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. Thus, some data with a return period of 5 years are included among measurements*.'

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.

Response to referee #2 (Manuscript number: wes-2024-68):

We would like to truly thank the reviewer for the detailed and very relevant comments that helped us improve the work to a high extent.

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

Lifetime extension of wind turbines is a very important topic of high industrial relevance. Using a probabilistic approach is also very relevant. Therefore, the paper is of interest to be published. However, there are a number of unclear sections and missing explanations, see below. A major revision is recommended.

Detailed Comments:

Line 79

' One must consider that the material properties are calibrated such that the target reliability level of 3.7 ISO-2394 (2015) is reached': this is an annual reliability index? And why 3.7? which components are considered?

Here, only the blade is considered. Assuming a moderate consequence of failure, a target reliability index of 3.7 can be assumed based on ISO 2394. However, looking at your later comment, referring to Section 2.4.4, we agree that we must stay aligned with IEC 61400-1 basis for calibration of safety factors for the results of all probabilistic assessments to be comparable. We would like to thank you for your important comment. The modifications are added now to the text. Considerations of the offshore version (25years of design lifetime for the type) of SWT-3.2MW, which was missing previously, is also added.

Line 80

'the levels are not': unclear – reformulate

Reworded to 'magnitudes' to make it clearer.

Line 161

Which 'exponent'? wind shear?

Yes- changed to 'shear exponent' in the text for clarification.

Table 2

Is full (Weibull) distribution n of turbulence used as specified in IEC 61400-1:2019? And if not add a comment on the potential influence.

1:

No, only the 90% quantile is used as in Ed.1. This is a good point. Thank you for your suggestion. Explanation added now as below:

'It shall be noted that group one is based on Edition 1 of the IEC standard and in case a full Lognormal or Weibull distribution are used (as in Editions 3 and 4, respectively), the results of the study differ (See (Mozafari et al., 2024) for differences). The following section includes the mathematical relations and procedures used in the study.'

Line 174

Rayleigh

Corrected. Thank you.

Line 174 + 178

Why use two different editions. Use ed 4 in order to obtain up-to-date comparisons?

The study shows the results based on Ed. 1 normal turbulence model (90% quantile as the representative value of turbulence in each wind speed bin). However, the difference that using Ed. 4 can make is shown in another research and the possible effect on the current study is discussed (see response to comment above regarding table 2).

Line 190

Explain equation

Explanations added now with a reference to IEC 61400-1 for further details.

Eq (4)

Missing m in eq?

Corrected now and fortunately it was only a mistake in the text not in the procedure. Thank you.

Eq (6)

k?

Explanation for 'k' is added now.

M to power m?

Clarified with addition cross sign and parentheses.

for composites the mean stress level is important. How is that accounted for?

1:

The mean stress level correction is not applied in calculations of DELs. However, since the aim of the work is to compare different approaches of obtaining DEL (all without correction), the effect on the results is low.

Line 213-215

Unclear – reformulate

Edited as:

'In Eq. (2), σ is the turbulence standard deviation (turbulence) of the free stream wind (ambient flow) considered as a random variable. In addition, μ_σ and σ_σ refer to the mean and standard deviation of the turbulence, respectively'

Eq (8)

How is lifetime damage obtained from 10-min damage?

By taking the weighted mean (by probabilities) of values of DEL_10min^m. Here the estimated value of (DEL_lifetime) was a mistake and has been omitted from the left side of the equation.

Line 218

'Probability of turbulence': which turbulence (ambient, effective, ...) is the probability linked to?

The word 'directional' is added for clarity.

Line 221

Conditional probabilities?

Yes! Reworded with 'conditional'. Thank you!

Line 235

Explain why 'log' is used

We refer to usage in equation 14 which is in form of log. A text is added.

Line 240

Describe what is 30-year return loads'. Is it 30-year extreme loads to account for the extreme loads being important due to the high Wohler exponent?

A reference is added to the relevant equations (and explanations) for return load in step 2.

