Review of wes-2025-92 "The influence of wind veer on fatigue loading for large floating wind turbines with flexible drivetrains"

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Dear authors,

This paper seeks to investigate the influences of drivetrain flexibility and wind veer on wind turbine damage equivalent loads, considered here in the context of blades, tower and main bearings. It undertakes this analysis in the context of large offshore floating wind turbines (22 MW and 15 MW). Results indicate that wind veer can increase DELs by as much as 30-70% for some components under some sets of wind conditions, and that accounting for flexibility of the drivetrain can result in DEL reductions on the order of 6-22% under some conditions.

While this manuscript certainly contains interesting results, I believe extensive revisions are required before it can be deemed ready for publication. My high-level concerns are as follows:

1) The paper lacks a clearly defined and specific set of research goals or questions, along with a clear narrative around why we're interested in this particular set of components, phenomena and metrics. A variety of analyses are undertaken, but the current exposition only links them fairly tenuously. By linking to prior work, motivating what is undertaken, and more narrowly defining objectives the paper can become more focussed, and its contribution made clearer (specific instances of this are highlighted throughout my "specific comments" below).

2) Related to the above, various results return only minor sensitivity to the effects under investigation, whereas in some cases the effects are significant. I'd suggest focussing on the latter, and possibly digging deeper in those cases to extract more insight and value.

3) The paper lacks a thorough treatment of relevant background research and associated contexts. The result of this is a large prior-knowledge burden placed on the reader, and the risk of some readers mis-interpreting findings, results and implication.

4) There are various point in the paper where assumptions and modelling decisions need clarification or better explanation/justification.

5) Finally, I feel some results figures need enhancing (e.g. use all seeds rather than just 1), some seem not to add much value and so might be safely removed (spectra), and some additional plots need adding (mean load plots for MB results).

To be clear, I am confident that good work has been undertaken which will be of value to the wind community. But, I believe the current manuscript falls short of what is required from a research publication.

Specific comments

Abstract – some bold claims are made here which I'm not sure the results quite live up to. Please consider a slightly more measured summary of paper outcomes in the abstract

General comment – the title is maybe misleading, as it highlights veer as the headline effect under investigation. Please consider a more representative title for the work which is presented.

General comment – there seems to be a general conflation between classic and rolling-contact fatigue in the paper (or at least the distinction is never made). These are not the same mechanism!

General comment – some amount of drivetrain flexibility is modelled, and its effects on dynamics and loads evaluated. But, it is also important to appreciate that the bearing housing is not modelled. If this component deforms during operation then additional interactions may occur which shorten bearing life. So, when describing what new effects are modelled, I'd encourage you to also highlight those effects which remain un-modelled.

Line 21 – "With increasing turbine size, the applicability of current modelling approaches becomes more uncertain. Mechanisms that have been neglected in the design of smaller turbines may play an important role for larger wind turbines. Increased structural flexibility is one feature that introduces new demands on state-of-the-art analysis tools." Please add some relevant references to back up these claims. This is indeed a much considered topic and so there will be useful citations to include here.

Line 24 – "Another challenge emerges from the rotor extending deeper into, or even beyond, the atmospheric boundary layer (ABL), so that previous assumptions related to the wind profile and wind field can be questioned." Again, please use some of the many relevant references to back up your claims.

Line 25 – "In this work, both of these topics are investigated." I don't think it's valid to claim this analysis specifically considers extension beyond the ABL, and since kinematic (Kaimal spectrum) wind fields are utilised, I'd also advise being careful when making claims about how representative these simulations are of the ABL.

Line 27 – "Based on the assumption of a relatively rigid drivetrain, main bearing response has traditionally been obtained by two separate analyses. First, a global analysis, with the drivetrain represented by a torsional spring and damper, outputs shaft loads. These shaft loads are then combined with either analytical calculations or a detailed local model of the drivetrain to obtain main bearing loads. Analytical calculations are fast but complex to derive for bearings that have off-diagonal or moment-carrying stiffness terms. Local models are accurate but computationally expensive and time-consuming to develop." The text quoted here is, as far as I can tell, the sum-total of background discussion provided on main bearing modelling work. No references are given to back up any of the claims made and, therefore, significant volumes of prior work have been completely ignored. This is highly problematic and leaves the reader without any appreciation for the extant work in this field. An important implication of this is that the reader now also lacks any appreciation for the nuances which remain present in main bearing research, i.e. we don't yet have a clear picture of the dominant failure mechanisms and so much ongoing modelling and data analysis work is specifically in aid of identifying candidate mechanisms (internal to the bearing and/or as a result of inflow conditions and load characteristics). Further work in this domain (e.g. investigating possible veer influences) is only valuable if carefully couched within the full context of main bearing R&D efforts and current understanding. Additionally, in the current exposition, it's not clear that the "global analysis" you describe is the aeroelastic-code (FAST, Bladed, Hawc2 etc). Again, clearer writing and proper referencing is needed. For example, you describe the global analysis as representing the drivetrain by a torsional spring and damper; is this true of all/most aeroelastic codes, or is that just FAST?

