

Reply to referee 1 (RC1)

We thank the reviewer for their comments and suggestions that have helped improve the manuscript, please find our replies below. Please note that the line numbers refer to the revised manuscript (with track changes). Also note that our replies are in black while the referee comments are in blue.

Dear authors,

thank you for your valuable submission. I enjoyed reading your manuscript, which I believe is worth being published in the journal Wind Energy Science. I only have a few minor comments that will hopefully improve the final draft:

- Abstract: The sentence “The difference in loads between unstable and stable conditions was found to be 98% for the tower and 20% for the blades” is too generic even for an abstract and is likely impossible to be understood without reading the rest of the manuscript. Also, the sentence “loads from the two sources” is unclear. What are the two sources? I'd suggest rethinking the abstract to make it more self-standing.

The parts of the abstract highlighted by the referee have been rewritten (lines 10 - 15). More information has been added with regards to the load comparison to make the abstract more self-standing. By loads from the two sources we mean simulation and measurements as now indicated in the revised abstract.

- Page 2, line 27: you write "On the contrary, atmospheric turbulence is often thermally generated, is inhomogeneous in the vertical direction and follows non-Gaussian statistics at the smallest scales". Although I agree with you, please include a couple of references supporting your claims.

Two references have now been included (line 29-30) to support the statement. One reference (Obhokov, 1962) relates to thermally generated turbulence and the other (Wyngaard, 2010) to non-Gaussian turbulence.

- Page 2 line 54: “due to inaccuracies in the aerodynamic modelling and the absence of certain controller features in the simulation environment”. The aerodynamic modeling has inherent uncertainties, but I don't think the authors pinned the discrepancies in the results to problems with aerodynamic modeling. The lack of the industrial controller certainly contributed to the offsets and the following TORQUE 2024 publication (Doubrawa et al., 2024), although limited to a single case, showed a notable improvement to the metrics when a rotor position controller was adopted. You already cite the work from Doubrawa et al., and you might want to add a note in the intro about it.

Brown et al. in fact speculate that inaccuracies in aerodynamic modelling could account for a certain portion of the errors in the one-to-one comparisons. Please

refer to the following line in section 6 pg. 17 of the article: “However, the simulated flapwise blade-root DELs and tower-base fore-aft DELs showed significant median biases of up to ~40% overprediction, which the authors speculate could be a result of inaccurate aerodynamic modelling in sheared conditions (note this shortcoming is being addressed currently by NREL in the ongoing development of OpenFAST) and omission of certain control features of the proprietary field controller, combined with possible errors in the simulated inflow wind fluctuations, and other unidentified errors.” As per the referee’s suggestion, we have added a note about the work of Doubrawa et al (line 58-60).

- Table 1: bottom two rows are identical, probably a typo perpendicular vs parallel?

Corrected the typographical error in Table 1 row number 8.

- The font size across the different figures of the manuscript is inconsistent. Please improve. (Font size in Figures 1, 2, and 5 is too large.)

The font size in the figure 1, 2 and 5 has now been reduced.

- Page 6, line 120: “Indeed, when computing the spectra for all 10 min periods within a given bin, we found the spread to be large, encompassing at least three decades.” What does this mean? Three decades? Or three standard deviations?

The sentence has been made clearer in line 132. By “three decades” we meant 10^3 or 3 decades on a logarithmic scale.

- Page 8 line 156: typo, as “expected”

Corrected typographical error in line 168.

- Page 9, figure 3: I suggest to move the legend to the top left plot, top right corner. It took me a second to find it, and currently it covers some of the data.

The legend has been relocated in Figure 9 to the top left plot.

- Page 12 line 213: I don’t think you are “reminding” the reader. I expected that results were going to be nondimensionalized, but I don’t think you had told me just yet. Maybe “highlighting” is a better choice?

The word “remind” has been changed to “inform” in line 226.

- Page 14 line 259: “It is difficult to replicate these in the simulation environment.” I’d suggest a small rewording. Modelling these effects is easy, the problem is the exact quantification of the imbalances.

The sentence has been reworded in line 272 such that it reads “As it is difficult to accurately quantify these imbalances, they cannot be replicated in a simulation.”.

- Page 14 line 262: “While we present an analysis of load measurements, Sathe et al. (2013) showed the effect of atmospheric stability on fatigue loads in a simulation environment. However, our results appear to be remarkably similar which indicates a fundamental relationship between atmospheric stability and fatigue loads on a pitch controlled horizontal axis wind turbine.”. Why however? Your results are similar to the ones from 2013? I’m confused by this sentence. Also, should it maybe appear after the results are discussed, and not before?