Yes. The return load is used to extrapolate the tail of the distribution; description in the updated document:

1:

'The extrapolation is used to complete the tail of the DEL10min distribution to account for highest values that might change the weighted mean value (DEL_lifetime) if included. These values can have high effect due to the high fatigue exponent of the composite ((Mozafari et al., 2023b))'

Line 240

'Forming a database based on the distribution': unclear – explain which distribution. If the realizations follow the distribution function how is new information obtained?

Rewording is done now in step 2 with added reference to the corresponding explanations as below:

'2- Forming a database based on the distribution found in step 1 and extrapolating to 30-year return loads (Eq. 10 to 12)'

(previous reference in the next paragraph was a typo and now is corrected from 'Step 1' to 'Step 2')

Eq (10) +(11) +(12)

Explain the probabilistic assumption behind eq (10)

Explanation and reference added now as below as below:

*'Equation 10 is extracted from formula of probability of exceedance a threshold level (here the load which happens once every 30s) assuming a Poisson process for describing the peaks over threshold problem (for further information see (de Oliveira JT, 2013)). In the current case, the frequency of exceedance is 1/*T_LR*'. It has to be noted that Eq. 10 is correct when* T_LR *is relatively large (here, equal to the number of 10 minutes in 30 years).'*

Eq (1): Lr is not included in the right-hand side of the equation?

We assume you are referring to equation 10. A modification in Eq. 10 is made as follows:

T-> T_LR

In addition, explanations added as mentioned above describe how 'T_LR' is related to the LR.

Probabilities in eq (11) always between 0 and 1?

Yes, the 'log' sign is now omitted to make the formula correct (only text mistake and not the applications).

Explain reference times for the probabilities

Explanations added as mentioned above.

Are the loads obtained 'random point in time' loads or maximum loads with a certain reference period?

First, the maximum loads with a certain reference return period are defined, and the frequency of lower loads is derived accordingly.

1:

Above statement is added to the text for clarification (line 290 in the updated document).

Section 2.4.4

The Frandsen and IEC turbulence models together with partial safety factors are intended for deterministic design and not for probabilistic design and reliability analysis. This link should be included in the probabilistic formulations.

We very much appreciate your relevant and helpful comment.
Below paragraph is added to the end of section 2.4.4:
'It must be noted that the Frandsen and IEC turbulence models together with partial safety factors are intended for semi-deterministic design and not for probabilistic design and reliability analysis. However, since the partial safety factors are calibrated based on achieving certain reliability level (to which we are also setting the values for) at the end of the design lifetime, the results are comparable. Such comparisons are presented in the next section.'

Line 268

'Probability of failure at time t and can be stated as the probability of exceeding a certain level': this probability is the probability of failure at time t and not the accumulated probability of failure up to time t and also not the annual probability of failure?

Thank you for noting. To avoid misleading, the paragraph is reworded as below to stay as general as possible in terms of expressions and explanations:

'In Eq. 13, P_f (t) is the probability of failure at time t. Commonly, this problem is referred to with a function named limit state function (g (x, t)), and the safe region is where this function is positive. Thus, the probability of failure would be the'

Figure 2

The uncertainty of DEL is modelled by log(DELlifetime) ? add description of the uncertainty modelled by DELlifetime . How is this uncertainty quantified and does it include model uncertainty in estimating the stress ranges (obtained from a validation process)? This stochastic modelling assumes that strain gauge measurements are available for the fatigue detail considered?

We believe the reference to is figure '2' incorrect and thus, our answer is according to the general approach with assumption of reference to figure 6. The answer to all questions is 'yes'. An explanation is now added to the descriptions of figure 6 (lines 401– 404) as below:

'The uncertainty of log(DELlifetime) in the site is modelled by a frequentist approach (Maximum likelihood) based on observations in the measurements and includes all sources of uncertainty. However, in the case of the other two approaches, the uncertainty of this parameter is assessed based on bootstrapping and thus, it only includes epistemic uncertainty. The data in Fig. 6 are normalized by the converged mean of DEL_lifetime obtained above using site measurements.'

Eq (14)

Where does the time t enter in the limit state equation?

1:

Below statement is added before Eq. 14 for clarity:

'The time is omitted from Eq. 14 for simplicity with the assumption that all variables are referring to a certain time.'