Many relevant main bearing papers can be found in WES, Wiley Wind Energy, and Energies journals. The following seem particularly relevant (to be transparent, I am co-author on two of these. Entirely up to you which – if any – you include):

- Electric Power Research Institute (EPRI). (2024) Wind turbine main bearing reliability analysis, operations, and maintenance considerations. Technical report 3002029874. https://www.epri.com/research/programs/113055/results/3002029874
- Hart, E., et al. (2023). Main bearing replacement and damage − field data study on 15 gigawatts of wind energy capacity. Technical Report NREL/TP-5000-86228. https://docs.nrel.gov/docs/fy23osti/86228.pdf
- Kenworthy, J., et al. (2024). Wind turbine main bearing rating lives as determined by IEC 61400-1 and ISO 281: a critical review and exploratory case study. *Wind Energy,* 27(2), 179-197. (also see Zaretsky 2016 referenced therein)
- Sadeghi, F., et al. (2009). A review of rolling contact fatigue, J. Tribol., 131, 041403.
- Tallian, T. E. (1992) Simplified Contact Fatigue Life Prediction Model – Part I: Review of Published Models, J. Tribol., 114, 207–213.
- Feiyu Lu, et al. (2024). A quantitative approach for evaluating fatigue damage under wake effects and yaw control for offshore wind turbines, Sustainable Energy Technologies and Assessments, Volume 66, 103824.

Beyond the lack of exposition concerning prior main bearing literature and modelling work, there is also a lack of similar discussion concerning all components and analysis metrics used. To put this another way, you don't motivate why the study is focussed on blades, towers and the main bearing (when there is extensive literature motivating each as important), and there is no discussion of the nuances of the DEL metric (and for the MB the DEL is based on L10 values, which have another raft of caveats associated with them) and how informative/restrictive it is. These last points all represent significant limitations in our ability to translate simulated loading into expected real world lifetimes and outcomes for components. That doesn't stop the study being valuable, but you have to clearly communicate that these limitations are present. I'd also add that these discussion don't need to be lengthy, and can mostly highlight key points and then refer to relevant literature, but they must be present.

Line 39 – "Wind veer often stems from the influence of frictional forces on the force balance"… what forces balance?

"Veer can also be a result of inertial oscillation in the ABL or horizontal temperature gradients"

The above two quotes aren't very intelligible to someone not already familiar with these concepts, can you please develop these points so they may be more readily understood.

Line 61 – "This paper addresses the influence of increased drivetrain flexibility and wind veer on damage-equivalent loads (DELs) in the tower, blade root and main bearings for two floating turbines" There is no motivation given for why we're interested in drivetrain flexibility and veer in particular? Why these two phenomena in particular?

Building on the above point, I'd add that the introduction is not very well crafted as it currently appears. More specifically, there is a lack of structured narrative to help the reader follow why we're interested in these specific combinations of components and effects, and where we are relative to prior work. One structuring issues example is that the introduction covers drivetrain flexibility and upscaling, then main bearing modelling, then wind veer. We're therefore jumping between big picture and small details, and without ever linking drivetrain flexibility with veer. Please revise the structure and content of the introduction in order to clearly and explicitly arrive at well-motivated research questions that naturally lead into the study which has been undertaken.

Line 62 – “A coupled drivetrain-turbine model was built in OpenFAST… etc” This is Methodology or Background content, and should not appear in the Introduction. Indeed, considering this alongside previous comments, perhaps what this paper lacks most is a proper Background section in which past work and relevant literature can be properly presented and discussed.

