Response to referee #1 (Manuscript number: wes-2024-68):

We appreciate the detailed comments from the reviewer as we believe implementing and addressing them have improved our paper to a high extent.

The reviewer notes and comments are presented in black, and our corresponding responses are presented in blue.

"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.

Thank you for sharing your thoughts which shows that other readers may also face unclarities based on the structure of the pre-print. Thus, we have updated the abstract and introduction to make it more clear for the reader and hopefully answer all the above questions. Below explanation is the response to comment #1 and it is also added to the introduction in its current form:

'When it comes to lifetime extension of wind turbines in a wind farm, one must re assess the service lifetime by replacing the design assumptions with the conditions experienced in the site. In such re-assessments, normally, fatigue is the main subject of interest because of its direct functionality of time. The information about lifetime in site can be gathered in different manners based on data availability. Some of the common scenarios are as below:

1. *In case only free-stream turbulence measurement is available, one can estimate the waked turbulence in each turbine's location using simplified models like Frandsen (Frandsen, 2007; Frandsen and Madsen, 2003), suggested by IEC 61400-1 (2019) for site suitability checks. The corresponding estimations are then used to perform aero-elastic simulations. Site-specific lifetime can be estimated using the resulting fatigue loads.*

2. *The turbulence measurements in the turbine's specific location might be available. In such scenario, one can use the measurements as inputs to the aeroelastic simulations and perform fatigue assessments. The time at which fatigue reliability reaches the target level can then be derived.*

3. *In some cases, the structural response (load/displacement) measurements are available for a limited duration of the lifetime in a specific hotspot. In case Supervisory Control and Data Acquisition (SCADA) also exist, one can form a digital twin for deriving the loads in other components/locations. On the other hand, direct utilization of the measurement data for assessing lifetime extension is also an option. However, the later involves challenges like spatial and temporal extrapolations.*

Often the structural response measurements in the site are owned by the turbine manufacturer and are not accessible for the wind farm owner/developer. In addition, the measurements are not gathered for a long time or in many locations. The purpose of the current research is to showcase the differences of lifetime extension assessment in different scenarios (with and without load/displacement measurements) using a case study wind turbine for which all the above-mentioned scenarios are feasible. Additionally, the study tackles two common challenges in scenarios with and without structural response measurements. First, it addresses the question of performance of the Frandsen model—as a simplified approach for estimating enhanced turbulence due to wakes— in a compact wind farm layout. Second, we present a method for statistical extrapolation of mid-term strain gauge measurements for estimating long-term fatigue loads.'

The rest of the introduction together with abstract and conclusion are also edited and modified accordingly to make the purpose and outcomes clearer.

2) Abstract: It remains unclear what the topic paper is (see comment 1)

Abstract is updated now to clarify the main intention of the research and outcomes.

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.

Thank you for sharing your thoughts. The title is now changed to '*Added value of site load measurements in probabilistic lifetime extension: a Lillgrund case study'* to better represent the main purpose of the paper (see response to comment #1).

The waked turbulence estimation is not the main purpose and is one of the two additional results (as mentioned in the new introduction). Thus, we keep it outside of title to prevent possible confusions.

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.

The whole introduction modified now to better represent the bigger picture and the full purpose of the paper with connecting different pieces.

5) The state of the art (L. 52-67) is not sufficient and does not clearly differentiate between simulation-based and measurement-based approaches.

Introduction rephrased and updated now.

6) L. 121: Where exactly is the met mast situated? Please, show it in Figure 1.

Added in figure 1 now with reference in L. 124. Thanks for mentioning.

7) L. 121: Are shadow effects of the met mast considered, e.g., reduced wind speeds if the anemometer lies behind the met mast.

This is a very relevant point. Thank you for mentioning. The met mast used for measurements of wind speed is placed on a pole on the top of the tower and thus there is no shadow effect included. The information is now added to the text (lines 124 and 125 of the updated paper) for clarification with an additional reference to the report on meteorological conditions of Lillgrund which includes more details.

8) L. 121: At which height(s) is the wind speed measured?

65 meters- added to the text (in L. 125) now for clarification.

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?

