In this paper, the authors describe a methodology to calculate the consumed fatigue life for a wind turbine main bearing and tower based on a combination of SCADA measurements, accelerometers, and dedicated strain gauge bridges and models. In general it is interesting, but the article could benefit a bit by further grammatical editing and trimming in length as it's quite long. The most interesting Figures 14-18 of results should be increased in size, as they are quite small even when zooming in electronically. I did not have the time or expertise to review the majority of the material on tower fatigue, including the entirety of sections 5.3 and 5.4. I offer the following comments for consideration to improve the article.

Abstract

- Line 19: I still have my doubts about the importance of "…low turbulence intensity…" mentioned here. See later comments on Section 5.5.2.
- Lines 21-22: Related to comments on Section 5.5.1, on the surface, I don't disagree with the statement "It was also found that the specification of the gearbox mounting stiffness can lead to a 60% overprediction of the main bearing loads" but it is misleading because it depends on one's view of the meaning of "specification" of the stiffness. In this case, the 60% overprediction is a result of comparing a realistic stiffness to a rigid gearbox mount, which is not a typical assumption as the elastomeric mounts are intentionally relatively soft. When the realistic stiffness is compared to no (or very low) mounting stiffness, there is very little difference in the main bearing loads. Although it's still worthy to describe in the body of the paper, from this perspective there is little interesting to note in the Abstract itself, so I recommend this sentence just be deleted.

1 Introduction

- Line 35: Do the authors intend the phrase "As a failure in the main bearing means a failure in turbine operation" to mean that the turbine itself may fail catastrophically, or just lead to downtime and replacement? From the next sentences, it seems the latter. If so, I recommend "As a main bearing replacement incurs significant costs and turbine downtime, …"
- Line 54: I am not sure of the meaning of "aims to maximize coverage using". I think simply "aims to use" is clearer.

2.2 Main bearing fatigue lifetime

- Lines 130-134: In addition to the text here regarding a_ISO, I believe adding a pointer statement that a drivetrain thermal model will be discussed in Section 4.4 would be helpful. Without any such statement, the reader is left wondering how a_ISO will be determined (I did). Such a statement is also made in Section 4.0, but it would have been helpful earlier.

2.3 Virtual load sensors

- Line 137: In general, the article seems to place emphasis on the benefit of "keeping the necessary measuring [instrumentation] in the nacelle." In this case, strain gauges in the nacelle rather than at the bottom of the tower. Is there really much benefit to this in practice though? Obviously, strain gauges at the tower bottom are very easily accessible. Can the authors comment?

3 Measurements

- Line 166: There is no "…graph on the left…". The discussion here does have some importance, as in the Abstract the very high fatigue lives are attributed to "the low average wind speed of the turbine site compared to the wind turbine design wind class IA."
- Lines 173 and 176: Maybe I am mistaken, but gamma is described as the yaw angle in line 170 but the azimuth angle in line 176.

4.1 Strain gauge zero-drift automatic calibration

- Lines 208-209: I don't understand the meaning of "No external dynamic load should cause zero strain." I don't necessarily disagree with it, but it seems to be a restatement of the fact that the external loads are assumed to be zero in idling and parked conditions. Also, I recommend "bending moment" be changed to "bending moments" here, or even "blade root and tower bottom bending moments" for greater clarity.

4.2 From blade and tower to main bearing loads

- Line 233: I believe adding a pointer "A sensitivity analysis is performed in Section 5.5.1 to evaluate the importance of this assumption" would be helpful.
- Lines 246-251: Here the drivetrain model is discussed, which assumes that the gearbox carries some load because of the main shaft bending flexibility. This may very well be fine, but I'll admit I don't know off the top of my head how good of an assumption this is compared to deflections resulting from main bearing internal radial clearance.
- Line 257: Here, thrust load-sharing between the two main bearing is discussed, but in the end I believe the rear bearing is simply assumed to act as the locating bearing and carry all the thrust. This would be a common design objective achieved by greater axial internal clearance in the front than the rear bearing. Is that a correct understanding of what the authors have implemented?

