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.

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.

- 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.
- 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.

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.

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.

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.
- 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.

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.

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.
- 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.

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.

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.
- 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?

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.

Figure 6

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

4.2.4 Contact angle

- Line 293: Please move this description prior to 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.
- 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.

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.
- 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.

5 Conclusions

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

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.
- Line 21: a space is needed between 90 degrees and in.
- Please italicize the D in "…groove radius/*D*" in Table 2.
- Line 291: there are extra spaces between 25 and 65 and the degree symbol.