Dear Peer Reviewer,

Thank you for your valuable and insightful comments on our manuscript. Your feedback has been instrumental in improving the quality and clarity of the paper.

Attached, you shall find a detailed response to each of your comments, along with an updated version of the manuscript reflecting these improvements.

Thank you once again for your thorough review and constructive suggestions.

Best regards,

Tahir Malik

RC1: 'Comment on wes-2024-35', Anonymous Referee #1, 23 Apr 2024

1. Please take care of some typos and incorrect sentences:

- Corrected

2. In the introduction and conclusions, the authors state the novelty of verifying the effect of TI and erosion on the prediction of AEP and performance losses using an aero-servo-elastic simulation tool. This is not new; similar results and comparisons have been presented, for example, in the following literature:

- This comment is valid in that indeed turbulence as been investigated over time. The current paper, however, does not investigate aerofoil performance as a function of turbulence, but instead focuses on rotor performance. Our primary objective is to understand how mean power output is increased at low wind speeds and decreased around the shoulder of the power curve as a function of various turbulence intensities, in a systematic manner. This aspect of the study is new and we shall make this clear in the revised paper. While turbulence is indeed investigated in the referenced papers, some of them focus on turbulence at the aerofoil level, which is not the focus of our research. However, we acknowledge that Cappugi et al. (2021) partly investigate power curves as a function of turbulence intensity and we shall reference their work appropriately in our paper.
  Furthermore, an answer to this comment can be found in the introduction that has been rephrased.

3. I understand the issue of working with proprietary data and models; however, the authors should indicate more clearly how the presented study and results, which are not replicable as the data from the case study are not disclosed and not generalizable as the results apply to a particular erosion type and distribution and to a specific WT model, can be beneficial for industry or research in this field.

- We acknowledge the reviewer's concern about the proprietary nature of the data and models employed in our study. While ideally, we would have preferred to publish all details, there are significant knowledge bases in data from operational wind turbines that cannot be described in detail due to proprietary constraints. Nevertheless, this should not preclude us from analysing the data and deriving valuable insights.

  The trends and relationships identified in our study are highly relevant for the industry. Specifically, wind farm owners and operators, rather than OEMs, face daily challenges reflected in this study, such as lack of access to detailed proprietary information such as aerofoil shapes – a challenge also encountered by the present authors. Understanding these challenges is crucial for researchers to effectively support the industry. Our paper focuses on relative changes in performance rather than absolute values, which is crucial as wind turbine operators should respond to percentage performance reductions rather than reductions measured in megawatts.

  Although the specific results pertain to a particular turbine model and erosion type, the approaches and methodologies employed are generalizable. Consequently, the methods and conclusions presented in the paper should be applicable to various types of wind turbines, advancing the understanding of wind turbine performance under varying conditions.

We shall include a comment in section 2.1 to highlight how this study can contribute to the general understanding of power performance degradation. This addition shall clarify the broader implications and utility of our research for both industry and academic audiences.

4. At pg3: "Although the tested airfoil is not an identical match to that in the HAWC2 model, this approach is deemed a suitable approximation for representing the outboard region of eroded turbine blades." This sentence is not supported by data in the paper; it could be helpful to show that the power curve obtained with the original airfoil matches the power curve obtained with the substituted clean airfoil.

- We appreciate the reviewer's comment regarding the need for clarity on the aerofoil substitution. We did not replace the original aerofoil in the multibody model. Instead, we used wind tunnel data from a different eroded aerofoil to understand the impact on lift coefficient (CL) and drag coefficient (CD). These impacts were then applied as adjustment factors to the original aerofoil characteristics of the proprietary multibody model, specifically for the outer 15% of the blade. This approach allowed us to simulate the effects of leading-edge erosion on turbine performance without altering the fundamental aerofoil design of the model. Therefore, in its clean form, the substituted aerofoil would match the original aerofoil exactly, as would its power curve. Furthermore, we shall include a comment in section 2.2 to clarify this.

5. pg.3 "To represent the effects seen in the wind tunnel experiments, derived factors were used to approximate the results. For simplicity, the lift polar representing the clean airfoil was scaled by a factor of 0.9. Additionally, two artificial drag polars were created by scaling the drag polar representing the clean airfoil by factors of 1.5 and 2.0, respectively." This aspect of the methodology is not very clear and requires clearer explanation in the text. Furthermore, a proper justification should be reported in terms of the scientific soundness of the approach, by reference to existing literature proving the reliability of the approach, or by some verification test or evidence.

