We thank Mr. Hart for the comprehensive review. Answers and comments are found below in blue. Because a lot of changes have been made, please consider that the revised manuscript contains the latest version; some paragraphs have been edited multiple times and the replies given in this document may not be up to date completely.

WES Paper Review:

Review of rolling contact fatigue life calculation for oscillating bearings and recommendations for use, with examples for wind turbine bearings

Reviewer: Dr Edward Hart (University of Strathclyde)

17/10/2023

Summary: This paper reviews the concepts and methodologies which have been proposed/developed for application in calculating (rolling contact fatigue) rating lives, as well as seeking to provide guidance on when each of the available approaches might be most suitable. The authors additionally seek to provide a unified view regarding how each of these methodologies relate to each other. Specific focus is placed on applications to slewing bearings in wind turbines.

General comments: This is an important topic which has arguably received less attention in wind research (and in rolling bearing research more generally) than it should, given the criticality of slewing/oscillating bearings to wind turbine function and reliable operation. The authors demonstrate a very high level of expertise regarding this topic, and the manuscript contains a good number of important contributions to support future research in this topic area as well as the improved application of such techniques in wind research more generally. As a result, this reviewer feels there is a clear contribution to the scientific literature, making a paper on this topic both novel and appropriate for publication in WES. Having said that, the submitted manuscript falls short of what I would consider as being immediately publishable (for reasons which will be outlined). I am therefore recommending that major revisions are undertaken by the authors to ensure the full potential of this valuable paper are realised. At a high level the following points need to be addressed:

1) The writing throughout the paper has a tendency to be imprecise and informal. Clarity and specificity are crucial when seeking to elucidate a complex topic. Extensive improvements to language and the rigour with which concepts are discussed is therefore encouraged to ensure clarity and avoid confusing the reader (specific details are provided under "Specific comments").

This has been addressed whenever criticized.

2) The paper focuses on slewing bearing in wind turbines, but the introduction/background of the paper does not introduce or outline these bearings in any detail or provide an overview of their design, design trends and load conditions and/or provide information of their failure rates or critical issues as informed by the scientific literature. I believe a more complete background on these bearings is therefore required.

We would say that the paper focuses on oscillating bearings, and the calculations of oscillating bearings are then applied to slewing bearings, among others. The main focus of the paper is a review of existing literature on *oscillating* bearings, which may or may not be slewing bearings. Some people have speculated that large slewing bearings behave differently from small bearings but there is little to no evidence in the literature to support that hypothesis, hence it is not a point that we cover to a great extent in this paper. It will be mentioned in the "current challenges and future work" section, though.

3) Motivation – the introduction appears to indicate that wear is an issue for wind turbine slewing

bearings, before saying that rolling contact fatigue will be the focus of this review. As a result, it feels like the authors undermine their own paper at this stage. Please take more time to elaborate on what we know about failure in these bearings, and to motivate why an analysis of rolling contact fatigue analysis remains important here (as it certainly does). The current exposition of this point feels rushed and underdeveloped.

The introduction comments on potential wear risks of *oscillating* bearings rather than slewing bearings in particular. (cf. above reply to 2))

Unfortunately we don't know a whole lot about failures of oscillating wind turbine slewing bearings, there are not many (if any) publications on failed pitch bearings in the scientific literature; informally through alternative information channels (social media such as LinkedIn) it is possible to find some information on failed pitch bearings but these are often related neither to wear nor to rolling contact fatigue. Rolling contact fatigue is one of the few failure mechanisms that can be calculated with some accuracy and thus it always makes senses to validate that a bearing is designed to withstand it. Moreover, due to the speed in which the wind industry continues to evolve, we would argue that past failures do not necessarily inform future failures, that is, new designs might cause issues that have not yet been observed because the turbines in the relevant MW class haven't been operating long enough. This being a review that focuses on oscillating bearings in general and applies the findings of that review to wind turbine slewing bearings, we don't think this is necessarily the place to discuss this topic.

Slightly changed the introduction to emphasize that RCF can always occur in principle and should be designed against:

Rolling bearings under oscillatory movements are commonly associated with wear damage to the raceways and rolling bodies (Grebe, 2017; Stammler, 2020; Behnke and Schleich, 2022; FVA, 2022; de La Presilla et al., 2023). Wear can set in very quickly, but it can also be prevented by a number of measures (Schwack, 2020; Wandel et al., 2022). Rolling contact fatigue, on the other hand, is always a possible failure mechanism even in a properly designed bearing (Sadeghi et al., 2009), except for very low loads (Ioannides et al., 1999), at which there is dispute about its occurrence (Zaretsky, 2010). In many cases, such as large oscillation amplitudes, or the use of oil lubrication, wear is unlikely to occur and thus, rolling contact fatigue becomes a more important focus. Engineers should therefore consider both wear and rolling contact fatigue as a possible failure mechanism. […]

4) Structure – I learned many fascinating new concepts while reading this review, in particular the various important effects present in oscillating bearings. However, these new concepts are dotted throughout the paper, only appearing as they are included in one method or another. It makes it very difficult to develop a clear appreciation for the overall operating/loading conditions, complexities and nuances of oscillating bearings when the information is provided in this manner. I would therefore urge the authors to introduce a new section at the start of the paper, along the lines of "Operational fatigue conditions in oscillating bearings" in which all of the critical real world aspects are described and discussed, prior to any consideration of the various fatigue models and which effects they account for. This will provide readers with a clear outline of the "ground truth", based on which they will be better placed to appreciate all later discussions of the various models and what they do or do not include.

Added a new subsection to the introduction: 1.1 Operational conditions of oscillating bearings

5) Throughout the paper, many complex concepts (e.g. subsurface stress time histories in rolling contact, lubrication and grease thickener effects, etc.) are only briefly mentioned, as if familiar to the reader, but without providing any references for further reading. I feel this reduces the overall usefulness of the review to the general WES reader, and so encourage the authors to go back through the paper, adding pertinent references for all such concepts throughout. Remember, many readers will have non-tribological/non-bearing specific backgrounds. In order to maximise the value and impact of this review, such readers should be provided with clearly signposted references in

which they can learn more about these concepts. There are also a good few instances where claims/facts are stated without a suitable accompanying reference to back up that claim. Finally, I'd note that this review paper has a relatively low number of references utilised overall. While this may be appropriate in some cases, I believe this particular review would benefit from a broader representation of relevant literature (detailed in "Specific comments").

More literature has been added

6) The guidance provided on which methods to use and when is arguably fairly limited. For example, in one instance, a total of 8 different methods are indicated as being suitable in your flow diagram at one point, without any more specific guidance on which might be best (for instance, I don't think you explicitly state that approximate methods should be considered as less accurate etc.). Based on your familiarity with the various methods, the foundations of their development and extent of experimental validation (which I am aware is low in all/most cases), I wonder if the authors might be able to provide a clearer path to delineating and selecting an appropriate method. It may be this is simply not possible at this stage. However, if that's the case then perhaps the best advice is to stick as closely as possible to ISO method(s) and their extensions, including only the necessary add-ons for the case one is dealing with. The logic behind this would be that design certification often require some chain of evidence/justification, which would best be provided by links to an international standard – at least until some other method is clearly demonstrated to be superior. I am very happy to be told I am wrong about this, but either way I think a more detailed discussion of deciding which method(s) to use would be helpful.

More guidance was added in Section 4.1, see also replies below in this document

7) The paper feels like it's missing a section on "Current challenges and critical future work". This is a topic for which it feels like we are still at the beginning of its proper scientific exploration. This review should therefore provide a roadmap for overcoming current challenges and improving the rating life predictions for oscillating bearings in wind turbines. This becomes even more critical if, as discussed in point 6, we don't currently have any good way to decide between the various available methods in some instances. Can you outline the current knowledge gaps and indicate the necessary experiments, data, analyses and modelling work which would allow us to bridge them?

Section "current challenges and future work" was added

Specific comments:

Abstract: "calculation" used twice in first sentence. The abstract also feels vague, can your overall recommendations and findings be listed more explicitly? Similarly for the application to wind pitch and yaw bearings.

The recommendations mostly refer to approaches that should be used under various circumstances; neither the approaches nor the circumstances can be briefly explained in the abstract. Main findings are difficult to note too. The abstract is brief because more detail is given in the paper itself.

