies, then that is certainly understandable. Finally, I just don't believe that "extreme wind conditions" in the title (but "ultimate limit state" in the Abstract and most of the paper) isn't even all that important compared to considering turbulence, material, and manufacturing characteristics and uncertainties *which are far more novel aspects of the work and comprise the majority of the manuscript*. I respect the author's desire to pick their own title, I'm just being honest that as-is it does not convey why this work matters to those who care about the design, manufacture, and selection of blade bearings. I will say that the Abstract is a much better reflection of the work than the Title – with the caveat that I'm still not entirely sure comparison of IEC wind conditions to the actual sites is really "apples to apples". Please see later comments.

  Regarding the use of terms such as "reliability assessment," "probability of failure," and "probability of damage," we agree that these should be used consistently and accurately. In the revised manuscript, we have taken care to clarify that the study focuses on the static overload limit state, and that this represents only one potential damage mechanism in blade bearings. The phrase "reliability assessment" is now explicitly framed as a structural reliability approach applied to this specific limit state, not to bearing reliability as a whole.

  In line with the reviewer's comment, we also revised our use of "probability of failure" and "probability of damage" to reflect a single overload criterion, and we use this terminology more consistently throughout the paper.

As for the title, we have carefully considered the reviewer's suggested alternative. We have revised the title to the following:

"Probability Assessment of Static Overload in Wind Turbine Blade Bearings Considering Turbulence, Design, and Manufacturing Variability"

Introduction

- Line 25: I appreciate most of the modifications here, but the new sentence "Because these modes initiate in the same high-stress regions, the static-overload reliability analysis developed here directly addresses the most critical damage mechanisms for oscillating blade bearings" goes a bit too far and probably appears too early in the text. Based on field experience, I would say ring fracture is the most critical failure mode. I do agree it is almost certainly related to the maximum ball load Qmax. Therefore, that portion of the analysis described in the paper is relevant, but the factor R is not well understood for this damage mode compared to the 4,200 MPa for static overload. I believe a sentence more like "Extreme applied loads, ball loads, and bearing material and design parameters are likely related to many pitch bearing failure modes, so the methodology presented here to assess the probability of static overload given their uncertainties could be tailored to many pitch bearing failure modes." Such a sentence, though, is better suited later in the Introduction.
  The section was modified and moved to a later part of the introduction.
  "Many of these failure mechanisms initiate in high-stress regions associated with maximum ball loads. While this study focuses on static overload, applied loads and bearing parameters that drive this mechanism are also relevant to other failure modes."
  "While the analysis is tailored to one specific failure mode, the approach—based on uncertainties in turbulence, geometry, and material strength—could be extended to assess other damage mechanisms that are similarly influenced by maximum loads and bearing design characteristics."
- Line 48-56: I think this list of standards is confusing and missing context. They immediately follow a discussion of pitch bearing reliability, so the reader will assume they all directly relate to that subject:
  - I recommend adding what IEC 61400-1 clause 9.8.4 requires of the pitch bearing design, not just generally for components.
  - I recommend adding a short description of the NREL DG03 (Stammler et al 2024) requirements that add to this. I also recommend adding stating in the next few years the NREL DG03 will be replaced with the newly proposed IEC 61400-18.
  - I recommend adding that IEC 61400-8 currently does not explicitly contain pitch bearings in its Scope. That's not to say it might not be valuable – it could be and could be referred to by IEC 61400-18, much like IEC 61400-4 for gearboxes refers to IEC 61400-8 for their structural components.
  - I understand better the references to "offshore support structures" and ISO 19900 series from the authors response; however, I still do not see how a "support structure" (i.e. foundation) pertain to a blade bearing compared to those mentioned above. Without further explanation from the authors how they see that ISO 19900 relates to a

pitch bearing, I recommend these be deleted. It is striking that these are mentioned, when standards directly related to the pitch bearing are ignored.