We used the word “however” because our methodology differs from that used by Sathe et al. (2013) who analyzed loads from aero-elastic simulations only. The results are indeed similar, as seen from Figures 9 and 10 of Sathe et al. compared to Figures 6 (a) and 7 (a) from our work. Since the sentence is after the discussion of the results related to the effects of atmospheric stability, we believe that the placement is appropriate.

Thank you again for the submission and congratulations for a very nice contribution to the

Replies to referee 2

We greatly appreciate the referee's in-depth review. We believe the points raised by them have helped improve the manuscript. Please note that the line numbers refer to the revised manuscript with track changes. The referee's comments are in blue while our replies are in black.

Reviewer's comments: wes-2025-278

Comparison of measured and simulated fatigue loads on a multi-megawatt wind turbine

January 26, 2026

General comments:

The manuscript entitled Comparison of measured and simulated fatigue loads on a multi-megawatt wind turbine by Patel et al. presents a valuable validation and measurement case to address the effect of atmospheric stability in turbulence models on fatigue loads on large, possibly one of the newest, onshore wind turbine. The paper presents full-scale measurement data which is used to derive empirical variation of parameters for several cases of non-neutral atmospheric stability. Considering the realistic validation case and the valuable comparison directly using full-scale measurement data to the in-house simulation results, the paper is suitable for the journal.

However, several points are still need to be addressed in the paper. I recommend major revision to address the points detailed in "specific comments" and "technical corrections" below.

Specific comments:

1. Line 24 - Could you point out which section in Veers et al. (2023) that support the statement? Do you mean the second last paragraph before Section 3.3 in Veers et al. (2023) [1]? If yes, please reference more accurately.
Yes, that is the paragraph we are referring to. A more precise reference to Veers et al. (2023) has been added in line 27.
2. Line 40 - I assume "which method" here refers to Mann, Kaimal models and wind fields from LES. It would be easier to mention them as "which inflow assumptions" or "which inflow models".
A correction has been made (line 44) where the phrase "which method" has been replaced by "which inflow model".
3. Line 57 - It is unclear which "two methods" meant here. Does this refer to the method discussed in Rinker (2022) [2]? Could you elaborate/revise for accuracy?
As the sentence talks about met-mast vs lidar-based inflow assimilation, the phrase "two methods" refers to these two inflow assimilation methods. The sentence has been made more elaborate in line 63.
4. Line 58 - I couldn't find the information that is related to the statement "...found that the uncertainties in the load estimates from the two methods were similar and occasionally lower when using the lidar" in Dimitrov et al. (2024) [3]. Perhaps, do you mean the other paper by the

same author, Dimitrov and Natarajan (2017) [4]? If yes, please correct the reference.

The referee is correct in pointing out the initial manuscript contained the wrong reference to a study by Dimitrov and Natarajan (2017). It has now been corrected in line 61.

5. The paper's contribution on using real-scale/full-scale measurements should be highlighted. It would be helpful to add one or two sentence in the introduction how important the validation using full-scale measurement data is and the importance of full-scale measurement (in real scale and natural atmosphere). It can be placed before the organization of the paper paragraph. As per the referee's suggestion, a sentence in the second last paragraph of the introduction in lines 71 to 74 with purpose of highlighting the paper's unique contribution.

6. Lines 65 and 66 - "..., the relationship between fatigue loads on a wind turbine operating in the eld and the atmospheric stability has previously not been documented in the literature.". Do you mean specifically for the large, newest o shore wind turbine (15MW)? The work of Holstlag (2016) [5] provides the analysis of atmospherical stability on fatigue calculation, though on 5 MW.

No, we mean a turbine operating in the real world, i.e., we show the relationship between fatigue loads and atmospheric stability using measurements of fatigue loads and measurements of turbulence. Please note that Holstlag et al. (2016) provides an analysis of the NREL 5 MW via simulations, thus the load analysis is not based on measurements.

7. Line 80 - "data signals" would perhaps be clearer instead of just "signals"

The phrase has been changed in line 87 following the referee's suggestion.

8. Lines 85 to 88: Please consider making this paragraph more concise. Redundancy has been detected, for example in the explanation of ' filtered dataset', when the authors may have meant 'curated'. The paragraph may need to be reorganised, as the next paragraph (Line 89) explains the f i r s t steps of data processing, which would be more appropriate to be placed at the beginning.

The paragraph has been made more concise (line 92 – 96) by removing information that is repeated elsewhere. As per the referee's suggestion we use the word "curation" instead of "filtering".

9. Line 89 - is 5 Hz enough? Could you provide explanation the effect of this chosen sampling frequency?

Yes, we believe 5 Hz is more than sufficient as this frequency lies well within the initial sub-range of the turbulence spectra. Indeed, as seen in Figure 3, not much energy is left in the spectra at 2.5 Hz. Thus, we can be confident that measurements capture enough of the spectra to obtain a good quality fit with the turbulence model. From the perspective of loads, turbulent fluctuations higher than 1 Hz are not deemed to be important as the turbine behaves as a low-pass filter. Moreover, the most important structural eigen frequencies are lower than 5 Hz and are hence captured by the resolution of the load measurements. A justification has been added in line 97 - 99.

10. Line 89 - "Only those periods were selected for further analysis, ...". The author focuses on the evaluation on a specific wind direction of 225- 315 , which is one of the "curation" steps that they performed to build the data sets. But I suggest to previously give more information on the data availability. For example, the measurement campaign that is used lasts between 2023 to 2025, then later it is stated that in the summer 2024 there are instruments error, so there are no measured data. Such information are currently scattered in the paper. A short paragraph or small diagram/table to explain data availability might help the reader from which period of months and year the chosen data are.

As per the referee's suggestion, a table on data availability has now been added. Table 2 now shows the data availability (in percentage) after each data curation step.

11. Line 91 - Is the averaging of 10-minute? Clearly stating the "10-minute" might help to remind the reader.

The averaging period has been added in line 99.

12. Line 104 - Please detail which anemometer and which height is used for the mean wind speed, also directly in the sentence.

The sentence in line 103 has been modified such that it now mentions the anemometer and height.

13. Line 107 to 109 - Which height do you compare to of the works Pena (2019) [6]? I assume based on the statement in these lines, it is compared to Figure 11(b) of Pe a (2019)? Then, could you explain how is the effect of the comparison to different height, since the measured data in this paper is from sonic anemometer 155 m, and in Pe a (2019), it is from 241 m.

We acknowledge that the distribution of stability at 155 m and 241 m is generally not the same. But for a given location we would expect it to have the same trends, i.e., if unstable conditions are common at 241 m, then they should also be relatively frequent at 155 m. So, our reasoning that the two datasets have a different seasonality, which causes the disagreement in the relative distribution of stability, still holds true.

14. Line 168 to 169 - A power-law fitting is used to characterize the mean wind speed profile (wind profile) along the height, and obtain exponent parameter. However, the power law is actually appropriate for neutral atmospheric condition. I expect fitting a power-law profile to only three wind speed measurement points (45 m, 105 m and 163 m) might be a simplification. I suggest to discuss the quality of fitting for the obtained results, such as what is the R^2 range for the fitting used here to obtained the resulting the Figure 5(a)?

We agree with the referee that using the power law profile to fit measurements from only three heights is a simplification. However, this method is often used in industry as it is recommended by the IEC standards [8]. Thus, it is important to highlight the drawbacks of this method, most notably by the fatigue load comparisons presented for the turbine blades in section 3.2. With regards to the quality of the vertical profile fits, the lowest value of R^2 from the 36 bins was 0.92. Thus, generally the wind speed profile up to hub-height fits the power law well. A note has been added in line 184.

15. Line 213 - Since the authors decided to anonymize the values, please explain more the impact to be expected from this anonymization.

Anonymization is just a rescaling operation which is the same for all the data in each plot. Hence, it preserves the qualitative difference between data in the figure. To comment on the quantitative aspects, we have attempted to do so via the text in section 3.1 and the z-scores in section 3.2. Anonymization is of course less than ideal as it does not show the absolute magnitude of the loads leading to some confusion as highlighted by the referee's comments no. 20 and 21. A short explanation has now been added to line 232.

16. Line 235 - It is stated that "As seen in Fig. 3 and Fig. 5, unstable conditions are generally associated with higher turbulence intensities and larger length scales ...". Which figure or data exactly that shows the effect of turbulence intensity? Do you mean indicated by the variance of spectra in Fig. 3? I expect, especially in lower wind speed, variation of turbulence intensities are present, especially along the height. I suggest to provide information from measurement on how is the turbulence intensities (in terms of ratio of velocity fluctuation to mean wind velocity, e.g. $I_u = u'/u$) along the height related to each of the stability classes or classified by the wind speed bins. Or provide a figure that shows turbulence intensity as its own value based on stability classes. Especially that later, the authors discussed a lot of "turbulence intensity" in paragraph of Line 240.