L11: "change the wind's angle of attack as it acts on the blade." Perhaps better as "the blade's angle of attack"?

Changed as suggested

L12 "Movements in modern turbines mostly consist of small oscillations with the occasional 90 degree movement to bring the turbine to a halt." -> "Pitch control actions in modern *wind* turbines mostly consist of small (x degs) oscillations with the occasional 90 degree movement to bring the turbine to a halt" (Language suggestion, plus, please indicate what "small" means here).

Changed to:

Movements in modern wind turbines mostly consist of small (typically $< 10^\circ$, often as small as $< 1^\circ$, cf. Stammler et al. (2020)) oscillations with the occasional 90°movement to bring the turbine to a halt.

L13 “Similarly, yaw bearings rotate the turbine to face into the wind. Their movements are typically fewer and, depending on the site and the yaw system design, longer.” Please revise language, this is unclear and imprecise as written.

Changed to:

Similarly, yaw bearings rotate the turbine to face into the wind. Their movements are typically fewer and, depending on the site and the yaw system design, longer (< 10° during power production but potentially more while idling) while they do not tend to become as low (< 1◦) as pitch angles (Wenske, 2022).

L15 “Rolling bearings under oscillatory movements are commonly associated with wear damage to the raceways and rolling bodies (Behnke and Schleich, 2022; Stammler, 2020; Grebe, 2017; FVA, 2022; de La Presilla et al., 2023)” -> Is damage more common in general, or is it that the damage that it does experience is wear more often than not? Please order references chronologically.

Wear can occur, there is no mention of how common it is (we are not aware of any such statistics). We are not sure it this answers your question, but the sentence has been rephrased to:

Rolling bearings under oscillatory movements are commonly associated with wear damage to the raceways and rolling bodies (Grebe, 2017; Stammler, 2020; Behnke and Schleich, 2022; FVA, 2022; de La Presilla et al., 2023). Wear can set in very quickly, but it can also be prevented by a number of measures (Schwack, 2020; Wandel et al., 2022).

References ordered chronologically.

L16 “In some cases, such as large amplitudes, varying amplitudes, or the use of oil lubrication, wear is unlikely to occur and rolling contact fatigue becomes more relevant.” -> Large amplitudes of what? (I assume motion, but you could mean load), please be more precise with language.

Changed to “large oscillation amplitudes”

L18 “and ensure they do not cause a failure of the bearing.” This sentence feels open ended and it’s not immediately clear what it adds.

Shortened to “Engineers should consider both types of damage as a possible failure mechanism.”

L19 You indicate that wear is maybe a dominant failures more, except under circumstances that don’t apply to pitch or yaw bearings (as far as I’m aware), but then say that this paper is going to review rolling contact fatigue… It feels as if like you start by outlining why a different paper is needed. Also, when describing wear vs rolling contact fatigue circumstances you don’t outline which yaw and pitch bearings may fail into and why. Please revise this discussion and provide further info on yaw and pitch specific failures.

Wear can occur in pitch and yaw bearings, (Bartschat, 2023) but how often it does is difficult to tell, there are no available statistics on this issue. It can also occur without causing failure but accelerate RCF.

Added information:

In many cases, such as large oscillation amplitudes, or the use of oil lubrication, wear is unlikely to occur and thus, rolling contact fatigue becomes a more important focus. Moreover, depending on its severity, wear in itself doesn’t necessarily cause a complete failure of the bearing but it can also accelerate rolling contact fatigue FVA (2022a, b). Engineers should therefore consider both wear and rolling contact fatigue as a possible failure mechanism.

L20 “There are a number of approaches for rolling contact fatigue life calculation in the literature,

see Sadeghi et al. (2009) and Tallian (1992) for an overview, but they are mostly intended for rotating applications." I think there are other good references to include here, please consider whether those listed are sufficient (I am a big fan of Zaretsky 2016 - Rolling Bearing Life Prediction, Theory, and Application
https://ntrs.nasa.gov/api/citations/20160013905/downloads/20160013905.pdf)

The two listed reviews are very comprehensive (in the sense that they cover a lot of models) which is why they were chosen. The review by Zaretsky that you cite only covers three models, which are all closely related to the one used in ISO, and all of which are covered in Sadeghi (2009), while Sadeghi alone covers 18 probabilistic and 15 deterministic models, though more superficially than Zaretsky – but at this stage of the paper we think a broad but superficial overview is sufficient.

The given citation was however added to the introduction chapter because it includes a thorough overview of basic rolling contact fatigue related information.

Figure 2: I am struggling to interpret the left hand side of this figure, why does is the blue line continuously loading, whereas the red line has zero regions? Please provide a more detailed description to help the reader.

The absolute value of the angle in the left figure is relatively meaningless, the red movement oscillates around a 0° position but it might as well oscillate around any other position, likewise for the blue movement. Relative movement of the bearing is the driving factor in rolling contact fatigue as it causes load cycles in the raceway.

We tried to clarify this by modifying the test and adding a footnote:
Some of the ISO-related methods are intended for constant oscillation amplitudes as depicted red in Fig. 2, where an oscillation with a constant amplitude about a position of 0◦is shown[2], while some other ISO-related methods and all non-ISO related methods are intended for arbitrary movement as depicted blue in Fig. 2.

[2] Rolling contact fatigue is driven by relative movement of one of the rings to the other, which means that the mean position of the oscillations in Fig. 2 only moves the position where load cycles occur on the raceway but has no effect on the life of the bearing. The critical difference between the blue and red lines is their relative movement, not their absolute position.

Line 37: "Fundamentally, rolling contact fatigue in oscillating applications is caused by a rolling element repeatedly rolling over locations on a raceway, as is the case in rotating applications." Maybe the reader could be given a little more lead-in to considering rolling contact fatigue. I suggest this as many engineers will be more familiar with cases of fatigue cracking and rainflow counting methods etc, rolling contact fatigue is quite different to that, since it is the passage of rollers which causes stress cycles and not simply the applied load varying. Some more discussion of this would, I feel, be helpful.

Added a paragraph on rolling contact fatigue in the Introduction:

Rolling contact fatigue is a possible failure mechanism of bearings. It is caused by the fact that, even under a constant external load, movement of the bearing (rotation or oscillation) causes movement of the rolling bodies (balls or rollers) relative to the bearing rings. If the rolling bodies transmit load to the raceway, their movement leads to stress cycles, because every location of the raceway changes from a loaded state while it is in contact with a rolling body to an unloaded one while it is not (cf. Fig. 6, left hand side, for a typical case in a rotating bearing). The resulting stress amplitudes can, over time, cause fatigue damage on the raceways, or, less frequently, the rolling bodies. The driving stress for rolling contact fatigue is typically considered to be shear stress. Fatigue can be initiated from shear stress below the surface of the raceway (subsurface fatigue) and from shear stress at its surface (surface fatigue) (Lundberg and Palmgren, 1947; Ioannides et al., 1999; Harris and Kotzalas, 2007; Zaretsky, 2013).

Equation 1 – please use the proportionality symbol here, as the tilde is ambiguous in its meaning.

All tilde occurrences in the paper have been changed to the proportionality symbol.

L44 I'd include a good background reference on this material, e.g. Zaretsky 2016 as above. Also, I'd suggest providing some discussion of the limitations of RCF modelling/prediction.

Added (Lundberg and Palmgren, 1947, 1952; Harris and Kotzalas, 2007; Zaretsky, 2013)

L46 I'd recommend defining this predicted life as the "rating life" to distinguish it from the observed/service life seen in the field.

Changed the sentence to:

They assumed the bearings to be rotating. $L_{10,rev}$ then gives the number of millions of revolutions at which 10% of bearings are expected to suffer the first visible raceway damage, also called "basic rating life".

L54 "For small oscillation amplitudes, a_osc typically becomes very large" this sentence doesn't really add much as it is. Are you able to provide some ballpark of the size of a_osc? E.g. "commonly falls in the range 10^x – 10^y" or similar.

Changed the sentence to:

For small oscillation amplitudes, a_osc typically becomes very large, with a_osc commonly (but not always) being in the range of 1...1000.