If you are referring to figure B1, it is misleading as it shows DEL versus time while time is in a special format of 'yyyymmddtttt' (tttt being the time for ex: 1130 means 11:30). Although the data are not continuously measured for the full 5-year period, they cover months #10, 11, and 12 in 2008 and all months in 2009, 2010, 2011 and 2011. Thus, although the duration is not fully covering 5 years it is representative of all the seasonal variations. The figure is replaced by a text explaining the data for more clarity. The explanation added is as below (as reference):

'*The measurement campaign has been running in 5 years but not continuously. The data covers about two years in terms of duration length. It includes different timings in the last 3 months of 2008 and all the months in 2009, 2010, 2011 and 2012. <u>Thus, some data with a return period of 5 years are included among measurements</u>.*'

The last sentence of the explanation above is also a response to comment #31.

10) L. 131: How much data has been removed?

88031 data remained. How many data did we have is unclear and unfortunately, we do not have access to the data anymore. A line is added to the end of the paragraph:

'*A total of 88031 data points remain after the filtration.*'

11) Table 1: It is not clear for which time 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.

The table shows the number of data points in 5 years (added clarification in the text). It is presented to show how the available data can represent the probability of each wind speed bin presented in another work (referenced in the table).

Yes, the values of probability are used to weight the DELs of each bin.

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.

Thank you for your relevant comment. The results of the load measurements on the channels show a good alignment with the measurements (Figure A1 for mean load values and table A1 for standard deviation of the load) and thus are reliable for the load channel under study. However, we agree that there is a weakness of the current work and must be emphasized more clearly in the discussions (unfortunately we have missed this important point in the current version). Explanation added now in the discussions (in point #2 in 'discussions').

13) L. 159: The site-specific turbulence distribution is not given, but only the reference turbulence intensity.

There is a mistake in the two columns which is now corrected. A description as below is also added to the ending of 2.3.1 for clarity:

'*In the current case, different distributions best describing the turbulence in each wind speed in the free stream is used. However, we do not present the details of those fits to be concise*.'

14) L. 162: How has the exponent of 0.1 been determined using in-situ measurement data?

It is not based on measurements. It is an estimation based on smooth terrain (open water) condition of the offshore wind farm. This description is added now to the text for clarity. A new reference for shear exponent estimation based on lidar measurements is also added ('Liew J, Göçmen T, Lio AW, Larsen GC. Extending the dynamic wake meandering model in HAWC2Farm: a comparison with field measurements at the Lillgrund wind farm. Wind Energy Science. 2023 Sep 8;8(9):1387-402.')

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)?

That was a mistake, and the table is corrected now.

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.

The purpose of the current research is to illustrate different scenarios and show how different the results can look like for the wind farm developers in real scenarios. The current case study, the available measurements and the generic model in hand are all representative of the common case scenarios (in fact one of the best availability of data). The purpose is not to differentiate between different theories for a theoretical case but showcase real scenarios of assessment.

17) Eq. (3) and (4) are not sufficiently explained, e.g., $di(\theta)$

Thank you for your comment. A description of unknown parameters including necessary references is now added to the equation and a footnote: '*For further details and derivation of the equations 3 and 4, see (Frandsen, 2007) and (IEC 61400-1, 2019)*' is also added now.

18) Section 2.4.2: Formatting and explanations are not sufficient, e.g., Iy and not Iy, Ns is not explained etc.

Corrections on formatting applied and further explanations added now.

19) Eq. (8) and (9): At the left side of the equation, the expectation E has to be removed, as $DELlifetime\ m = E(DEL10min\ m\ )$ and not $E(DELlifetime\ m\ ) = E(DEL10min\ m\ )$

Agreed (as the result would just be a realization of DEL_lifetime^m based on the number of DEL_10min realizations it may get close to the estimated value). Thus, both equations are modified and corrected now.

20) Eq. (9): Index i is missing.

It is relatively small; however, it is there. Parentheses are added to make it clearer.

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.

The effect on the mean value will be small but not zero. The importance is discussed in the literature review in the introduction. However, for the sake of clarity, a reference to Mozafari et al. (2023a) and Mozafari et al. (2023b) is added -to show the necessity of such investigations- as below:

'(For reference to the importance of statistical extrapolation in estimation of DELlifetime please see (Mozafari et al. (2023a)) and (Mozafari et al. (2023b))'

22) L. 245: You neither show the fitted distribution for the lifetime DEL nor you state what type of distribution it is.

This line is a part of description of the general methodology. The distribution of the DEL based on the current data is shown and discussed later in results.