  - "The standards didn't set reliability targets for machinery components" is stated twice in lines 55 and 56. Although this is true, I recommend this be tailored to what the standards do or don't say about the pitch bearing as described above, as that is the subject of the paper.

  The text was replaced by the following text:

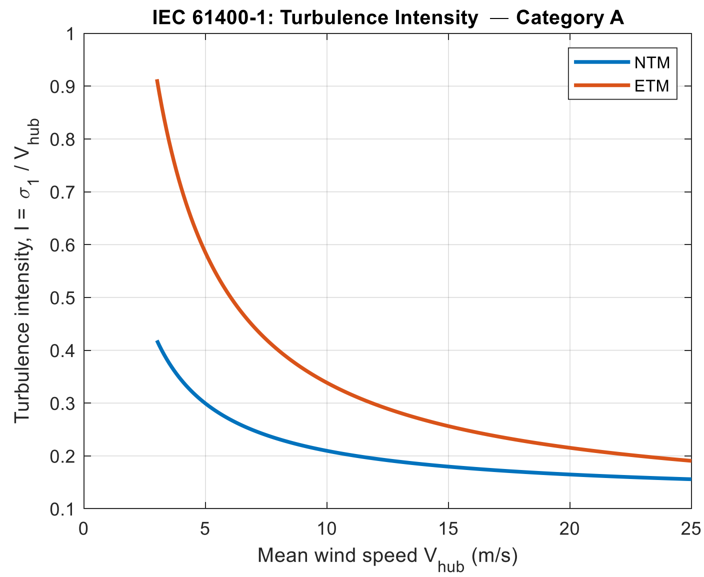
  "The design of blade bearings is governed by a combination of turbine-level and component-level standards. IEC 61400-1, Clause 9.8.4, requires that blade bearings demonstrate a minimum static safety factor against permanent deformation in the ultimate load cases. This requirement is based on limiting the local contact stress between the balls and raceways to a threshold value. DG03 expands on this by recommending a contact stress limit of 4200 MPa, based on ISO 76, and defining a methodology to evaluate loads, contact geometry, and bearing strength. DG03 is widely used in the industry but is expected to be superseded in the coming years by the proposed IEC 61400-18, which will standardize pitch and yaw bearing design. While (IEC 61400-8, 2024) provides structural design guidance for nacelle and hub components, it does not explicitly include blade bearings within its scope. Nonetheless, it may become a valuable reference if adopted in future standards such as IEC 61400-18, just as (IEC 61400-4, 2025) references it for gearbox structural components."

- Lines 56-69: The first sentence "The current paper studies the reliability of the blade bearing at ULS, with a deeper focus on the effect of the wind" misses much of the content of the manuscript and far understates the novelty of the work. As mentioned earlier, it doesn't fully "study the reliability" as it focuses only the probability of static overload. "The effect of the wind" is a relatively vague expression, compared to how the manuscript treats uncertainties with wind (i.e. turbulence), load, material, and manufacturing parameters that are far more interesting. The last sentence "Moreover, a sensitivity analysis on the effect of the bearing's main parameter on the probability of static failure of the blade bearing was performed" is very hard to understand. What is "the main parameter"? Please refer to my comment on "Title and terminology" and line 25. The novelty of this work is in the method (or framework) to assess wind (extreme or otherwise), load, material, and manufacturing uncertainties on the probability of static overload. The method described here could be directly relevant to or applied to other far more interesting failure modes, potentially such as ring cracking, given sufficient understanding of the variable R for each of them. Static overload is just a convenient illustrator for the purposes of the paper, as R is basically = 4,200 MPa. This whole paragraph is really quite important, as it sets the stage for the paper and it simply isn't well constructed currently. I think it goes too far when it attributes all failures to static overload, as generally implied here and specifically stated previously in Line 25.
  The text replaced by the following text:
  "This paper analyzes the probability of static overload in a blade bearing at the ultimate limit state (ULS) using a structural reliability framework, with a deeper focus on the effect of the wind conditions—particularly turbulence intensity—as well as uncertainties in bearing loads, material strength, and manufacturing tolerances. The goal is to quantify the probability that

static contact stress exceeds a specified limit, not to assess all failure modes. While static overload is used here as a representative limit state due to its clear stress threshold (4200 MPa), the methodology is generalizable. Extreme loading, combined with uncertainty in geometry and material properties, is relevant to other critical failure modes—such as ring cracking—if the appropriate limit-state definition is available. A sensitivity analysis is also performed to identify which parameters (e.g., raceway conformity, ball diameter) most strongly influence the probability of static overload."

2.3 Wind sites

- In Table 3, the reference wind speeds and reference turbulence intensities are shown for classes A+, A, B, and C and the 13 actual wind sites are only mentioned. No corresponding characteristics are given for the 13 sites (other than pointing the reader to Rezai and Nejad, 2023). Later, however, the most relevant information for the 13 sites is described in Section 3.3 (titled Description of DLC). There, "extreme" turbulence intensities are listed, but I am not sure I understand what an "extreme" TI is. Is it just the calculated TI at the site as described by IEC 61400-1 (i.e. ratio of the wind speed standard deviation to the mean wind speed), which for most of these are higher than standard IEC classes (and thence "extreme")? I recommend that Table 6 be moved from Section 3.3 to Section 2.3. Or am I entirely missing something? Why is the description of the TI at the sites, which is an important part of the analysis, not in the Section 2.3 Wind sites? Is there something to "extreme" TI, compared to the reference TI? This becomes important later in Section 3.3 where the DLC and turbulence models are introduced. It's even more important in Section 4 and 5 that describe the effect of turbulence on the probability of failure.
  The extreme turbulence value table is moved to section 2.3.
  IEC 61400-1 defines two different turbulence models: Normal and Extreme. In IEC 61400, the normal turbulence model (NTM) follows equation 10, but the extreme turbulence model (ETM) follows equation 20. ETM is higher than NTM because it represents extreme and rare cases and therefore exceeds NTM across all wind speeds. The reference TI commonly quoted for turbulence classes is defined at Vhub = 15 m/s under NTM, and it differs from extreme turbulence. The plot of ETM and NTM for IEC class IA is shown below.



In this study, extreme turbulence intensity means the maximum turbulence intensity in each wind speed range at the site, and extreme turbulence intensity for the IEC wind classes is directly calculated by TurbSim.

3.1 and Figure 1

- Line 95: Here it is stated "In the next section, the procedure for each step is presented" when referring to Figure 1. I appreciate the inclusion of a procedural figure; however, I don't see in the paper where Steps 1-4 are discussed, at least not explicitly (i.e. Step 1, Step 2, etc). It is generally understood from Sections 2.1 and 3.3 there is a model of a turbine used to calculate blade loads referred to in Steps 1 and 2. Note that the blade loads in Step 2 (N, L, D, C) are different than the loads in Equation 4 (Fr, Fa, M). The FEM and MBS models referred to in Step 3 are not used in the procedure, while I honestly can't tell what Step 4 is at all (the figure is not legible), but I don't believe it's represented in the paper. As described in Section 3.2, the blade root loads are used to determine the maximum ball load using Equation 4, and thence the maximum Hertz stress in Equation 2 and the static safety factor in Equation 1. Honestly, this figure misleads the reader. I recommend deleting Figure 1 or significantly revising it to relate to the methodology of the paper itself. A figure showing a pitch bearing, the forces acting on it (Fr, Fa, M), and relevant bearing dimensions (Dpw, D, alpha) and contact properties (f, a, b, Qmax, and sigmamax) would be far more relevant here as these are actually discussed in the paper.
The figure has been revised to present the forces on the blade bearing, relevant bearing dimensions, contact properties, and static safety factors. A description of each procedure is presented after the figure.

3.2 Safety factor and failure function

- Line 103: With the recent revisions, the sentence "In this regard, the static safety factor (S0) is the ratio of the allowable ball load to the actual ball load (Harris et al., 2009)" appears to be out

of place and contradictory to Equation 1, which immediately follows. I recommend it be deleted.
The text was removed to avoid redundancy and potential contradiction.

- Line 150: On the surface, the sentence “Uncertainty in dimension has an effect on the dimensions of the contact area” seems self-evident. However, I believe the intent is that the uncertainties in pitch bearing design dimensions, such as pitch diameter, ball diameter, contact angle, and groove conformity, affect the dimensions of the contact area a and b. Please clarify. This is then further described in Section 3.2.2, so I believe I am correct.
The statement is correct, and here the dimensions refer to bearing design parameters. The text has been revised as follows to make it clear.
“The uncertainties in blade bearing design dimensions, such as pitch diameter, ball diameter, contact angle, and groove conformity, affect the dimensions of the contact area a and b”

3.2.3 Uncertainty in loads

- Line 216: Here Vr must be defined. I believe it means rated wind speed (i.e. 11.4 m/s) from Table 1.
V_r denotes rated wind speed (i.e. 11.4 m/s). The definition of V_r is added to the text.

3.3 Description of DLC

- Line 240: I’m not sure I’d say “The DLC 1.3 contributes to an extreme turbulence model (ETM)”, I think rather “uses” or “includes” is a more accurate statement.
The phrase is corrected accordingly.

- Line 242 and Table 6: I am not sure I understand the transition between the discussion of DLC 1.3 and the site characteristics in Table 6 and “extreme” turbulence intensity. As mentioned earlier, why is this Table not in Section 2.3? Are these extreme values calculated differently than normal and thus not comparable to reference TIs? For 10 m/s for instance, the values of 0.65 and 0.87 for 2 of the sites are extremely high. It is no surprise then that later these sites lead to a higher probability of failure compared to a reference turbulence intensity of 0.16 for IEC 1A.
The table moved to section 2.3. As stated previously, the calculation of turbulence intensity is the same as normal turbulence intensity, but these values are extreme cases of turbulence intensities of one year of wind data. As mentioned before reference turbulence intensity referred to the wind speed of 15 m/s in the normal turbulence wind model. For example, as shown in the figure related to the comment on Section 2.3, normal turbulence of wind speed 15 m/s in IEC 1A is equivalent to 0.18, while its extreme turbulence is 0.26. For wind speed 10 m/s, TI in NTM and ETM are 0.21 and 0.34, respectively. The values of 0.65 and 0.87 in the sites are extremely high, and we accept that it is the main reason for the high probability of failure. These

values show the importance of considering the real wind site condition instead of IEC standard value for ETM.

4.1 Probability distribution function

- Section 4.1 and Figures 3 and 4: the text is extremely small. Please recreate the figures with larger text. A good rule of thumb is as large as the main body text in the document. I will also admit that I have trouble following the narrative here, so I'm not entirely sure I understand what is happening. Overall, when I compare the distributions of 15 and 3,000 seeds in Figure 3, they appear relatively similar. Later, 300 seeds are settled on from the trend in Figure and Table 7 (although if I wanted to argue 200 looks fine as well). This appears to be the net conclusion of this section. Overall, it could be simplified I believe. Not being an expert in this area, I really have trouble understanding this section and how this relates to simulations of DLC 1.3 and the resulting annual probability of failure.
The figure was recreated with a larger text. Different seed numbers were studied in order to obtain a suitable seed number. Each of these seed number sets represents a probability distribution. By increasing the seed number, a bigger number of realizations is created, and the accuracy of the result is higher, but on the other hand, the simulation time will increase. In order to quantify the process of seed selection, it is important to quantify the goodness of fit of the distributions and the convergence of the results. This section is responsible for quantifying the selection of a suitable seed number. A text was added to this section to clear our intention.