Line 85 – “For such large rotors, it is generally recommended to account for large deflections in OpenFAST by modeling the blades using the geometrically exact beam theory in the module BeamDyn. Unfortunately, it was not computationally feasible to combine BeamDyn and the drivetrain model described in Sect. 2.1.2 for the floating turbines”. Isn’t this a major issue, given the current study is specifically looking to understand loading on large flexible turbines? What if the errors induced by poor blade modelling are swamping any relevant effects from the flexible drivetrain or veer presence? It isn’t enough to simply state that it wasn’t possible, you need to seek to verify what the impact of this modelling decision will likely be on results. Please consider how this can be done. My suggestion would be to run simulations of both turbine models with the standard/simple drivetrain representation using both BeamDyn and Elastodyn (modal repr.), and then compare blade, tower and main bearing loads coming from each. If those differences are “small”, then it provides some confidence that Elastodyn could be sufficient for the elastic drivetrain case. More generally, this simplification and its impact on results needs more thorough treatment in the paper.

Line 91 – “modified to include a flexible drivetrain, modeled together with the tower” This reads a little like the dynamic multibody interactions would only be between flexible drivetrain and tower. Is there also interaction between the flexible drivetrain and the rotor?

Line 102 – “and a downwind locating SRB” A locating SRB is necessarily double-rowed

(e.g. file:///C:/Users/edwar/Downloads/tpi_251_de_en.pdf). This, and any general discussion of bearings/bearing-types is important contextual information that is currently missing from the paper. Also, why were SRBs chosen, and how were they sized etc… (maybe those details are in a previous paper, but they’d still be relevant here).

Line 103 – “The diagonals of the main bearing stiffness matrices…” Stiffness matrices have not been introduced, there is therefore a high burden placed on the reader to already know about these concepts.

Line 103 – “The diagonals of the main bearing stiffness matrices were estimated using Schaeffler’s tool (Schaeffler, 2025) considering the force and deflection of the bearings for a number of different combinations of mean loads” Which bearings? If you have specific bearings being modelled, don’t you also have the dynamic capacity and other information which would allow you to calculate L10 lives directly, rather than relying on relative damage? If you can then I believe you should, as then you can also account for the site wind speed distribution and combine all load conditions into a single resultant L10 life. Additionally, that would remove ambiguity between which bearing is expected to fail first (if it fails from rolling contact fatigue).

Table 2 – you utilise stiffness cross-terms, but it’s not clear from the explication provided how they are utilised in the flexible drivetrain implementation. Please provide some context.

Line 109 – “Note that the main bearing stiffness estimates differ from those presented by Krathe et al. (2025b) due to updates in estimation methodology”. What updates? Are the new values better?

Line 110 – “Note also that there are large uncertainties in the main bearing stiffness estimates”. More fundamentally, bearing stiffness is a function of load. This follows from understanding that under increased load, more rollers are reacting the applied load. This is another point where the bearing context is absent, but would help elucidate the point being discussed. Was the validity of using a single linear spring to model the bearing under different load levels demonstrated in a previous paper? My concern it that the linear assumption could lead to large errors in some load cases. This is why in the past we’ve coupled our linearised spring model to a bearing contact model which estimates local stiffness as the load changes. For these same reasons, I’m not sure how informative the bearing stiffness sensitivity analysis is here, given the bearing stiffness will in fact change continuously throughout operation. Interpreting that sensitivity analysis should be done with these things in mind.

Line 116 – “glue-code (OpenFAST) time step of 0.0015 s and 0.0025 s, respectively.” Why these values? I assume for convergence reasons. Did you arrive at these (if so how?) or are they standard values?

Line 134 – If you’re interested in veer effects, it could obfuscate the results to also change the power law shear exponent. As it turns out the values across load cases appear to be almost identical, so I don’t think this is an issue. But if they were different it would make it difficult to discern what was driving any changes which were observed.

Equation 3 – similar to the above comment, if you’re interested in veer, don’t let TI vary between each load case! (Again, they don’t vary much so probably not a huge issue).

Section 2.2.3 – shouldn’t this be within the previous subsection on Environmental Load Cases?

Section 2.3.1 – Please highlight here the fact that DELs allow for comparison between how damaging two sets of conditions are, and that they don’t translate to a life estimate for that component. Also important to highlight that the concept for Loads is borrowed from the concept of Damage Equivalent Stress. Importantly, there is no good justification for reformulating the latter as the former, and that this is only truly valid if stress is proportional to the applied load. For turbine blades and tower this is highly likely to be true most of the time. DELs should therefore be highlighted as an indicative tool at best, and one for which we don’t currently have a good alternative in general.

Line 171 – “Neq is the equivalent number of load cycles until failure” Under the calculate DEL

Line 173 – “The Wöhler exponent is taken as 10 for the blades and 4 for the tow” reference?