- We appreciate the reviewer's feedback regarding the clarity and scientific justification of our methodology. Unfortunately, due to proprietary constraints, we are unable to present the specific polars used in our simulations. To address this limitation, we aimed to represent roughness and erosion effects in a simplified manner.

  The goal is to simulate a realistic scenario where erosion has a measurable negative impact on the aerofoil's aerodynamic characteristics (lift and drag coefficients). While we acknowledge that not all aerofoils degrade in the same way, these factors are meant to represent a moderate and severe level of degradation, respectively.

  Our approach is based on findings from other wind tunnel tests, which showed that one instance of degradation resulted in a lift reduction to 0.9 times the clean lift coefficient (CL_clean) and drag increases to 1.5 times (CD_clean) and 2.0 times (CD_clean), respectively. This does not imply that all aerofoils exhibit this degradation pattern, but rather it represents one scenario of moderate degradation and one of severe degradation. These factors correspond to (CL/CD)_clean ratios of 0.9/1.5 = 0.6 and 0.9/2 = 0.45, respectively. This ensures a significant degradation effect in our data.

In other words, the reduction factors are chosen to ensure the simulated data reflects a meaningful deterioration in performance, even if the exact values might not perfectly match every real-world case.

The reduction in CL and the CL/CD ratio naturally leads to a reduction in power, which is why these factors are applied. We shall include a comment in section 2.2 of the paper to clarify this reasoning. Additionally, we shall reference appropriate literature to support the validity of this approach, demonstrating that it is a scientifically sound method for approximating the effects of erosion and roughness on aerofoil performance.

We believe this explanation shall enhance the clarity of our methodology and provide the necessary justification for our approach.

6. Figs 7, 8, 10, 12, 13 show many overlapping curves and are very difficult to read. I suggest reducing the number of entries to a subset of the most significant ones.

- ➢ We appreciate the reviewer's comment regarding the readability of Figs 7, 8, 10, 12 and 13. We acknowledge that we are challenging the reader. In response to your feedback, we shall tried our best to reduce the number of entries in these figures, so as to remove those that are most dispensable. Additionally, the figures are better represented and described. Where highlighting trends, we have enhanced the graphs using colour variations to achieve this. These adjustment shall help enhance clarity and readability, making the data more accessible and easier to interpret.

7. Please improve the clarity of captions for Tables 1 to 5. (From the captions of Table 1 and 2, it is not possible to understand the difference between the data presented in the two tables).

- ➢ This is a good comment that we understand when we read them again. They shall be changed to:
  - Table 1. Change in AEP as a function of TI. Row 2 shows AEP change relative to “clean” performance at TI=6%; Row 3 shows AEP change relative to “P400” performance at TI=6%; Row 4 shows AEP change relative to “P40” performance at TI=6%.
  - Table 2. Change in AEP as a function of TI and roughness level: AEP change relative to “clean” performance at TI=6%
  - Table 3. Change in AEP as a function of TI and roughness level: AEP change relative to “clean” performance at TI=6% with V_ave=6m/s
  - Table 4. Change in AEP as a function of TI and roughness level: AEP change relative to “clean” performance at TI=6% with V_ave=6m/s
  - Table 5. Change in AEP as a function of TI and roughness level: AEP change relative to “clean” performance at TI=6% with V_ave=6m/s

8. I understand it is part of the title and the research, however, the discussion about time averaging in the power curve is somewhat off-topic with respect to the other two. Indeed, TI and erosion are features related to the model of the case study, while the time averaging is a data processing parameter. Furthermore, I needed more than one reading to understand the point from section 3.4. I would suggest reducing this part, possibly moving it to an appendix, recalling the main outcome not in the results but in the presentation of the case study and methodology. This will improve the readability of the paper and also simplify the outcomes and conclusions.

- We appreciate the reviewer's comment regarding the discussion on time averaging in the power curve. We believe the averaging of data is very relevant and its inclusion is crucial for understanding the analysis.

  The use of 10-minute average values is a standard approach for investigating data from measurements and since our aim is to enhance the understanding of these measurements, we investigated this through simulations.

  Our investigation revealed that the impact of time averaging is almost as significant as the effects of roughness itself. Therefore, we believe it is pertinent to include this discussion. We do however acknowledge that the current presentation may challenge readability and that this aspect was not adequately explained.