Line 288

Explain how R=10 is used and why R=10 to account for mean stress level?

The SN curve is derived for that kind of loading; thus, the mean stress effect is already included. This explanation is added now to the same line for clarity:

'We consider R = 10 for fatigue properties (SN curve) of the composite Mikkelsen (2020). Although the variability of data is included as the CoV of such curve, a calibration is added at the end to set the mean value of material strength to a certain level at which target level of reliability is obtained at year 20.'

In addition, on the load side, we believe that in the flapwise direction, the ratio of mean to ultimate strength of the material is low compared to the relatively higher cycle range values making the effects of mean stress correction negligible in computation of DEL mean and standard deviation (as shown in [1]).

Table 3

How is the mean value calibrated?

As mentioned in the last paragraphs of introduction: '… the material properties are calibrated such that the target reliability level of 3.3 is reached after 25 years based on design class. ' A brief explanation is also added to the table 3 and the introduction as well as beginning of section 3.3 explaning that a factor is multiplied to the mean level of K.

Mean and standard deviation of log (DELlifetime) are missing in the table?

CoVs of all random variables are included now. However, the mean cannot be presented due to confidentiality.

Line 295

'Based on survival in the year before': not correct – reformulate

Rephrased to 'conditional on survival in the year before'

Figure 2

Add explanation of all symbols in the figure

[1] Veers, P. S., "Fatigue Loading of Wind Turbines," Wind energy systems: Optimising design and construction for safe and reliable operation, Woodhead Publishing Ltd., Cambridge, UK, 2011.

1:

Added now.

Explanation of the symbols are added now to the figure

Figure 3

Could a Weibull distribution (as used in IEC 61400-1:20+29) fitted to the upper tail be as representative as the distributions considered?

Weibull is used in IEC 61400-1 for ‘loads’. We do use the extreme value theory to model the tail of the ‘DEL’ data (Eq. 10 to 12). However, in general, we need to fit a full distribution as well since after all it is the weighted mean of DEL_10min that is of interest and not the tail. In the current case the Weibull distribution was not the best fit to DEL data.

Line 336

‘Extrapolate the distribution to a 30-year return load’: figure 3 shows random point in time observations of the turbulence level. Is this distribution used to estimate the load with a return period of 30 years? Or is the load with a return period of 30 years estimated using e.g. a peak-over-threshold technique considering the extreme, statistical independent loads observed during the measurement period (as in DLC 1.1 load extrapolation)? More explanation is needed.

Thank you for your comment. The latter is correct. More detailed explanations are now added in the methodology (section 2.4.3).

And how to use the load with a return period of 30 years for fatigue assessment?

More detailed explanations are now added in the methodology (section 2.4.3).

Line 357

The target annual reliability index in IEC 61400-1 Annex K is 3.3 (and not 3.7 as indicated in some DNV standards – assuming a ductile failure mode)

This is correct- We also keep the same level as the annex to get a fair comparison when using a reliability-based approach. However, the offshore version of the turbine being used here was neglected before and fortunately, the 25 years of lifetime is giving almost 3.3 in the current curves. Corrections are made now to aim for annual reliability index of 3.3 after 25 years.

Figure 7

As mentioned above the IEC and Frandsen models are intended for deterministic design with safety factors, not for reliability analyses. Recommendation: use the same approach for reliability analysis as in papers and reports related to fatigue of welded steel details in wind turbines.

Thank you again for the comment about the deterministic design versus reliability analyses.

Kindly see the below explanation:

1:

'It must be noted that the Frandsen and IEC turbulence models together with partial safety factors are intended for semi-deterministic design and not for probabilistic design and reliability analysis. However, since the partial safety factors are calibrated based on achieving certain reliability level (to which we are also setting the values for) at the end of the design lifetime, the results are comparable. Such comparisons are presented in the next section.'

Regarding the second comment:

Because this study focuses on blade-root moments, which are composite materials, it is not clear to us why we should use the same approach as those for fatigue of welded steel details.

Table 4

How is sensitivity defined?

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 322-324).

1: