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Title: Damage Equivalent Load Synthesis and Stochastic Extrapolation for Fatigue Life Validation

The paper presents two approaches in the overall context of extrapolations of damage equivalent loads (DELs) of wind turbines. The first approach is the "reconstruction" of DELs based on aggregated measurement data, i.e. mean, max/min and standard deviation data of loads. For this purpose, the variation of DELs is modelled using a Gaussian process. The second approach is the subsequent extrapolation to long time periods. This extrapolation is either based on the reconstructed DEL data or on limited simulation data. For the extrapolation itself, a 3-parameter Weibull distribution is fitted to the limited data to extrapolate extremes, e.g. 1-year, DELs.

Although the topic of fatigue damage extrapolations is definitely a really relevant topic, this work has several severe drawbacks that have to be improved before a publication in WES might be possible. Moreover, explanations and details regarding the methods are missing.

<u>Major points:</u>

1) The use of a 3-parameter Weibull distribution for extrapolations of fatigue loads has already been conducted by Moriarty et al. "Extrapolation of Extreme and Fatigue Loads Using Probabilistic Methods" in 2004. They fit the distribution to the cycles directly and not the 10-min DELs. Nonetheless, this work and subsequent publications on fatigue damage extrapolations have to be discussed. Otherwise, the innovation of the "extrapolation part" of this work remains unclear.
2) For me, the relevance of 1-year extreme DELs (or even longer return periods) is not clear. Surely, high cycles tend to dominate the overall fatigue behaviour, especially for blades featuring a high material exponent m. Nonetheless, especially for steel components, a single high DEL is normally not design driving. For the overall fatigue, the sum of DELs ($\sum DEL^m$) is important. Hence many medium DELs can be much more relevant compared to a single high DEL, if m is moderate. For example, in Fig. 5, the underestimation of DELs around 1 might lead to an underestimation of the overall DEL. Please, comment on this and show how good $\sum DEL^m$ is approximated. For blades with high m, I believe that this procedure will give accurate approximations for $\sum DEL^m$, but for steel components with low m, I doubt it.
   Actually, in line 40-42, you already state yourself that DELs are quite stable with respect to outliers. The same applies to the aggregated DEL with respect to single high 10-min DELs.
3) The assumption that the underlying stochastic process is a Gaussian process is a strong assumption. It has to be justified somehow. For example, the variation in loads might change of time (no longer a stationary Gaussian process) or it is due to several superimposed effects (e.g., different Gaussian processes for different wind directions).
4) Regarding the simulations, many information are missing. For example, in line 145-150 or line 185-189: how many simulations for the fitting?, what is changed (seed only or wind shear, turbulence intensity, etc. as well)?, settings of the aero-elastic model?, …
5) L. 156 and Fig 3 and 4: You state that simulated DELs do not feature the same variations. However, if we look at Fig. 3 and 4, for some wind speeds (e.g. 23 m/s), the variation is even higher for simulation data. Hence, the "larger variation" for measurement data for medium wind speeds, which is visible in Fig. 3 and 4, could also be only due to more data points, i.e. visual effect. Please comment on this. It might also be useful to actually determine the variation (overall and for different wind speeds) by calculating mean values etc. This would exclude visual effects.

6) If I understand it correctly, GPA is mainly relevant, if load measurements are available, but neither times series nor cycle counts are stored, but only mean, max/min, and std. values are stored. If this is the case, this has to be stated more clearly in the abstract and/or introduction. This is a strong limitation and makes the approach much less relevant. In most cases, either no load measurements at all are available or time series are available.
7) L. 227: For me, it is not really clear how you determine the DEL using Eq. 4-7. Some more details would be nice, e.g. how exactly do you determine σ_L?

<u>Minor points:</u>

1) Citations for Eq. 2 and 4 would be nice.
2) L. 115: What value is used for α in this work?
3) L. 130: The assumption of using a 3-parameter Weibull distribution has to be justified.
4) L. 132: Additional explanations regarding the fitting process are required. If I am correct, Hoole et al. (2019) do not use suggest this method for the fitting itself, but for validation purposes. Moreover, in Section 4.1, you also state that you use the median rank for the validation. Hence, the question how you conduct the fitting itself arises.
5) Eq. 8: I think, the equation is incorrect. The outer brackets have to be removed.
6) L. 147: Why does Fig. 1 show that the "extrapolation is stable". The figure shows a fitted distribution. This has to be smooth. Either more information are needed or the statement should be reformulated.
7) Fig. 2: Please indicate the investigated turbine.
8) L. 170-172: Can you somehow show that the blade root moment is narrow band and the tower moment is very narrow band? Otherwise, the assumption remains mainly unjustified again.
9) L. 195: Why am I interested in extreme DELs and not aggregated DELs (cf. major point 2)
10) L. 198: How many measurements are a "small sample"?
11) L. 198: "over all turbulence levels": How do you guarantee that all turbulence levels are covered? Moreover, is it realistic that measurement data is available for all turbulence levels, if you state previously that you only use a "small sample"?
12) L. 205: This procedure of weighting gives you somehow a maximum lifetime DEL. However, this is not directly correlated with the lifetime (cf. major point 2)
13) Fig. 5 and 6: The difference of these two figures is not clear to me. Is Fig. 6a just an extension of Fig. 5 for more wind speeds?
14) Fig. 6: A legend for the wind speeds would be nice
15) Fig. 6 and l. 210: I cannot really see that there is a good fit. Perhaps zoom in; make clear which measurement data corresponds to which extrapolations; reduce the number of curves in the plot; etc.
16) L. 229-231: You propose that simulation data can be used to determine the required data for GPA. However, details regarding the precise approach are missing, e.g., how much simulation data is required. Moreover, the question arises, whether this leads to a better extrapolation compared to the direct fit of a Weibull distribution to simulation data. Only if this is the case, the use of GPA in combination with simulation data is useful.
17) Fig. 11: Similar to Fig. 6, for me, it is not really clear how good the fit is. Especially, it is not possible to see whether GPA or the Weibull fit leads to better results. Please rethink the figure.

Editorial changes, syntax, typos, etc.:

1) L. 16: The dot before “Dimitrov at al.” has to be removed
2) Eq. 1: Neq is not explained and “eq” should be an index
3) L. 109: “Eq. (4)” and not “equation 4”
4) L. 116: “std.” and not “std,”
5) Eq. 7: “eq” should be an index
6) L. 166: “turbine designs” not “turbines designs”
7) L. 191: “load simulations” not “loads simulations”
8) L. 239: “$\alpha \rightarrow 1$” not ”$\alpha - > 1$” if I understand the statement correctly