  To address this, we have rephrased the introduction to clarify the rationale behind the analysis of time averaging. As well as rephrasing of section 3.4, with the inclusion of a new Figure 14. The conclusions section is also rephrased.

9. The conclusion section needs some revision. It sometimes presents repeated information ("This research contributes valuable insights into the multifaceted effects of turbulence intensity, blade roughness, and time averaging on wind turbine performance") or conclusions that seem not really related to the data presented ("it highlights how data analysis techniques can either mask or reveal the subtle effects of erosion and turbulence"). Furthermore, a clearer conclusion should be drafted on the discussion about extracting information on erosion-related performance damage from in-field measurements and SCADA data.

- On reflection and in response to this feedback, we have rephrased the conclusion to eliminate redundancies and ensure that it is clearer.

RC2: 'Comment on wes-2024-35', Anonymous Referee #2, 23 Apr 2024

- Line 1: typo (TI)can -> (TI) can.
➢ Fixed

- Line 5: Erosion and roughness are closely related and sometimes their meanings are mixed up. In the abstract, please specify clearly why erosion is represented in this study with different roughens values.
➢ The abstract has been updated as has the opening sentence of Section 2.2

- Line 20: energy losses of 7% (if it is AEP, please specify it).
➢ Fixed

- Line 30: strategiesBadihià strategies Badihi
➢ Removed sentence due to updated introduction

- Line 30: (2022)Gonzalez -> (2022) Gonzalez
➢ Fixed

- Line 30: The sentence " the lack of insight limits the potential benefits of quantifying APE loss" is difficult to understand, please rewrite it.
➢ Removed sentence due to updated introduction

- Line 35: " protection (LEP) and aerofoils more robust towards the effects of erosion" please give previous examples and references for LEP and robust airfoils.
➢ The following reference has been added to the paper at a different section: Bak C, Anderson P, Madsen HA, Gaunaa M, Fuglsang P, Bove S, Design and Verification of Airfoils Resistant to Surface Contamination and Turbulence Intensity, Conference: 26th AIAA Applied Aerodynamics Conference, August 2008, DOI: 10.2514/6.2008-7050

- Section 2.2. Please in this section it is necessary to specify the % of blade in which roughness is considered. It is 15% but can be found in subsequent sections.
➢ Wording improved to clarify

- Line 79: it is mentioned that the NACA airfoil used in Krog Kruse experiments it is not the same as the one in the HAWC2 model. Please specify if in the computations presented in the paper the airfoil used in the outer part of the blade is the NACA one or the one in the confidential wind turbine model.
➢ Very good point. It has now been reworded to clarify that it was the original proprietary aerofoil to which the factors have been applied, rather than replacement with an alternative aerofoil.

- Line 87: please improve the explanation of the derived factors. If they are used in the airfoil coming from the HAWC2 model it is not very clear specified.
➢ Section 2.2 improved to clarify this point

- Line 94: please specify what is the 'plate behaviour'
➢ Text updated with explanation of meaning

- In Section 2.2 it is not clear whether the control system will be activated during all the simulations performed in this work. That is, describe if the control system detects that the power production is not achieved due to the affected airfoils will make any actions and mitigations.
  - ➢ Section rewritten to clarify

- Line 169: 'with imposed wind shear conditions' : please explain which are these conditions.
  - ➢ Updated text.

- Figure 8: it is difficult to compare since the lines are in top of each other in 2 groups. Maybe a table or separating in different figures for each TI could help. The question that should be answered in this figure is: for all the TI studied the % of power loss is similar or depends on the TI value?
  - ➢ Improved this graph as well as numerous others. Where highlighting a trend we have enhanced the graphs using colour variations to achieve this.

- Line 213: the sentence ' averaging effect of time averaging' is a bit confusing for the reader. Please rewrite it.
  - ➢ Corrected

- Figure 9 caption: change -> Change
  - ➢ Corrected

- Figure 9: all the cases for P40 are computed for different TI, but the reference is always clean and TI 6%: is this consistent?
  - ➢ Doublechecked for consistency

- Figure 12: In the legend 'Clean 6% TI' appears twice, please specify the difference between them in the text and in the legend.
  - ➢ Corrected

- Line 272: It is suggested that the unexpected behavior detected for certain turbulence conditions that is presented in Table 1 is removed for the study. Once it is clarified it could be presented in future works.
  - ➢ Corrected. Turbulence of 20 and 25% are not representative of offshore conditions and have been removed from all data tables.