L55 "There are two possible definitions of an oscillation "amplitude" " – technically there are an infinite number of ways one might define this amplitude, I'd change "two possible" to "two common"

Changed as suggested

L56 "For small oscillation amplitudes, much of the existing literature will predict a high likelihood of wear, particularly for grease-lubricated bearings (Behnke and Schleich, 2022; Stammler, 2020; Grebe, 2017; FVA, 2022). Nonetheless, as discussed in Sec. 3 of this review, it is definitely possible for rolling contact fatigue to occur without wear2 even for oscillating amplitudes as low as $\theta = 1^\circ$ ($\phi = 2^\circ$)." I agree this is important context, but it feels a little shoe-horned in here. Is this the best place for this information, can you expand a little to give the reader a little more to grab hold of?

Incorporated this information into the introduction instead

Equation 3 – you use the notation a_OSC in Equation 2, but then switch to a_Harris for Equation 3. Please determine a way of making it clear to the reader that the latter is an instance of the former (it is reasonably clear that this is the case from context alone, but for the sake of rigour I'd like to see it made more explicit).

Added the sentence "All factors a in this paper are instances of a_osc as shown in Eq. 2."

Line 67 "Thus, taking an exemplary bearing that oscillates with an amplitude of $\theta = 10^\circ$ and that, if it were rotating, would have a life of $L_{10,rev}$ = 1 million revolutions, and applying Eq. 2 and Eq. 3 gives a life of $L_{10,osc} = 90^\circ\ 10^\circ\ L_{10,rev}$ = 9 million oscillations according to the Harris factor. This is because it will execute an arc of $A = 40^\circ$ per oscillation, which is considered as 1/9 th of a rotation by the Harris factor" While a worked example does help make things clear for the reader, I wonder whether readers will need such a simple equations explained in such detail?

It is a very simple example but understanding the Harris factor is the most essential lesson from this paper that anyone who works with oscillating bearings needs to take away. Therefore we would like to keep it.

Line 74 I think it's perhaps not an exact equivalence between Harris and LRD automatically, since Harris didn't deal directly with changing load conditions, while LRD does. Is it more correct to say LRD is equivalent to applying Harris independently to each different load case?

Yes, we extended the following sentence:

Doing so is in principle identical to using the Harris factor, if the Harris factor is used to sum up movement independently at each of the same load cases.

Line 78 Please indicate to the reader that these two effects will be described in detail below.

General comment – we are learning about what the loading etc looks like for an oscillating bearing by going through a series of incorrect or approximate approaches. This means the reader has to do a lot of mental gymnastics to keep track of what is actually happening versus what people assumed was a reasonable approximation. I think perhaps the paper would serve the reader better if early on there was a section which simply described the true characteristics of loading (along with any other pertinent effects) in oscillating bearings (as per Fig 5). When discussing the various approaches, the reader will therefore know the "ground truth" and hence be able to better follow where approximate methods miss important aspects of the real world case.

Additional section 1.1 has been added

L86 "but without a derivation of the approach" does "the approach" refer to the original or simplified one?

Changed to "but without a derivation of the simplified approach"

L86 Footnote 6 is rather long and difficult to follow since it seems to assumed familiarity with the works it mentions. Please consider rewriting for clarity and possibly moving this into the main paragraph, rather than as a footnote.

The footnote was intentionally not placed in the main paragraph because it is supposed to give background information for readers that want to look into the original references, but is not required to understand the main paragraph. Nonetheless we added information for clarification.:

Breslau and Schlecht (2020) give a more appropriate treatment of this effect by introducing the factor a_osc,2 with their Eq. 19, which does not contain the simplifications taken by Rumbarger in his simplified approach. This equation was rearranged (without simplifications, but to obtain a less cluttered equation) by Houpert and Menck (2021) into a corrective factor called fθ_crit_i,o in their Eq. 45, here used for the recommended approach.

L88 Is it necessary to list every combination of "a_xxx" notation that has been used in the literature for the oscillation factor? We expect differences in notation and naming from paper to paper, but I'd expect readers to be able to cope with that by simply reading the work to find out what notation is being used. There are currently a great many footnotes dealing with notation and it feels unnecessary.

Hence why they were included as footnotes, some of the publications contain a number of factors and we wanted to be completely clear which factors are being referred to (because, as shown in this review, there tend to be quite a few misunderstandings in the world of oscillating bearings)

Fig 4 lacks units on the polar plot and is difficult to interpret, what am I looking at here? What does the radial scale mean? why do all fluctuations disappear for theta = theta_crit?

Changed the figure to another one that is hopefully easier to interpret:

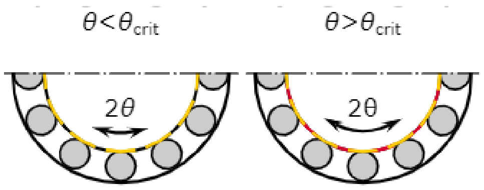

Figure 4. Rumbarger effect: stressed volume on the inner ring as a function of inner ring angle θ relative to $\theta_{crit} = \theta_{crit,i}$, for $\theta < \theta_{crit}$ and $\theta > \theta_{crit}$. Yellow volume is stressed twice per oscillation cycle (cf. Fig. 3), red volume is stressed four times per oscillation cycle. Black volume is never stressed. Only stress cycles for the inner ring are shown.

L 105 "The recommendation in Eq. 5" I'm not sure this makes sense, "the form of equation 5" maybe?

Changed as suggested

General comment – currently this reads as a long list of things people did, with continual caveats, corrections and footnotes. The paper would benefit from a careful consideration of structure and narrative in order to make this as easy to follow for the reader as possible. Perhaps a graphical description of early summary of all developed methods, how they relate to each other and where in the paper they will be discussed would help?

Only a few of the methods (Rumbarger's approaches and Houpert's, and the people applying them) really relate to each other, while a lot of the other publications effectively exist in a vacuum. A historical summary roughly reads like shown in the following. Please let us know if the paper would benefit from showing this in a graph:

Rumbarger and Jones(1968) introduced Rumbarger effect, was simplified (with error) in Rumbarger (2003), Harris et al. (2009)

Houpert (1999) introduced Houpert effect

Breslau and Schlecht (2020), Houpert and Menck (2021) corrected Houpert (1999) and introduced a simplified (but correct) version of Rumbarger and Jones (1968)

Schmelter (2011), Schwack et al. (2016) and Wöll et al. (2018) applied Harris et al. (2009), Houpert (1999).

Hwang (2023) and Münzing (2017) apply Harris et al. (2009)

Menck (2023) generalizes the approaches by Breslau and Schlecht (2020), Houpert and Menck (2021)

Hai and the other non-ISO related methods effectively exist outside the above literature independently for themselves.

L109 : "Strictly speaking, the life of an unevenly stressed volume (as illustrated 110 in Fig. 4, yellow and blue) is not the same as that of an evenly stressed volume which occurs in a rotating bearing (identical to Fig. 4, red)" Do you have a reference for this?

No, this is an original "finding" or note of this paper that is elaborated in Appendix C. It follows from Eq. 1, where the volume V has exponent 1 and the stress cycles N have exponent e (≠1). There is not much more information in the paper because this effect is quite inconsequential. Added the following footnote (and additional text) for clarification:

Strictly speaking, the life of an unevenly stressed volume (as illustrated in Fig. 4, right hand side) is not the same as that of an evenly stressed volume which occurs in a rotating bearing[16] (identical to an oscillating bearing with θ = θcrit) if the total movement of both bearings is the same.

[16] This follows from Eq. 1, where the volume V has exponent 1 and the stress cycles N have exponent $e \neq 1$.

112: "Recommendation" again, I don't feel this is the correct word here

Changed to "The factor chosen for Eq. 5 thus follows the above-mentioned publications."

L120 Footnote 14 is very dense and really gets down into the weeds, in particular it introduces contexts and notation (e.g. load integrals) which are not utilised or fully explained in this review. Either these more complex points are important enough to be included properly, or I'd recommend a briefer footnote which simply gives the reader appropriate context and the main outcomes.

This footnote is intended for readers who want to look deeper into the derivations, and it is intended to point out that the approaches by Houpert and Menck (2021) and Breslau and Schlecht (2020) are very similar but not perfectly identical. This was clarified with a sentence in the beginning:

[17] The two approaches are not completely identical but very similar: […]

L128 It's been mentioned a few times by now, but I am still unsure specifically what the "Houpert effect" is describing in any sort of detail. Maybe link the name to Figure 5 behaviour more explicitly?