4.2 Sensitivity analysis

- This short paragraph simply says "onshore…1A". As mentioned earlier, I believe it would be valuable to restate this was for DLC 1.3 along with some discussion of how frequently these conditions occurred, as the probabilities of failure are given on an annual basis. I believe this relates to Section 4.1, but I'm really not sure.
The paragraph is revised.
"The probability of failure in the bearing with variation in the ball diameter, pitch circle diameter, conformity, and contact angle is studied. The onshore wind field with an extreme turbulence intensity grade of IA according to IEC 61400-1, DLC 1.3, is considered. 10^8 samples were considered in the simulation with the Monte Carlo method, and this process was repeated 20 times."
DLC 1.3 is an extreme and ultimate load case, and it does not happen frequently. In our study, the values of turbulence intensity of the wind sites are the maximum for one year; consequently, they could happen once a year.

- Beginning here in Sections 4.2.1 through 4.2.4 and Figure 6, I will admit having difficulty in understanding previous discussion of uncertainties χd, χf and χm and the variation in the probability of failure. To be honest, from Figure 6e what I glean is that other than the lowest groove conformities, the *uncertainties* do not have an appreciable effect on Pf. Is this a fair assessment? I wonder what to make of that? Nothing is offered in the text, which really only

focuses what happens over the range of mean values (which, I must note, is different than the uncertainties). It is no real surprise that when mean values change, the static capacity, static safety factor, and probability of failure all change similarly. So...is the final conclusion that the treatment of uncertainties that comprised several pages of the manuscript effectively unnecessary? Or am I missing something?
χd, χf, and χm, are the distributions that are used for the variation of each of the dimensions, force, and material. Indeed, uncertainty in the three main parameters of ball diameter, pitch circle diameter, and contact angle does not have a significant effect on pf, ball diameter has an effect, but not significantly, while the reliability of the bearing can change from 10^{-8} to 10^{-2} within 5.5% changes in the groove conformity. This effect is important as it shows that with small changes in this value, the probability of failure increases or decreases. This phenomenon is considered for the calculation of the probability of failure in all the other calculations for the IEC case and wind site, as stated in Section 4.2.3.

4.2.3 Raceway conformity

- Line 287: The sentence "Consequently, the uncertainty of the raceway conformity with normal distribution with a standard deviation of 0.5% for the uncertainty of dimension, χd, is considered" seems to be just a restatement of the analysis parameters, rather than new information. But why are χf and χm not mentioned? Here, I would be interested in discussion of the effect of the uncertainties as they at least have some effect on Pf at low groove conformities.
  In line 287, it is stated that only groove conformity is assumed in further simulation as an uncertainty of dimension because it has a dominant effect. χf and χm are defined in Table 6 and stay the same throughout the whole paper; therefore, it is not mentioned again. Otherwise, they should state in every section what is redundant to our understanding. The following text is added to show which variables are considered as an uncertainty in dimension.
  "*The initial contact angle, pitch circle diameter, and ball diameter do not have a significant effect on failure probability and are not considered in the paper as an uncertainty variable regarding dimension*."

Figure 6

- This is an important plot, but again the text is very small. Please increase font size.
  The font size is modified.

- Vertical error bars are used to indicate some range or distribution characteristic (maybe standard deviation) of Pf around each mean value. What exactly do the vertical error bars indicate? I don't believe this is stated in the text. Is this the max and min? Or a standard deviation?
  The error bars are related to the max and min values. The definition of the error bars is added to the text.

- I’m not sure I would call Figure 6e as “combining a to d” as each one is still plotted individually, Figure 6e is a summary of the individual effects.
  The caption is corrected.

4.2.4 Contact angle

- Line 293: Please move this description prior to Figure 6.
  The description moved before Figure 6.