23) Eq. (10) where does this equation come from? It does not exactly match with Eq. (12), which is frequently used in literature.

Reference and extra explanations are now added (lines 280-286).

24) Eq. (11): This equation is wrong, as it gives negative probabilities, since the CDF is always between 0 and 1.

The 'Log' sign was extra and is now excluded- Thank you for the correction.

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 Pf are not explained, it has to be Iy and not I, the left side of Eq. (16) has to be $\Delta Pf\ (X, t + \Delta t)$, m not R etc.

Revisions are made as below:

1. R is correct. Description added in parentheses.
2. Eq 6 is corrected now.
3. Formatting of eq. 4 is improved with addition cross signs
4. Δt and Pf and other parameters in equations 16 and 17 are now explained.

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.

Reference to the reasons for choice and the comparison for a similar case is provided in *(Mozafari et al. (2024))'*

27) L. 308: How do you define "enough data"?

Text modified as: '*The plot of each direction bin only includes the mean wind speed bins in which there are enough available data to cover the comparison (more than 20 points)'*. This choice is very qualitative, as some bins had very few data (even less than 10) because of low probability of occurrence.

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.

We assume the reference is Figure 2. Agreed that the wording must be corrected. Below correction is made:

'If we consider no outlier in turbulence measurements and approve the data as they are, according to Fig. 2 in the wind bin 1 (free stream condition), the Frandsen model and the IEC design level turbulence underestimate the higher tail of the site turbulence in low mean wind speeds while overestimating it in high mean wind speeds (over the rated speed). In addition, Frandsen model estimations are higher than design in high mean wind speeds while being the same as IEC representative value in low mean wind speeds'

29) L. 330: Why do you investigate this type of multi-modal distributions and not others?

The text is modified to:' *We investigate the mixture of two or three Gamma distributions as well as a mixture of two or three Gaussian distributions as multimodal distributions have shown good candidacy for modelling of fatigue loads (see (Mozafari et al., 2023a)*'

30) L. 334-344 and Figure 4 and 5: Why do we need this? For Section 3.3, it is not needed.

This section is answering one of the side questions of the research showcasing the performance of the Frandsen model in different wake scenarios (different wind bins).

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.

In fact, data includes points with return period of 5 years (Kindly refer to reply of comment #9). However, the consideration that the tail is representative for only one season shall be included. This is added now to the discussion section.

32) Table 4: How did you determine the sensitivities?

As mentioned in the table description, they are '*importance rank of the random variables'* . Explanation and reference are added in the methodology now for more clarity (lines 312-313).

33) Table D1: How are the parameters of the different distributions defined? Typos etc.:

As mentioned in the description (L. 515): 'The *maximum likelihood method is used for fitting and the prediction error is measured by Akaike information criterion (AIC).'* However, for more clarification, the table title is also modified to be more descriptive.

34) L. 69: "assess" not "assesses"

Corrected.

35) L. 86 and others: "Section" and not "Sect." or "section". Same applies to "Eq.", "Table" etc. Or at least be consistent.

Corrected the 'section' and checked the whole text for consistency and for aligning with WES journal guidelines (In the beginning of sentences 'Section / Figure 'and in the middle of the sentences 'Sect. / Fig.')

36) L. 133: "in Table 1" not "in 1"

Corrected. Thank you.

37) L. 138: I think it is "D1" and not "D2". Overall, reference to figures in the appendix are not always correct.

The appendix numbering and referencing is now updated. Thank you for mentioning.

38) L. 174: "Rayleigh" not "Reighley"

Thanks for noticing. Corrected now.

39) L. 241: 365 × 24 ... not 365 ∗ 24 ...

Applied.

40) L. 346: "in Fig. 6" not "in 6"

Corrected.

41) Figure 6: Iref not Iref

Corrected.

42) Table 4 (and appendix): Do not use the notation 7.62e-3, but 7.62 × 10−3

Corrected now.

43) L. 392: "fatigue" not "Fatigue"

Corrected. Thanks for noticing.

44) L. 419: "h and more"?

Typo. Deleted now. Thank you.

45) L. 446: "In the following sections, we compare the turbulence levels in three scenarios of the study"?

Editing mistake. Omitted now. Thank you.

46) Caption of Table D1 has to be corrected.

Corrected.

47) Caption of Figure D2 has to be corrected

Corrected now according to comment #24 reply- additional information added to the caption as well.