Changed by adding a sentence:

Aside from the effects also considered by Harris, the Houpert effect considers that the stress cycle history of the moving ring will be different for an oscillating bearing than for a rotating one. This is illustrated in Fig. 5 for an exemplary element on the moving ring.

L133 "For very small oscillations $\theta \rightarrow 0^\circ$ ($\phi \rightarrow 0^\circ$) on the other hand, the elements increasingly converge toward the stress cycle history seen in a stationary ring, see Fig. 5" It seems that you're possibly saying that it will converge to the stress cycle history of a stationary ring in a rotating bearing (which I don't think it does), so instead I guess you mean that for small cycles the stress time history for the moving ring in an oscillating bearing becomes similar to the stress cycles for the stationary ring in that same oscillating bearing? Either way I think some clarification is needed here.

This is somewhat complicated to express in words but in Houpert's derivations it indeed converges against the stress cycle history of a stationary ring in a rotating bearing, because Houpert's effect alone (unlike Rumbarger's) does not consider the fact that there is a discrete number of rolling elements in the bearing; rather, he integrates over the circumference of the bearing and implicitly assumes all locations to experience the same number of stress cycles. (This is a standard procedure for the life calculation of rotating rolling bearings, ISO 281 equations are all based on this same way of modeling stress cycle history)

This was clarified by means of yet another footnote:

[20] In Houpert's model the stress cycle history of an oscillating ring converges, for small oscillations, against that of a stationary ring in both a rotating and oscillating bearing. These two cases (a stationary ring in a rotating and oscillating bearing) can be considered identical here because Houpert's effect alone, unlike Rumbarger's, does not consider that there are a discrete number of rolling elements in the bearing for the circumferential distribution of load cycles. Rather, he assumes all circumferential locations to experience the same number of stress cycles (with differences in load cycle magnitude only), as is common in a rotating bearing, and integrates over a continuous load distribution around the circumference. This is standard practice for the life calculation of typical rotating bearings and as such also employed in ISO 281.

Equation 6 how does a_Harris change between stationary and rotating rings? I don't see what's different in the mentioned cases shown in this split equation.

a_Harris does not change, but the Houpert factor converges against the life of a bearing with both rings assumed stationary relative to the load, to which a_Harris is applied. Changed the text as follows:

a_Harris in a bearing with both rings for $\theta \rightarrow 0$ stationary relative to load[21]

[21] Both rings being stationary relative to load slightly reduces the life as compared to standard calculations (in which one ring is assumed to be rotating) because it increases the equivalent load of the ring which would otherwise be assumed to rotate. It does not affect the factor a_Harris.

L138 "If applied correctly, the Houpert factor will either be identical to aHarris in the above given cases or shorten the life of the bearing in all other cases. The Houpert effect is thus most noticeable for narrow load zones and small oscillation angles." I don't see why the latter follows the former (as implied by the use of "thus"), perhaps this is additional information rather than being implied by the first sentence?

"Thus" was meant to refer to Equation 6 and the information before the sentence "If applied correctly". Removed the "thus" and added symbols:

The Houpert effect is most noticeable for narrow load zones (small ε) and small oscillation angles θ.

Footnote 18: Again there is lots of additional info being given about notation used in other work, I am just not sure if that is really adding anything or simply risking confusing the reader. Please consider.

The information may be confusing but it is close to impossible that anyone understands these minute details if they are not mentioned here, and for readers who wish to employ the Houpert factor alone, this is necessary information.

L149 "The above three factors have been covered in a number of publications" please provide a more complete list of these references here, even if you do go on to then point to the most recent.

Included a footnote:

[24] A comprehensive list including all publications with relation to the factors, to the best knowledge of the authors, includes: The factors are derived in Rumbarger and Jones (1968); Houpert (1999); Rumbarger (2003); Harris et al. (2009); Breslau and Schlecht (2020); Houpert and Menck (2021), and they are used or discussed in some form in Schmelter (2011); Schwack et al. (2016); Münzing (2017); Wöll et al. (2018); FVA (2021); Menck (2023); Hwang (2023)

L164 Why are the various methods below this point introduced in reverse chronological order? Surely you should start with Hai et al. (2012) and work your way forwards in time to Menck (2023).

The review points out differences of Hai et al and Wöll et al to Menck, and hence it is necessary to start with Menck so it can be referenced in the text that follows. The approach by Hai does not build on the other publications, and the other publications do not build on it either, so the order of appearance can be chosen arbitrarily.

Kept Menck (2023) as the first reference but reordered the others.

L165 "whole bearing" does this mean that Menck (2023) calculates life for individual raceways and then combines these into a total bearing life afterwards? If so I'd suggest making that clearer in the description of Menck (2023) in the previous paragraph.

Yes, however the critical point here is that Menck (2023) starts with segments of a raceway which are even smaller than an entire raceway

Changed a sentence in the description of Menck to:

The individual survival probabilities of all segments can then be combined into raceway lives, which can be combined into a total bearing life.

Footnote 21 appears to contradict a sentence which follows, which seems to indicate equivalence only in cases of sinusoidal movements.

The following sentence points out they (Wöll et al) showed equivalence only for sinusoidal movements, but the footnote points out equivalence in all cases, because of the review author's understanding of the model

Changed the footnote and moved it to the end of the second sentence:

[…] The model is shown to be identical to a bin count using the Harris factor, cf. Sec. 2.1.1, for simple sinusoidal movements27.

[27] Even though they only show equivalence for sinusoidal movements, one can conclude that their numerical approach is equivalent to usage of the Harris factor for any type of movement if one evaluates the life and the corresponding movement of the bearing as shown in Sec. 2.1.6 with each time step used as a bin, and uses only the Harris factor.

L167 "For a stochastic time series, their numerical approach produces a shorter life than either Harris', Rumbarger's, or Houpert's approaches applied to a bin count." Where is this shorter life shown, can you add a reference or other evidence to demonstrate this?

Table 4 in Wöll et al (2018) and the sentence that follows ("Evidently, the numerical approach yields the highest failure probability and thus the lowest lifetime")

L173 "and conclude that using these bin counts "overestimates the lifetime for non-sinusoidal loads and speeds"" can you please comment on whether this conclusion is correct or not (I assume not as you indicate they use some erroneous formulations but this isn't made explicit either way).

It's difficult to comment which of these methods is more accurate because they all contain simplifications; their conclusion is obviously correct if their numerical model is used as a reference but there is no reason why it should be considered as more accurate than the other methods.

Added a sentence:

It is not possible to assess the accuracy of this statement because their model is based on the life of the whole bearing and thus also includes simplifications as pointed out by Sec. 2.2 of Menck (2023).

Also intensified the wording in Hai et al from "The simplifications make it difficult to establish" to "The simplifications make it impossible to establish"

L173 "They also produce a simple method to calculate an equivalent load for oscillating loads but it fails to take local effects into account as accurately as Menck (2023)." Can you please indicate where this is demonstrated (in Menck (2023) perhaps?).

Menck (2023) Sec. 2.2

L190 Typo "made make"

This was supposed to read as (The simplifications made) (make it difficult […]) but we removed the "made" now

Section 2.1.5: As mention previously, I feel there needs to be an earlier section describing all relevant real world effects in oscillating bearings, rather than encountering these various concepts one at a time and dispersed throughout discussions of simplified modelling/analysis approaches.

Added section 1.1 – Operational conditions for oscillating bearings

L195 "the orthogonal shear stress below the surface changes from maximum ($+\tau 0$) to minimum ($-\tau 0$)" There is quite a lot of complexity surrounding these stress components and how they behave, therefore please link to a reference which provides detailed information, e.g. A Review of Rolling Contact Fatigue by Sadeghi et al. 2009 (or similar).

Added (Lundberg and Palmgren, 1947; Harris and Kotzalas, 2007) because they both include the derivation for the standard $\tau 0$ value

L196 “This load cycle does not take place to the full extent at the outer ends of an oscillation cycle as depicted in Fig. 3.” I don’t think Fig 3 is explicitly showing this, it is maybe something which may be “inferred from” Fig 3, or is a result of the behaviour therein?

