"Probabilistic lifetime extension assessment using mid-term data: Lillgrund wind farm case study" (Manuscript number: wes-2024-68)

In this work, a probabilistic lifetime assessment of a wind turbine rotor blade is conducted. Three different approaches to determine the turbulence are compared: a standard IEC approach, the Frandsen model and using real measurement data. Furthermore, comparisons of the simulation results with real strain gauge measurements are done.

Fatigue assessments are an important topic in the context of wind turbines. Conducting them probabilistically is not yet state of the art and an important research topic. Nonetheless, in its current form, the manuscript is not sufficiently structured, explanations are missing and it features some mistakes. Hence, without a major revision, it is not suitable for a publication in the WES journal.

Comments:

1) The structure of the paper must be improved to make clear what the main innovation/topic is. Currently, it seems to be a mixture of "probabilistic fatigue assessment", "validation using real data" and "turbulence modelling".
    a. If the main topic is the probabilistic fatigue assessment, what is the difference between this paper and Mozafari et al. (2023) "Sensitivity…"
    b. If the main topic is the validation using in-situ data, more information regarding the measurement data must be given. Furthermore, in this case, a clearer focus on the results based on measurements and less work on simulations would be needed.
    c. If the main topic is the turbulence modelling and its effect on the turbine reliability (I think that this is the idea), the title, abstract and introduction must state this clearly.
2) Abstract: It remains unclear what the topic paper is (see comment 1)
3) In my opinion, the title of the paper does not represent in main topic of the work. Perhaps, turbulence modelling can be included in the title.
4) Introduction: The connection between the assessment using the Frandsen model (simulation-based, l. 24-44) and the limited data (measurement-based; l. 46-50) is unclear.
5) The state of the art (L. 52-67) is not sufficient and does not clearly differentiate between simulation-based and measurement-based approaches.
6) L. 121: Where exactly is the met mast situated? Please, show it in Figure 1.
7) L. 121: Are shadow effects of the met mast considered, e.g., reduced wind speeds if the anemometer lies behind the met mast.
8) L. 121: At which height(s) is the wind speed measured?
9) L. 124: Your data is biased, as you only cover periods in the winter/spring. This should at least be discussed. Is this bias relevant for your work?
10) L. 131: How much data has been removed?
11) Table 1: It is not clear for which time period the wind direction bin probabilities are given. Are these the probabilities for the same five years? And are they used somewhere. If yes, please highlight it. If not, you might just remove them.
12) Section 2.3.2: Your measurements come from an offshore turbine. The simulations seem to be done for an onshore turbine or all details regarding the offshore part are missing. Just simulating an onshore turbine and comparing it to offshore measurements does not seem to be sensible, even if you focus on blade loads.
13) L. 159: The site-specific turbulence distribution is not given, but only the reference turbulence intensity.
14) L. 162: How has the exponent of 0.1 been determined using in-situ measurement data?
15) Table 2: Why are the cut-in, the rated and the cut-out wind speed different compared to the real turbine (Section 2.1)?

16) L. 174: For groups 1 and 2 you use Rayleigh distributions (covering wind data of full years) whereas the biased measurement data (see comment 9) is used for the strain gauge-based approach. Hence, a direct comparison, as in Figure 7 is not possible.
17) Eq. (3) and (4) are not sufficiently explained, e.g., $di(\theta)$
18) Section 2.4.2: Formatting and explanations are not sufficient, e.g., I_y and not Iy, N_s is not explained etc.
19) Eq. (8) and (9): At the left side of the equation, the expectation E has to be removed, as $DEL_{lifetime}^{m} = E(DEL_{10min}^{m})$ and not $E\left(DEL_{lifetime}^{m}\right) = E(DEL_{10min}^{m})$
20) Eq. (9): Index i is missing.
21) L. 240 and l. 247-264: For me, it is not clear, why we need all this. If I understand it correctly, you fit a distribution to the 10min values (step 1). Then, you sample from this distribution to determine the lifetime value (step 3 and 4). Why do we need the DELs with long return periods. A single DEL with a high return period does not influence the overall lifetime DEL. Hence, they are not relevant and actually not used for the reliability assessment in Section 2.4.4.
22) L. 245: You neither show the fitted distribution for the lifetime DEL nor you state what type of distribution it is.
23) Eq. (10) where does this equation come from? It does not exactly match with Eq. (12), which is frequently used in literature.
24) Eq. (11): This equation is wrong, as it gives negative probabilities, since the CDF is always between 0 and 1.
25) Eq. (14) to (17): Please, revise these equations, as they are not always correct, formatting has to be improved and explanations are missing, e.g., Δt and $\mathrm{P_f}$ are not explained, it has to be $\mathrm{I_y}$ and not I, the left side of Eq. (16) has to be $\Delta P_f(X, t + \Delta t)$, m not R etc.
26) L. 289: Why do you apply FORM and not MCS? Your limit state function can be evaluated computationally efficiently, so that MCS should not be a problem and MCS is more accurate.
27) L. 308: How do you define “enough data”?
28) L. 313: You state that the Frandsen model and the ICE design underestimate the turbulence for low wind speeds and overestimate it for high wind speeds. I cannot see this in Figure 14, e.g., the Frandsen model is above the 75% quantile for 4m/s and below the same quantile for 20 m/s.
29) L. 330: Why do you investigate this type of multi-modal distributions and not others?
30) L. 334-344 and Figure 4 and 5: Why do we need this? For Section 3.3, it is not needed.
31) L. 336: You state that “the probability of the largest data observed” corresponds to five years. However, this is not correct, since you do not have data of five full years.
32) Table 4: How did you determine the sensitivities?
33) Table D1: How are the parameters of the different distributions defined?

<u>Typos etc.:</u>

34) L. 69: “assess” not “assesses”
35) L. 86 and others: “Section” and not “Sect.” or “section”. Same applies to “Eq.”, “Table” etc. Or at least be consistent.
36) L. 133: “in Table 1” not “in 1”
37) L. 138: I think it is “D1” and not “D2”. Overall, reference to figures in the appendix are not always correct.
38) L. 174: “Rayleigh” not “Reighley”
39) L. 241: 365×24 … not $365 * 24$ …
40) L. 346: “in Fig. 6” not “in 6”
41) Figure 6: I_{ref} not Iref
42) Table 4 (and appendix): Do not use the notation 7.62e-3, but 7.62×10^{-3}
43) L. 392: “fatigue” not “Fatigue”
44) L. 419: “h and more”?

45) L. 446: “In the following sections, we compare the turbulence levels in three scenarios of the study”?
46) Caption of Table D1 has to be corrected.
47) Caption of Figure D2 has to be corrected.