4.3 Tower bottom virtual load sensor: thrust and fatigue loads

- Figure 4: Is quite small. I recommend each one be enlarged. Additionally, I'm not sure I understand the value of examining the PSD of the azimuth. I assume this signal looks similar to a triangular wave (i.e. roughly linear from 0 to 360 degrees repeating for each complete rotor revolution), so it has spectral content at each rotor frequency, not just 1, 3, and 6P as described in line 279.
- Line 280: I don't understand the sentence "…only the accelerometer signal can well capture the first fore-aft turbine frequency (around 0.62 Hz Rinker et al. (2018)), while its amplification of higher frequency components compared to Mfore–aft cannot be considered pure electrical noise." To me, it appears that there is nearly as strong content of this frequency in Mfore–aft as the accelerometer as shown in the middle figure.
- Line 284: Gearbox vibrations are described relative to the accelerometer measurement; however, I don't believe it's described or shown in the paper where exactly this accelerometer is located. Is it attached to the bedplate, for instance? I don't necessarily disagree with the statement here; however, the vibration frequencies in these plots are from 0 to 5 Hz or so, which is quite low for a "gearbox vibration" in general, for instance, as a result of tooth meshing. More importantly though, what is the significance of the extent of attenuation (or not) of the

accelerometer compared to Mfore-aft? If there isn't any significance, then why is this discussed? If there is significance, what is it?

4.4 Drivetrain thermal model

- In general, the thermal model described here is interesting. Aside from the statement "…it yielded 3 C MAE" for the gearbox temperature, I don't have a good sense of the actual temperature results. I see this is later included in Section 5.5.4, so I am wondering why this mention of 3 C MAE is included in the Methodology section rather than the Results section. Having said that, I think practically speaking such a thermal model might not be needed as many or most turbines have an existing main bearing temperature measurement.
- Line 312: Here, the grease cleanliness level is mentioned, but what assumption was made about it? Typical contamination maybe?

5.1 Continual calibration routines

- Line 322: Here it is stated "From the left charts, it is possible to observe larger zero drifts for the blade root compared to the tower bottom strain gauges." This may be true, but it is quite difficult to tell as the channels have offsets that overshadow any change in them over time. A better way to show the drift would be to normalize each one by their initial value and plot the change over time.

5.2.1 Fatigue damage accumulation

- I thought I understood how the lifetimes of the tower (1770 years) and front (166 years) and rear (333 years) were determined, but apparently not. For example, I had assumed if 0.003 D is accumulated in the tower in 9 years, then it would take 3,000 years (= 9/0.003) for D to reach a value of 1. Similarly for the main bearings (9/0.03 = 300 years for the front bearing). It's not clear to me that Equations 2 and 7 explain this difference. If the authors could address how the stated lifetimes are calculated, I believe it would be beneficial.

5.2.3 Effect of periodic calibration on the main bearings L10

- Line 361: I recommend adding "…can influence the lifetime estimation of main bearings for this methodology."

5.5 Main bearings loads and fatigue lifetime analysis

- Figure 14 especially would benefit from increasing in size.

5.5.1 Sampling frequency and gearbox mounting stiffness assumptions

- Although on the surface I don't disagree with Figure 15b or the statement that "overprediction of the front and rear dynamic equivalent loads could be reached, if a gearbox is assumed to be rigidly fixed in a 4-point drivetrain" in line 451, the "baseline" that is being compared to for this paper is a baseline of rigid stiffness of the gearbox mounts, which I don't know is a very common assumption. This results in (large sounding) overpredictions of up to 60%. I think it's much more common to do the opposite: that is, neglect the stiffness of these mounts as a "baseline" for a simple model (which is the description used in the Conclusions), which when comparing the estimated stiffness to would result in a very similar prediction. The conclusion one would derive then sounds quite a bit different. It would be that the dynamic equivalent loads are not affected (or only affected by a very small amount, it depends on the actual numbers in Figure 15b) by the estimated gearbox mounting stiffness compared to an assumption of no stiffness. This then makes the statement in Line 449-450 "…the assumption on which stiffness should be used to model the gearbox mounting fixation points into the bedplate has been shown to be one order of magnitude more relevant" to ring hollow. See related comments to Abstract and Conclusions.

5.5.2 Environmental and operational conditions (EOCs) mapping of the main bearings dynamic equivalent loads

- To be honest, I had quite a difficult time discerning what the take-aways are from Figure 16 compared to Figure 14 discussed in all of Section 5.5.2. Are there any? In reading the text, I found myself more easily referring to Figure 14 for interpretation. Figure 14 tells me that Peq for both bearings is relatively well tightly grouped between the 10th and 90th percentiles, regardless of the shear or TI. Section 5.5.2 seems to try to convince me of the opposite. As an example for the rear bearing, let's take the sentence "Approximately 10% increase in load driven by a change in turbulence from 15% to 8%." Although this may be true, what is the significance? From what I can tell from Figure 16, what is described here is the small change from yellow to yellow-ish green. Is that correct? Another reason I bring this up is that the manuscript is already quite long, and I do not see anything new in Section 5.5.2. As far as I can tell, it could be deleted and any additional explanation included in the discussion of Figure 14. All Figure 16 seems to tell me that is new is that the TI is up to 30% and the shear coefficient up to 0.4, and that the highest values of Peq for the front bearing are at low shear (this latter fact has already been described elsewhere). It is only modestly interesting that the Peq for the front bearing tends to be largest around 10% TI, but then again it's a relatively small increase (maybe 20 kN out of over 200, or less than 10%) and it may be an artifact of averaging over 10 minutes. If this section and Figure 16 remains, Figure 16 especially would benefit from increasing in size.

5.5.3 Main bearing lifetime using thrust estimate from virtual load sensors

- Line 474-475: I think this sentence would benefit from rewording to something like "This might come from the fact that the bearing fatigue is dominated by the mean value of Peq rather than fluctuations in the radial and/or axial loads."

5.5.4 Main bearings modified rating life: L10m and aISO

- Line 479: Here max main bearing temps of 55 and 61 C are mentioned, but what is shown in Figure 17 is ambient temps? Is there a reason for this? Why bother to calculate the main bearing temps if they are not to be shown in Figure 17, only discussed in general? It's the bearing temperature that really matters
- Figure 17: I'm not sure I understand the life mentioned in each subfigure, which ranges from 20 to 10,000 years. Is this the basic life for an individual 10-minute bin "j" from Equation 6, or the combined and modified life from Equation 8? The use of L10m,f and L10m,r in the Figure says the latter, but it feels like it is a graph of the former. Equation 8 implies aISO has a single fixed-value and is applied to the combined L10; however, Figure 17 implies aISO is calculated for every 10-minute period from the main bearing temperature and dynamic equivalent load (and fixed

contamination level and fatigue load limit) and applied to the basic L10 before being combined. Is this a correct understanding?

- Lines 491-492: Like section 5.5.2, a citation of Kenworthy (2024) would be appropriate here, as they came to the same conclusion.
- Line 496: Here L10,r is given as 315 years, while if I understand the Abstract and section 5.2.1 correctly it is 333 years. Shouldn't they be the same?

6 Discussion

- If it were me, I would integrate anything new in Section 6 into the main body of the article itself. It's an opportunity to shorten the article, as it feels much of what is described here has already been stated earlier, and there's an additional Conclusions section.
- Line 502: Strictly speaking, I think this should state "…eight full strain gauge bridges…", shouldn't it?
- Lines 523-526: Like previous comments on Section 5.5.1, Abstract, and Conclusions, I believe these sentences are misleading. Among other reasons, stating "The stiffness of the gearbox mounting could lead…" alone is rather vague. What is meant here is "Assuming the gearbox is rigidly mounted could lead…"; however, as stated earlier, this is an usual assumption. It is more typical to assume the gearbox mounting has little or even no stiffness. As mentioned earlier, I would rather see these statements "flipped" in their comparative sense, with the resulting conclusion being that modeling of the gearbox stiffness had little impact on Peq or L10 (actual amount TBD).
- Lines 530-534: The sentences here regarding the relative importance of TI to other factors seem to be in some ways contradictory and in other ways misleading. Both Kenworthy (2024) and the results in this manuscript indicate that main bearing temperature and grease cleanliness first and foremost, followed by shear, are much larger factors in main bearing rating life than the turbulence intensity. The reader would not get that sense right now.

7 Conclusion

- Line 558: I recommend this be changed to "…has a longer life as the shear exponent increases…"
- Line 560: As mentioned earlier, the statement "…the rear main bearing, at rated wind speed, has higher loads for lower turbulence intensities" seems to have mis-placed emphasis. It is extremely difficult to see this in Figure 16, when in comparison the mean wind speed has a far, far greater effect. I am not able to tell whether, for a given wind speed, if a change in TI or shear exponent has a greater effect.
- Line 560: The statement "Neglecting the stiffness of the gearbox mount renders unrealistically high Peq, but having the stiffness values within realistic ranges results in little influence on the lifetime" hearkens back to Section 5.5.1, but I believe it uses wording opposite to Figure 15b. From Figure 15b, I would write this statement that "Assuming a rigid gearbox mount renders unrealistically high Peq…" or "Neglecting the stiffness of the gearbox mount has a minimal impact on Peq". See previous comments on Section 5.5.1 and Figure 15b. Like the Abstract, it's worth considering if this point is even important enough to mention in the Conclusion. It might be deleted without losing anything, as it's really a sensitivity study like downsampling the SCADA rate which is not mentioned in the Conclusions. I leave it to the authors to decide.

- Line 562: Although this may be true, is there a reference that can be cited that life corrections have been validated for other bearings (smaller and/or oil lubricated)? Such a statement would actually be beneficial earlier in the manuscript, rather than only here.

Minor grammatical comments:

- Line 5: Rather than “minimally intrusive strain gauge at blade and tower” I believe simply “tower and blade root strain gauge bridges” is clearer.
- Line 26: I believe the preferred citation style is here “…global benchmark reports (IEA and NEA, 2020; IRENA, 2024).” Similar for line 29 “…the foundations (Ziegler et al. 2018; IEC-TS-61400-28, 2020).”
- Line 42: Use plural “reported failures” here.
- Line 45: Use citation style “(Santos et al., 2024)” here. I recommend reviewing citation style throughout. Maybe the WES editors will take care of this themselves.
- Line 86: Should be “number of cycles” here.
- Line 102: I’m not sure “reasoning” is correct here, I believe the meaning is “meaning” as used later in line 104.
- Line 113: Italicize P, X, and Y here in the text.
- Line 139: I’m not sure I understand the meaning of “Benefiting from instances statistics…”. Similarly for line 159.
- Line 146: A space is needed “…different. Similarly, Dimitrov and Gocmen (2022) show…”
- Line 194: Does the V25 have a S355 steel tower as stated in line 79, or only “assumed” to have a S355 steel tower as stated here?
- Line 197: Here and elsewhere, I believe “L10,m” should be “L10m”.
- Line 262: Here “…time steps,NlaggedFNN Dimitrov and Gocmen (2022)…” seems a little garbled. Is “Nlagged” a variable?
- Line 270: DLC does not need to be redefined here, as it was already done in Line 77. Similar for other instances of “design load cases (DLC)” after this point.
- Line 324: There is a double period in this line.
- Line 433: There is a double-space here.
- Line 439: Instead of “is incremental”, I believe “incrementally increases” makes more sense here.
- Line 541: Should be “continually calibrated”.