Line 181 – “These factors were estimated based on product catalogs” Earlier it seemed like you had known bearings from which you were modelling stiffness. Do you not in fact have that? What is it specifically you are getting from catalogues, I’m guessing the contact angle, or fatigue load limit (e)? Just to be clear, if you look in ISO 281 Table 8 it gives you a formula for e as a function of contact angle.

Table 7 – For the non-locating bearing Fa = 0 and (assuming contact angle is 0) e=0, so the relevant row should read 1, 0, NA, NA, 0.

Section 2.3.2 – this treatment of MB DELs lacks any discussion of the L10 life, ISO 281 formulations, or any of the surrounding context, caveats and uncertainties tied in here. Whether or not rolling contact fatigue (surface or subsurface initiated) is a dominant failure mechanism for main bearing in wind turbines remains unknown, beyond that it also remains unclear to what extent ISO lifing methods provide accurate rolling contact fatigue lives given the size and complex nonsteady load conditions of these bearing (especially in large wind turbines). Beyond that, rolling contact fatigue for bearings is expressed as an L10 life, i.e. the time after which 10% of a population are expected to have failed. Another critical factor is the fat that, due to the nature of rolling contact fatigue and the ISO 281 method, main bearing L10 values are driven by the mean MB load rather than load fluctuations (discussed in Kenworthy 2024, listed previously). These are all important and occasionally subtle points which should accompany any application of these methods, to avoid misleading people to believe these metrics can be used to confidently assess main bearing longevity. DEL formulation does allow one to dispense with the bearing dynamic capacity (Cd), but again this should be highlighted to the reader via a proper discussion. Finally, equation 7 is simply presented without any derivation or explanation. Finally if t_i is simply the timestep, then no subscript "i" is necessary.

Section 3.1 – is this a results section? Surely it's more appropriate within the methodology or an appendix?

Line 190 – "Aerodynamics were omitted and mooring lines were replaced by linear springs to facilitate detection of modes. Rigid-body mode natural frequencies were found through decay tests." These statements are made without context, justification or supporting references (if applicable). Again, a high burden is being unfairly placed on the reader here.

Figure 4 – Why are we only looking at results for a single seed? Pleased instead develop plots which summarise information across seeds. For example you could easily plot the mean, std, max and min across each set of 6 seeds at each wind speed. This would provide a more robust picture of impacts here.

Line 218 – "It is difficult to compare main bearing loads between the rigid and flexible…" It feels like we're back into methodology here, why isn't this discussion had in the methodology section?

Line 220 – "As a proxy for main bearing loads, the standard deviations of the shaft axial load and pitch and bending moments are compared among the two models". As mentioned previously, MB rating life (L10) and hence DEL_MB is principally driven by the mean MB load and not by standard deviations. This is counterintuitive, and why a detailed treatment of bearing and rolling contact fatigue complexities are essential. Please undertake the same analysis (summarising across all seeds) for the mean loads as well as std.

Figures 4 and 5 – When revising these figures, please use the same fixed axes on all plots in order to make them directly comparable to each other without careful study of axis scaling.

Section 3.2.2 – This section is more of a verification that the uncertainty around bearing stiffness is likely not driving large errors in the results. As such I'd suggest moving it to an appendix, as it distracts from the main focusses of the paper.

Figure 7 – as mentioned above, it is mean loads that are mostly going to drive your MB DEL. Please also develop figures showing changes in mean loading as you change bearing stiffness. This should include Fa, Fr and P values. As above, it seems this has only been plotted for a single wind seed at each point, please therefore also represent results for all seeds here as well.

Line 254 – what is the logic behind including a case for shear and veer without turbulence? I can't see why that adds anything?

Section 3.3.4 – I'm not entirely sure what this section adds? Certainly spectra weren't mentioned in the methodology section. If there was going to be a discussion of control implications then perhaps these spectra would provide some additional value, but in the current paper version they feel unnecessary.

Line 312 – "Wagner et al. (2010) illustrated how the angle of attack and relative wind speed seen by the airfoil changes with the azimuth position of a rotor blade. For a simple rotor at 8 m s−1 without blade pitch, for veering wind, they found that the angle of attack was larger than for uniform wind while the relative wind speed was lower. The opposite was true for backing." Why is further background information being added into the results section? Surely this should have been discussed in Background and then the analysis foreshadowed in Methodology.

Line 360 – is a 6.5% reduction for the 15 MW turbine to be described as a "significant reduction"?

Conclusions – some threads are pulled together here, but overall the paper feels uncertain as to its specific purpose and aims. I think the whole narrative needs to be clarified and tightened up throughout.