This was meant to read as “This load cycle does not take place to the full extent at the outer ends of (an oscillation cycle as depicted in Fig. 3)”.

Reading this now we agree this is confusing wording. Moreover, the information is already included in the following sentence when the central image of Fig. 6, blue case, is being referred to. Therefore changed to:

This stress cycle history behaves different in oscillating bearings: […]

L201 “None of these effects is considered in the ISO-based approaches named herein.” To help the reader, can you provide a little more to remind them which of the described approaches fall in this category (e.g. “this being all approaches outlined in Sec X.Y”).

Changed sentence to:

None of these effects is considered in the ISO-based approaches (all approaches covered in Sec. 2.1) named herein.

L205 “As far as the authors are aware, there are no simple models to estimate the thickness of the lubrication film as a function of the oscillation and thus determine its potential effects on rolling contact fatigue” You mention that poor lubrication might lead to wear (which is not fatigue), but then indicate that we are currently unable to determine the effect of lubrication on fatigue. Do you perhaps need to include an earlier sentence which indicates that as well as wear damage, lubrication also impacts rolling contact fatigue (although I appreciate that the lines can become fuzzy here, since surface initiated rolling contact fatigue can be argued to include some elements of wear…). Anyway, as written it feels a little confusing for the reader, so please consider a suitable revision in one form or another.

Added an introductory sentence:

Lubricant film quality is well known to have a significant impact on rolling contact fatigue life (Ioannides et al. (1999); and Kotzalas (2007)). The thickness of the lubricant film is affected by oscillation, […]

General comment: Throughout the paper, many complex concepts (e.g. subsurface stress time histories in rolling contact, lubrication and grease thickener effects, etc.) are briefly mentioned as if familiar to the reader, but without providing any references for further reading. I feel that this reduces the overall usefulness of the review to the general reader, and so encourage the authors to go back through and add in pertinent references for all such concepts throughout. Remember, many readers will have non-tribological/non-bearing specific backgrounds. In order to maximise the value and impact of this review, such readers should be provided with clearly signposted resources to learn more about concepts that may be important for them to consider within their wind-energy-meets-oscillating-bearings research.

Added more context:

As far as the authors are aware, there are no simple models to estimate the thickness of the lubrication film as a function of the oscillation and thus determine its potential effects on rolling contact fatigue. Most bearings are grease-lubricated (Lugt, 2009), including most pitch and yaw bearings (Becker, 2011; Wenske, 2022). Grease consists of, among other things, thickener and base oil (Lugt, 2009). Film thickness estimation would likely become even more challenging with grease lubrication due to the effect of the thickener.

L207 "Therefore, the effect of lubrication is mostly ignored in all models of which the authors are aware." ISO 281/16281 include some effects of lubrication in the modified life factors. I am aware this in no way accounts for oscillating behaviour, but since some accounting for lubrication is present I would ask the authors to be a little clearer in what is included and what is not. E.g. maybe rephrase to something like "Oscillatory effects on lubrication are mostly ignored…"

The sentence was supposed to only refer to models for fatigue life in oscillating bearings. Added context:

[…] mostly ignored in all models for rolling contact fatigue calculation in oscillating bearings […]

2.1.6 Binning: You have already talked about binning in some other contexts above, there is risk of equivocation of those various concepts and so I'd suggest a more descriptive title for this subsection.

We don't understand this point, this subsection talks about binning and the previous discussions did, too. Nonetheless, changed the title to "Binning for oscillating bearings".

L211 "The most accurate way to calculate the rolling contact fatigue life… according to the assumptions in Eq. 1 made by ISO related approaches… is to use the Finite Segment Method according to Menck (2023)" This is a fairly strong claim that is being made. Based on what I know of these methods I absolutely concede this is likely to be the case, but has that been demonstrated explicitly anywhere? To fully back up this claim, one would need to have experimental verification I'd think. Perhaps maybe a more qualified statement, such as "<u>It is argued that</u> the most accurate way to calculate… is <u>likely</u> to be the Finite Segment Method according to Menck (2023), because…"

The claim argues that Menck (2023) is most accurate according to the assumptions of Eq. 1, not necessarily in relation to test results. I.e. Menck (2023) is the most accurate method if one sets out to accurately apply Eq. 1 to an oscillating bearing. Therefore it is sufficient to have a theoretical argument that proves this claim. That argument can be found in Menck (2023), Sec. 2. Therefore changed to:

As argued in Sec. 2 of Menck (2023), the most accurate way to calculate the rolling contact fatigue life of a bearing under varying operating conditions according to the assumptions in Eq. 1 made by ISO-related approaches is to use the Finite Segment Method according to Menck (2023). This is because the Finite Segment Method considers local load changes rather than summing global, location-independent bearing damage over time.

L216 "Doing so for oscillating bearings necessitates the use of bins" This statement is ambiguous, bins of what? Please revise for clarity and specificity.

Changed phrasing to:

"Doing so for oscillating bearings necessitates the use of bins representing similar operating conditions in combination with oscillation factors."

L220 "along with a number of other assumptions made by Lundberg and Palmgren (1947)". Please provide references to more recent works which critique rolling contact fatigue formulations/assumptions (e.g. that of Erv Zaretsky, listed previously). This will provide readers with a more up to date critical analysis.

All the oscillation factors are essentially based on Lundberg and Palmgren (1947) though, so they contain the same simplifications, hence the citation. Furthermore, the argument here is supposed to state that they are an approximation as compared to the correct application of Lundberg-Palmgren Theory to an oscillating bearing, not compared to experimental results. (though a proper theoretical application of LP-Theory should obviously result in a better experimental result, logically, this claim is not being made here).

It is an approximation since the aforementioned factors have all been developed for constant oscillation amplitudes around the same mean position and they all assume there is a constant load acting on the bearing as it moves, along with a number of other assumptions made by Lundberg and Palmgren (1947), resulting in the life of a whole bearing, a process in which local information is lost.

L222 "Typically, variable load is taken into account in fatigue calculations by using rainflow counting" This is true for classical fatigue of structural components (e.g. beams etc.), but is not used for rolling contact fatigue. Since we have been discussing the latter, there is a risk here that the reader assumes rainflow counting is applied for rolling contact fatigue analysis also. Please provide further clarification here to avoid such confusion.

Changed sentence to:

Typically, variable load is taken into account in fatigue calculations by using rainflow counting (ASTM, 2017) for classical fatigue of structural components.

Equation 7: The notation here is not properly defined, e.g. please be explicit about what Ni, Pi and Pm denote – and please include this immediately following the equation itself.

Clarified

L231 References not provided in chronological order.

ordered

Footnote 30 and Lines 235-238: These both constitute information of additional approximations that either do or may form part of the analysis currently being described. However, there is no further context provided by the reader of the quality of approximation that either may represent. If further information on this (e.g. from analyses in the literature) may be given then please include it. At the very least, please point out that these additional approximations are increasing the uncertainty surrounding the rating life values we obtain (which themselves contain uncertainty as ISO fatigue equations are imperfect to begin with). This is fine, but I feel the reader should be made aware that these additional approximation come at a "cost".

Changed footnote:

[36] Strictly speaking, this equation only applies for a constant load direction, but it can be used as an approximation with some variations in the load direction, too, as proposed here. The same applies for Eq. 8. This increases the uncertainty surrounding the calculation result somewhat, which is explored in Sec. 4.4.

General comment: There are a lot of approximations and uncertainties being stacked on top of one another in these various methodologies. Given there is also a lack of experimental data to indicate how accurate any of these methods are, surely this provides at least one aspect of "critical future work" which should be highlighted?

Sec. 4.4 indicates, though, that there is not a huge difference between either of the methods; experimental deviations to any model are likely to be greater than differences in between the various models. Moreover, application of the Palmgren-Miner rule is common practice in industry (not just in wind energy but everywhere)

While validation is never a bad thing one must take into account that financial means for the limitation are always limited and may be better suited in areas where bigger question marks pop up.

L239 Notation in equations, again not defined or explained well.

Changed to

If it is not possible to determine P_i for each time step, potentially due to the calculation being too costly, it is possible to apply Eq. 7 to the force and moment components making up P (including radial force F_r, axial force F_a, and bending moment M) and to then determine P_m = f(F_r,m,F_a,m,M_m) from a suitable function[37] f() based on their values F_r,m,F_a,m,M_m for each bin (calculated as per Eq. 7, but using F_r, F_a, M instead of P).

[37] Functions f() for bearings with only radial and axial load components can be found in ISO 281 (ISO, d). Examples of a function f() for pitch bearings can be found in (Harris et al., 2009; Menck, 2020), where the latter publication is to be preferred.

Equation 8: Please provide one or more references for this equation. As previously mentioned, important background material is not being signposted for readers. Also subscript B missing on denominator phi.

Corrected subscript and added reference to (cf.also Zaretsky (1997); Kenworthy et al. (2023))

Equation 8: I think this may be in an unhelpful form… As written the phi have the same units as the life values. Hence, the total life of the bearing across all operating conditions (combined) is phi1+ phi2 + … + phiB. This means you actually have to know the complete life of the bearing to form that expression. But, if you multiply the top and bottom of the equation by 1/(phi1+ phi2 + … + phiB) you end up with a form in which you only need the proportion of time spent in each operating condition. Note – using the form you wrote out for a finite set of load cases, and where each phi is just the time spent in each, is essentially equivalent to the proportional approach I outline, but you have to make the implicit assumption that those load cases are proportionately representative of the lifetime conditions. Hence, I feel it better to explicitly include the concept of proportional time for the sake of clarity.

Changed to:

All of the bins $b = 1 \ldots B$ obtained are then typically combined into one final life using the Palmgren-Miner hypothesis (cf. also Zaretsky (1997); Kenworthy et al. (2023)) according to

$$L = \frac{1}{\frac{\phi_1}{L_1} + \frac{\phi_2}{L_2} + \ldots + \frac{\phi_B}{L_B}}, \quad (8)$$

where $L_1, \ldots, L_B$ denote the life in bin b. This may be either the life in oscillations, revolutions, or time. Typically, the life would be in oscillations if oscillation factors have been used but it may be converted to time or revolutions. L denotes the total, combined life of all operating conditions. The variable ϕ gives the proportion of oscillations, revolutions, or time performed in that bin. It is calculated according to

$$\phi_b = \frac{s_b}{s_1 + s_2 + \ldots + s_B} \quad (9)$$

where variables $s_1, s_2, \ldots, s_b, \ldots, s_B$ are the oscillations, time, or revolutions that occurred while in that bin, but must have the same unit as the lives in Eq.8. It follows that $\phi_1 + \phi_2 + \ldots + \phi_B = 1$.

L252 “the most accurate approach … is to use each single step… In order to account for oscillation effects, it would then be required to consider the larger oscillation cycle (amplitude) that a specific step is part of and adjust its life based on that, where the step will typically make up a fraction of the complete oscillation”. As per a previous comment, I’d again suggest a more qualified statement regarding what may or may not be the most accurate approach. Here I say this because you go on to outline how an additional approximation/interpretation of the data becomes necessary to implement in this way. Perhaps you mean accurate in terms of what we’re allowed to do under linear damage accumulation, in which case it may be possible to make a stronger claim. But, I wonder whether overlaying rainflow counted oscillations on top of varying loads moves us outside of where we’re strictly applying the linear damage rule as used by Palmgren-Miner? I am happy to take your lead on this, please just consider these points.

We would argue that the entire process of binning, strictly speaking, moves us out of a proper application of the Palmgren-Miner rule, cf. Sec. 2 of Menck (2023). But here the application of the Palmgren-Miner rule to a whole bearing is used as a reference, from which we attempt to deviate as little as possible. Changed text to:

It is worth noting that binning is solely used to reduce the number of data points from real-life data or a simulation. Using modern computers, if there is no hardware-specific necessity to reduce the number of data points, it is possible to use each single step taken from e.g., an aeroelastic wind turbine simulation or some other data set and treat it as a separate bin to which Eq. 8 is directly applied, rather than processing the steps into a reduced number of bins. From the perspective of a proper application of the Palmgren-Miner rule to a whole bearing, usage of each single step is the most accurate approach. It is thus both easier and less error-prone, as well as more accurate than binning beforehand

2.2 Non-ISO related approaches: The summary of existing literature/approaches is not in chronological order. Please start with oldest and work to newest unless there is a good reason for deviating from this.

Changed order

L260 “Individual loads are combined using the Palmgren-Miner hypothesis” Has the linear damage accumulation assumption (does it count as a hypothesis?) been explicitly introduced (in detail) somewhere in the paper, and linked to relevant references in the literature for the reader to learn more if necessary?

Yes, in Eq. 8, now with references

L262 “fatigue criteria such as Fatemi–Socie or Dang Van could also be applied” is it helpful to name these if they are not expanded upon? Maybe just say that they point out other criteria could also be applied. If you want to name them then please at least include a relevant reference for each.

Both of these criteria are sometimes used in other models so the fact that these specifically where mentioned may be interesting to some readers. Added references

L262 “They obtain empirical values used for the Palmgren-Miner hypothesis” empirical values of what?

Changed to: They obtain empirical values of the cycles to failure used for the Palmgren-Miner hypothesis from a test of a full-sized blade bearing

L264 “and further note that “a large number of tests are necessary for reliable results”.” Had they conducted such tests in their own work? Can you please comment in the paper as to whether they provide evidence that their approach works or is better/worse than others.

No, changed text to

However, they note that “fatigue criteria such as Fatemi–Socie (Fatemi and Socie, 1989) or Dang Van (Dang Van et al., 1989) could also be applied” in subsequent work. They obtain empirical values of the cycles to failure used for the Palmgren-Miner hypothesis from a test of a full-sized blade bearing and an assumed slope of the S-N curve from the literature. Further, they note that “a large number of tests are necessary for reliable results”, but that “currently, not enough tests have been carried out to determine a reliable service life” with their model.

L272 “They use orthogonal shear stress” the max value at each point in time?

Since they have a three-dimensionally discretized model (as stated in the text) there is no need to use the maximum value, they can use the actual orthogonal shear stress of each element over time and apply a rainflow count

L276 “the Weibull weakest link principle” Please describe and provide reference to relevant literature (possibly Zaretsky 2016 again, he calls it strict-series-reliability).

Added a reference to Weibull (1939) though Escalero just calls it the "weakest link principle"

L276 "The authors demonstrate their method for a reference case in which a blade bearing was tested." What was the outcome of this test/comparison? Did it provide a good prediction of observed life?

See Sec. 3, added reference to Sec. 3

L285 "The model is applied to rotating and oscillating bearings under constant operating conditions" Does this application imply anything regarding the efficacy of this approach? Please add some further comment or information.

We do not understand this question, how would this sentence imply anything beyond what it states?

L304 "none of the bearings show evidence of fretting corrosion" until now we have only seen mention of fatigue and wear damage. Why is this new type being included now, should it not appear earlier as well in that case? (I'd suggest including it in my suggested new section of real world information for oscillating bearings).

Fretting corrosion is a type of wear damage. Rephrased:

Despite the tests going as low as an amplitude of $\theta = 1^\circ (\phi = 2^\circ)$, none of the bearings show evidence of wear […]

Footnote 32: You don't say what x or b are in this note. We are also focussing on fatigue and not wear, hence this parameter has not previously been discussed and is arguably not helpful to readers not familiar with wear testing for oscillating bearings. I think this note can therefore be safely dropped. If it is to be retained then it should relate to material on wear introduced earlier so the reader can interpret the information in a useful way.

Added sentence:

Low values of $x/2b$ are often used to indicate wear potential (de La Presilla et al., 2023).

L311 "The test duration is equivalent to the L10 of the ball screws" is this the L10 for rotation or under oscillatory conditions?

It appears to be for a slightly modified version of the Rumbarger factor according to our best understanding. Changed text as follows:

The test duration is equivalent to the L10 of the ball screws, which Muenzing determines based on the simplified version of the Rumbarger factor found in NREL DG03 (Harris et al., 2009), cf. Sec. 2.1.2, which he modifies[40] to be equal to 1 for $\theta \geq 90^\circ (\phi \geq 180^\circ)$.

[40] The application of the Rumbarger factor in the reference takes place by changing the equivalent load P as done in other references (cf. Sec. 2.1.2) but his application, including his changes, are equivalent to those described here. The modification to a_Rumbarger = 1 for large amplitudes is presumably the result of a misunderstanding: Münzing claims the NREL DG03 to state that for oscillation amplitudes of $\theta > 90^\circ$ ($\phi > 180^\circ$), the influence of the oscillatory movement can be neglected and the life of a continually rotating bearing can be used for an oscillating one. This is not stated in NREL DG03 though, rather, it implies that the life of a rotating bearing and that of an oscillating one are identical in the case of $\theta = 90^\circ$ ($\phi = 180^\circ$) only, but not for amplitudes exceeding this value (cf. Harris et al. (2009): "The total stressed volume and number of stress repetitions per cycle are identical to a bearing in continuous rotation when [ϕ] = 180°")

General comment: Combining the discussions of deviations between oscillating and rotating bearings with experimental findings; it seems there is a natural expectations that oscillatory (fatigue) lives are longer than the equivalent rotating life (although wear is another animal!). If this is a reasonable inference then could/should this be given as an explicit interpretation? Section 3 currently lacks any concluding discussion which seeks to identify commonalities between experimental findings, if present.

It's reasonable to expect that the life of an oscillating bearing (measured in oscillations) is larger than that of a rotating one (measured in rotations); as stated in Sec. 2.1, a_osc is commonly in the range of 1…1000. We do not quite understand why you believe this follows from Section 3 though. There is no concluding discussion on commonalities because, frankly, most of the test results contain such a low number of tests that they are very difficult to interpret and there are not many commonalities between the few presented tests.

L330 “For the ISO-related approaches, recommendations are given according to the underlying physical phenomena considered in the derivations as described in this paper” So there is therefore an assumption applied that a method which explicitly includes a given phenomena is necessarily superior to one which doesn’t. This is a reasonable basis on which to make these recommendations, but it could also be the case that (for various reasons) a simpler method is superior to a more complex method, even if the latter includes more effects. For instance, this could occur if more complex interactions are present which perhaps counteract impacts from the physical phenomena which the more sophisticated methods attempts to capture. As a result, it is perhaps worth highlighting at this point that recommendations based on included physical effects alone is the best we can do at present, but not guaranteed to produce better results in all cases.

It is indeed possible that a simple model is more accurate or that a randomly generated function produces even more accurate results, but that would be completely accidental, so we don’t think it needs to be highlighted here. There is a chapter on test results which shows that there are only few published experimental results and that the experimental basis for any recommendation is somewhat shaky.

Footnote 33: please include relevant reference(s)

For the equation? The equation is the result of a back-of-the-envelope calculation, so there is no source, Harris (2007) and other publications contain similar equations, Menck (2023) also contains it but also without derivation; we could add a derivation but are unsure if this is useful

L341 “For the Rumbarger effect, based on Sec. 2.1.2 and App. A, the flowchart recommends combining this effect with the Houpert effect for non-axial loads (i.e., radial and moment loads).” The flowchart actually indicates this for non purely-axial loads (different to non-axial). The diagram is also a little confusing, since “yes/no” can be followed in two directions in many places, with no indication given as to which is preferential. Note for the legend entry with bold outline “for time series” isn’t super clear. I initially looked for fully white boxes and saw none, only after this did I realise it was referring to the bold outline. I’d suggest a clearer indicator (e.g. dashed green bold outline instead?).

Changed to “non purely-axial”

Changed outline of boxes to magenta color

Section 4.1 (General comment): There is fairly limited guidance on which methods to use provided here. For example, at the first stage-gate whether the answer is yes or no a total of 8 different methods are indicated, without any further guidance on which might be best. Based on familiarity with the various methods, the foundations of their development and extent of experimental validation (which I am aware is low in all/most cases) I’d hope the author’s might be able to provide a clearer path to delineating and selecting an appropriate method. It may be this is simply not possible at this stage. However, if that’s the case then perhaps the best advice is to stick as closely as possible to the ISO method (so favouring ISO-based methods), including only the necessary add-ons for the case one is dealing with. The logic behind this would be that design certification often require some chain of evidence/justification, which would best be provided by links to an international standard – at least until some other method is clearly demonstrated to be superior. I am very happy to be told I am wrong about this, but either way I think a more detailed discussion of deciding which method(s) to use would be useful.

We agree, added paragraph:

For general users seeking to apply a life calculation, ISO related approaches are preferred to non-ISO related ones due to their simplicity and the fact that there is much more empirical basis underlying them. In case of an invariant load direction and oscillation amplitude θ, various methods are shown in the figure. Among the ISO related ones, that by Menck can be considered to be most accurate, however, it is also complicated to apply. A less accurate (i.e., an approximated) but simpler method will be most useful for most readers. Among the approximated ISO related methods for an invariant load direction and θ, "Bins with Palmgren-Miner" is the recommended approach due to its wide use in many areas. Among the non-ISO related methods, Table 2 gives an overview of advantages and disadvantages of each method. Since only users with very specific aims will refer to these methods, it is up to readers to take their own decision as to which of these methods, if any, to use.

L365 "An exemplary cardan joint connects two shafts whose axes are inclined to each other" A simple diagram would be helpful here.

The introductory text really just adds some spice for the reader but is otherwise irrelevant to the calculation, hence, we think this is not necessary here.

L370 "According to Fig. 7" I think this would read better as "In the context of Fig. 7" At present it seems to imply that the time invariance is because of Fig. 7.

Changed as suggested

4.2 and 4.3 Applications: These are both very simplistic examples which basically demonstrate how to follow a flow diagram. Would it not be possible to go on to apply realistic loads for those components and example bearings in order to obtain life values, perhaps also comparing to what you'd get if a simpler formulation had been used instead of the recommended one?

A life calculation would not add a lot in our opinion, but the factors were now explicitly calculated and compared to each other for both cases. See updated manuscript.

L387 References given in reverse chronological order, please switch to chronological.

Changed order

L390 "Moreover, according to Sec. 3, the experimental validation for these models is still lacking. Therefore this section will focus on ISO-based approaches, which remain the most common life calculation methods for rolling contact fatigue." Here you seem to apply some of the logic I suggested including in your recommendations for which methods to use, so it seems we are somewhat in agreement in the underpinning logic at least.

Yes

L393 "Moreover, the load direction changes slightly, though mostly for smaller loads (Menck et al., 2020)" Can you please expand on this for clarity. Surely the gravitational loads are constantly cycling and hence driving large load direction changes a lot of the time? (Is this perhaps overcome by one of the other load components?). I'd also suggest including a fuller description of the load conditions of pitch and yaw bearings in my suggested expanded Background section on slewing bearings in wind turbines.

The main load component that drives the loading within the bearing is the bending moment M, which consists of two components, Mx and My. Mx is driven by gravitational loads but My at its peak load is bigger and therefore the resulting moment tends to be concentrated in the direction caused by My for high resulting moments. This can be seen in (Menck et al. 2020) Fig. 5: For high resulting moments M the load is more concentrated than for lower ones. And high bending moments are the ones that are driving the life (since P ~ M, approximately, and thus $L \sim 1/M^3$).

Changed to:

"Moreover, the load direction changes due to the Mx component of the bending moment caused by gravitational loads, though for bigger resulting moments, My is driving the direction of the resulting moment (Menck et al., 2020)."

L394 "Therefore, according to Fig. 7, the Finite Segment Method (Menck, 2023) would be the most appropriate ISO-based method for an engineer to use" This is not a given from Fig 7 alone. Yes the figure indicates the other ISO-based methods are approximate, but the figure and accompanying text never make the claim that approximate methods are necessarily poorer than the rest. If such guidance is being given, please include this in Section 4.1 more clearly.

Changed sentence in 4.1: "Dashed arrows represent mathematical approximations, which are considered less accurate than exact calculations."

L405 "is large enough to have rolling elements cover the entirety of the raceway at one point or another" reference?