4.3 IEC wind conditions

- I assume Figure 8 is conducted for the reference parameters of the bearing stated in Table 2, which explains why the Pf for IEC onshore 1A is 2e-5, as this is basically the same as Figure 6. Is this what was done here? Again, additional explanation would be helpful. Then other classes and reference TIs are studied. Having said that, why are no vertical bars shown like in Figure 6? Again, is it because the effect of the uncertainties are negligible? Is this what “The variance of the results is too small, and it indicates that the clusters are closer together, suggesting less diversity and more consistency.“ It is just no surprise that as TI and average wind speed decrease, the Pf decrease. Knowing how much is valuable. But again my takeaway is that the uncertainties don’t matter compared to the mean values.
  Figure 8 is conducted for the reference parameter of Table 2 for onshore IA. An additional explanation is added.
  There is a difference between cluster and uncertainty. Uncertainty is considered in each simulation by applying the probability distribution in force, dimension, and material. In order to see the consistency and repeatability of the simulation, it is repeated 20 times for each IEC wind class and wind sites to make a cluster. Those vertical lines were the variation between different simulations and not uncertainties.

- Line 314: From the original Title, the authors have added the sentence “This exercise is referred to as a code-site comparison”. I will admit I still don’t see the value in mentioning this. Here it seems the term “site” refers to something other than the actual wind sites described later in Figure 10. What is being discussed here is the sensitivity of the probability of failure to the number of simulated seeds for different wind classes.
  The sentence was removed.

4.4 Wind sites

- It is worth mentioning in this section that the turbulence > 0.6 at Sujawal and > 0.8 at Aysha, compared to IEC 1A which is 0.16. It is true that this is given in Table 6, but other than a number buried in a Table, it is not discussed. The statement is made that these sites “are categorized in the IEC II class while their Pf is higher than the IEC I class wind sites”, but nothing is said about their turbulence level. As can be seen from Figure 8, the TI makes a large difference. As far as I

can tell from Table 6, these TI are much higher than even class A+. This does not feel like an apples-to-apples comparison.
As mentioned before, the IEC turbulence intensity in ETM is not 0.16; however, to distinguish the difference between these two sites and IECs, the following text is assessed.
"These high Pf are the result of high turbulence intensity, as addressed in Table 4."

- Figure 10: Please label the red line at 5e-4 as the acceptable component class 2 "safe-life". It took me several minutes to figure out that's what it was – for a time I thought it was the described maximum Pf of the IEC cases.
The figure is modified to clarify what the red line is.

5 Conclusions

- Line 342: I recommend "reliability of the blade bearing" be changed to "probability of static overload of the blade bearing".
Text is corrected accordingly.

- Line 341: Here again, only the range of mean value is being discussed, with groove conformity and ball diameter having the greatest impact on Pf. This is true. I am still struck though that the manuscript spent several pages developing the methodology for treatment of uncertainties in Section 3.2 through 3.2.4 (over 5 pages) and as far as I can tell, they have a negligible effect and no mention of this is made in Section 4 or 5. Then again, maybe I am completely not understanding the meaning of the vertical bars in Figure 6.
As discussed before, the vertical line is related to variation in the cluster and not uncertainty itself. It shows how much the simulation is repeatable and does not have any relation to uncertainty.

Minor grammatical comments:

- Line 10: the citation style here is better as "...Emissions by 2050 (IEA, 2023)." Maybe I'm overly fussing about it, but I recommend the authors review the correct use of \citet{} and \citep{} (if using LaTex) depending on how the remainder of the sentence is written. This occurs in many places in the text.
The citation is reviewed and corrected throughout the whole paper.

- Line 21: a space is needed between 90 degrees and in.
The text is corrected.

- Please italicize the D in "...groove radius/*D*" in Table 2.
The text is corrected.

- Line 291: there are extra spaces between 25 and 65 and the degree symbol.
The text is corrected.