We admit that the earlier discussion on the impact of turbulence intensity was a bit vague. We have tried to address that by adding a figure that shows the measured turbulence intensity for each wind speed and stability bin. This can be now seen in Figure 7.

17. Figure 7 - which wind speed height are used for the x-axis? 155 m?

The caption in what is now Figure 8 has been updated to include the height at which the wind speed is measured.

18. Line 248 - Please add the value of rated wind speed (even if it's an estimation only), such as "At wind speeds below rated (... m/s), "

For reasons of confidentiality, we cannot mention the precise rated wind speed of the turbine. We have changed the sentence to give an approximate value in line 261.

19. Line 252 - Could you shortly mention the two drivers of fatigue loads in this line? Maybe shortly, inside a bracket (...)

The two drivers of fatigue loads, namely shear and turbulence, have now been added in brackets in line 266.

20. Line 255 to 256 - It is stated that "in case of the edgewise loads on the blade, as they are mainly driven by gravity, no dependence on stability or turbulence is observed." But in Figure 7(b) before rated wind speed (approximately), the unstable and very unstable show lower damage. How does this sentence then stay true?

The edgewise fatigue damage does appear to be lower in unstable conditions however the difference is very small. Although this is difficult to show in absolute terms due to the anonymization, please consider that the difference between the

maximum and minimum edgewise loads at 21 m/s and 5 m/s respectively, is four times lower than the minimum edgewise loads (since the y-axis starts from 4). Edgewise loads are typically in order of 10 MNm for a turbine blade of similar length (for reference, please see figure 5d in Syed et al. [7]). Thus, the difference between the max and min edgewise loads is in the order of 1MNm. From the figure, unstable conditions apparently show edgewise loads 0.3 units lower. Thus, this difference is in the magnitude of 0.1MNm which we believe is not very significant. However, to avoid confusion, we changed the sentence to "... no **significant** dependence on stability or turbulence is observed."

21. Figure 9(d) - In neutral condition, the measured damage values on blade root (flapwise) are higher than simulated for larger wind speed (larger than 16 m/s). Could you address this in the paper?

The referee is correct in pointing out that the mean measured loads appear to be higher than the simulated loads but when including the variance of the individual 10-minute measurements or seeds in case of the simulations, we see that the difference is less than 3 sigma. Thus, in our opinion, the difference is better explained by random errors rather than any systematic error in the modelling framework. An explanation has been added in the revised manuscript in line 313 - 315.

22. Line 300 to 305 - Interesting!

Technical corrections:

1. Equation (2), Line 103 - I assume, the equation of friction velocity is 1-D simplified, in the along-wind (u) wind direction. There should be, however, apostrophe ($'$) for each velocity component u and w , as it works with velocity fluctuations.
Equation (2) has been corrected.
2. Table 2, column 2 - Please add the unit of Monin-Obukhov length, such as "(m)" for easier reading
A unit has been added to what is now Table 3, column 2.
3. Line 154 - Do you mean "...with the t derived from Eq (7) ..."?
No, we do not, as the fit is derived using Eq (6). Equation (7) relates the 1D spectra to the spectral tensor. Indeed, the 1D spectra are precomputed into a look-up table, so when fitting the model to the measured 1D spectra, only Eq (6) must be evaluated.
4. Figure 3 - Please consider to put the legend on Figure 3(a) instead of on 3(f)
Fixed as also suggested by referee 1.
5. Line 227 - Regarding the word "straddles", maybe use simpler word more appropriate to the context, such as "which is around the turbine rated wind speed"

The sentence in line 240 has been modified to reflect the referee's suggestion.

References

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- [3] N. Dimitrov, M. Pedersen, and . Hannesdóttir. An open-source Python-based tool for Mann turbulence generation with constraints and non-Gaussian capabilities. *Journal of Physics: Conference Series*, 2767, 052058, 2024.
- [4] N. K. Dimitrov and A. Natarajan. Application of simulated lidar scanning patterns to constrained Gaussian turbulence fields for load validation. *Wind Energy*, 20(1):79–95, <https://doi.org/10.1088/1742-6596/2767/5/052058>, 2017.
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- [6] Peña, A. Sterild: A natural laboratory for atmospheric turbulence. *J. Renewable Sustainable Energy*, 11, 063302. <https://doi.org/10.1063/1.5121486>, 2019.
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- [8] IEC: IEC 61400-1 Ed4: Wind turbines - Part 1: Design requirements, Tech. rep., International Electrotechnical Commission, Geneva, Switzerland, 2019.