Added a footnote with explanation:

[46] The entirety of the raceway is covered by rolling elements if for the largest amplitude θ_{max} done by the bearing, $\theta_{max} \geq \theta_{crit}$ is true. Since all pitch bearings perform 90◦ movements (Burton et al., 2011) (corresponding to $\theta = 45◦$), this is achieved in virtually all pitch bearings: Due to the rolling element diameter being small compared to the pitch diameter (Wenske, 2022), pitch bearings commonly have close to Z = 100 and more rolling elements and small values of γ. This means that for a four-point bearing as used in Menck et al. (2020), $\theta_{crit,i} = 2.48◦$ and $\theta_{crit,o} = 2.42◦$, values which are easily exceeded by a pitch controller even without taking the 90◦ movement into account (Bossanyi et al., 2013; Bartschat et al., 2023).

L414 "The five approaches are ordered with increasing accuracy to the right of the figure" Accuracy implies "more correct with respect to the ground truth", here I believe you are instead claiming accuracy under the assumption that linear damage accumulation is a reasonable approximation, please clarify this in the paper. Similarly for "This is the most accurate method and can be used as a reference for the others."

Changed sentence to:

The five approaches are ordered with increasing accuracy to the right of the figure, where "increasing accuracy" means that the Palmgren-Miner hypothesis is applied as accurately as possible.

Footnote 40: References?

Added references (Lundberg and Palmgren, 1947; Harris and Kotzalas, 2007; ISO, a)

Figure 8: Why not have L10 in years, that's much more meaningful to wind engineers. Similarly, results indicate rating lives of around 100 days or less, is this realistic? Please discuss.

Lives are given in h because, as you stated, they would be very low if shown in years. A discussion of the accuracy of calculated rating lives for pitch bearings is outside of the scope of this review and can be found in, for example, (Menck,2020)

L445 "The design of yaw bearings thus lends itself to binning, since detailed time series will typically not be available” This is due to a lack of information, rather than this necessarily being a route to accurate deign lives. I’d suggest rephrasing to indicate this is out of necessity only (while highlighting that further information on yaw time histories should be prioritised). Is this another point to include in “critical future work”, a better understanding of yaw bearing movement time histories?

More accurate information on yaw bearings would be preferable but we wouldn’t necessarily call this “critical” future work since yaw bearings (no source for this statement but hearsay) tend to cause much less issues than, say, pitch and main bearings.

Rephrased sentence to:

Since detailed time series will typically not be available, binning will often be necessary in order to calculate the life, though detailed time series would be preferable, if available.

L450 “Finally, the design of large scale yaw bearings, like that of pitch bearings, usually includes a large number of rolling elements in excess of 50 or even 100 and more per row, giving small critical angles” The critical angle will depend on both the bearing geometry/size as well as the number of rollers. Can you provide some further information or analysis to show that even for large diameter bearings this conclusion still holds? Additionally, do you have a reference for the numbers of rollers?

This is covered in the above footnote denoted 46; included a reference to that footnote

L464 “Since yaw bearings, like pitch bearings, are strongly affected by a tilting moment, a life which is around 10% shorter than that obtained with the Harris factor is to be expected.” I am not sure how the conclusion follows from the opening of this sentence, can you please elaborate for clarity.

Changed to:

Since yaw bearings, like pitch bearings, are strongly affected by a tilting moment, each of their raceways is commonly loaded around half of its circumference (Chen and Wen, 2012; Schwack et al., 2016; Menck et al., 2020; Graßmann et al., 2023), corresponding to a load zone parameter (cf. Sec. 2.1.3) of $\varepsilon = 0.4...0.6$. With this value of ε, a life which is around 10% shorter than that obtained with the Harris factor is to be expected for small oscillation amplitudes (Houpert and Menck, 2021).

L466 “If the main wind direction is truly evenly spread over all compass directions, it is permissible to use the equivalent load of a ring that rotates relative to the load for the outer ring, approximately equivalent to simply using the Harris factor” Is this because of rollers rotating internally? I am not sure I follow the logic here. In addition, this is a fairly strong claim which is being made. There will never be a perfectly circular wind rose, and no guidance is provided to serve as a cut-off for “evenly spread”. Is there any data or a short analysis which can be presented to help indicate when this is safe to apply? (At present it feels a bit hand-wavy).

The argument here is completely related to the logic of the Houpert effect, the effect of rollers is neglected for this argument. For a typical run-of-the-mill bearing, all circumferential locations of the rotating ring see similar load cycles eventually due to the ring’s rotation, whereas the stationary ring only experiences them concentrated in the same positions since the load direction is constant relative to the stationary ring. This flips for the given example: If the rotating (oscillating) ring actively follows the wind, it is always loaded in a similar position, hence it is loaded like the stationary ring in a run-of-the-mill bearing in most industrial applications. The outer ring, on the other hand, can see load all over its circumference (similar to a rotating ring in said run-of-the-mill bearing) if and only if the wind truly comes from all directions over the duration of the turbine lifespan. This is a very theoretical case and in most cases it will be more practical to assume that the outer ring also experiences concentrated loading, which is the more conservative approach.

Added a footnote:

[51] In this example, the behavior of a typical bearing is flipped on its head. Typical bearings in most

industrial applications experience concentrated loading on the stationary ring, since it is stationary with respect to the load. The rotating ring, on the other hand, sees loads all over its circumference (cf. Fig. 5, "rotation": All elements on a rotating ring are loaded like the example one, only with a time shift.). In a yaw bearing in which the wind comes evenly from all directions eventually during the turbine lifespan (for example, 25% of operational time coming from north, 25% from south, 25% from south and 25% from west), the outer ring is loaded in all positions at some point and thus experiences similar damage accumulation over its circumference as a rotating ring in a typical bearing. This is a very theoretical example to illustrate potential influences of the Houpert effect, in most cases, it will be easier to simply assume concentrated loading as discussed above, which is the more conservative case.

General comment: The paper feels like it's missing a section on "Current challenges and critical future work". This is a topic on which it feels like we are still at the beginning of its proper scientific exploration. This review should therefore provide a roadmap for overcoming current challenges and improving the rating life predictions for oscillating bearings.

This section has been added. See current manuscript.

L470 "Most of these approaches have been proposed and used in the literature without an explanation as to when they apply. The aim of this paper was to explain when which approach can be applied." This particular aim was achieved, but I feel there could be more guidance on how to select a method from the set of those which could be applied in any given case.

Now added in Section 4.1 as discussed above

L477 "All ISO-based approaches shorten the calculated life compared to the results using the Harris factor (or are identical to it) if applied correctly." I am not sure if the review paper provides a detailed discussion of why this is the case, is it that oscillation always means at least one part of the raceway is worse off than in an "equivalent" rotating bearing?

This is because all ISO based approaches are a combination of the Rumbarger and Houpert effect, both of which reduce life for different reasons that are discussed in their respective sections of the review. (For the Houpert effect, this discussion wasn't yet included, it has been added now)

Added footnote in Houpert section:

[23] It shortens the life in all other cases because, from a viewpoint of rolling contact fatigue, the even distribution of loads over time that is present on a rotating ring is the best case scenario for damage accumulation of a ring. Any oscillation that deviates from this loading causes increasingly more concentrated damage accumulation on selected locations of the ring. Concentrated loading (as present, for instance, on the stationary ring in a typical bearing in most industrial applications) causes a higher equivalent load, and thus a lower life, if all else is equal, than the loading of a ring that rotates relative to the load (Lundberg and Palmgren, 1947; ISO, c). Note that the Houpert effect is, however, expressed through a factor here, rather than by changing the equivalent load.

Added sentence to conclusions:

All ISO-based approaches shorten the calculated life compared to the Harris factor (or are identical to it) if applied correctly. This is because all ISO-based approaches that deviate from Harris do so because they either incorporate the Houpert or Rumbarger effect, or both, and both of these effects cause either the same life or a reduction in life compared to the Harris factor if applied correctly. ISO-based calculation approaches that increase life compared to the Harris factor are erroneous, potentially due to being overly simplified. Some phenomena described in this paper that have not yet been analyzed in the literature could slightly increase lives even for ISO-based methods.

L482 “Some results from the ISO-based approaches suggest that their predictions may be relatively close to the actual life” I’m not sure if “relatively close” was ever quantified. I assume you are referring to results in the experimental section? Please add some clarification here, or earlier in the paper.

Rephrased to:

Some experimental results from the ISO-based approaches compared well with the calculated life, suggesting that that the predictions of ISO-based methods may be relatively close to the actual life, while validations of the alternative approaches are mostly lacking.