- Table 1 caption: ' the same profile' -> it is not clear which profile
  - ➢ Caption improved

- Table 1 and Table 2 captions: please include the velocity in this case to be consistent with Tables 3-4-5
  - ➢ Updated

- Line 398: these factors and these aspects appear several times in the paragraph.
  - ➢ Corrected

- Conclusions section: In line 409 it is said that the impact of blade erosion was less significant and right after, 'Blade roughness can significantly affect power-production' Even thought it is ok, do not write them so close because is a bit confusing.
  - ➢ Good point. Section re-worded.

- Please review the whole conclusions section, it is very schematic.
- Entire conclusion reworked

- Competing interest sentence: the word ‘by’ appears twice and is a typo.
- Corrected

RC3: 'Comment on wes-2024-35', Anonymous Referee #3, 04 May 2024

First and most importantly the scientific archival value of the study is not apparent, and perhaps not strong enough for journal publication. Despite the large quantity of data presented in the study the main take away that I was able to see from the study is the fact that differences in power curve and AEP caused by erosion can be similar or even smaller in magnitude than the differences caused by turbulence intensity. This can make it hard to diagnose blade erosion in the real world, as the decrease in power output may be masked by variations in turbulence intensity. This message is interesting, but the large amount of data that is presented in the paper is redundant for this relatively straightforward message. In addition, authors do not suggest ways to work around this issue but the discussion in the paper is limited to presenting the data and little else. Finally, as the authors suggested, the large effect of turbulence on wind turbine power curve can already be found in the existing scientific literature, making the specific contribution of this work somewhat more unclear, and diminishing the scientific value.

- We appreciate the reviewer's detailed feedback. The primary aim of our research was to develop methods for detecting wind turbine performance degradation, particularly due to blade erosion, in operational turbines using real-world SCADA data. This is a critical issue in the industry, where significant time and resources are spent annually to diagnose such degradation, which has proven challenging due to the lack of an established correlation between blade erosion and full-scale turbine performance degradation.

  Furthermore, the extent to which turbulence intensity and time-averaging practices influence the analysed performance, potentially masking the effects of blade erosion, is not well understood. We acknowledge that this message may not have been effectively communicated in the original manuscript.

  We respectfully disagree with the assertion that our study lacks scientific value. This topic necessitates further insights so as to better analyse full scale turbine measurement data. Our research contributes to support the enablement of detection of erosion, a challenge recognized by both industry and academia.

  To address the reviewer's concerns, we have rephrased both the introduction and conclusion to more clearly articulate the purpose and significance of our study. We emphasize that understanding these influences is essential for accurately diagnosing aerodynamic degradation in wind turbines, a complex task due to the interplay of various factors.

  Furthermore, while we acknowledge that the effect of turbulence on wind turbine power curves is well-documented, our study goes beyond this established knowledge. Rather than focusing on aerofoil performance as a function of turbulence, we investigate rotor performance. While turbulence is indeed investigated in existing literature, some of them focus on turbulence at the aerofoil level, which is not the focus of our research. Specifically, our focus is in the context of how turbulence intensity variations can obscure the detection of blade erosion in operational settings.

  In response to the reviewer's feedback, we have especially rephrased the introduction and the conclusion section. We believe these revisions and clarifications throughout shall better highlight the scientific value and practical implications of our research for the wind energy industry.

To add to this, the data appears to be presented without a clear goal in mind. This makes the manuscript, despite it being divided in many subsections, very difficult to read. It is unclear what is the “glue” between the sections and how they contribute to the final take aways. In addition, many graphs are hard to understand (such as Figures 7, 13). Some other Figures are hard to tell apart – for instance it took me quite a while to understand the difference between figures 16-1 and 18-19.

- This adds to the comment above and we recognize the need to be clearer in our message. To address this, we have rephrased sections including the introduction and conclusion.

Figure 11 and especially 10 are misleading, and if I have understood how they are computed, incorrect in my opinion. In fact, the Cp seems to be computed by dividing mean power by mean wind speed. This is incorrect, as Cp is an instantaneous value and should be computed based on instantaneous power and wind speed, and then averaged. Please explain how these values are computed. Authors attempt to warn readers about the high values of Cp in figure 10 at lines 237-241 but the explanation could be improved. The main reason for the large Cp values is the fact that wind turbine power near cut-in as a function of wind speed is cubic, thus increases in wind speed increase power more than decreases in wind speed do.

- We appreciate the reviewer's comments and concerns. We are fully aware of the reasoning behind the high Cp values and we shall make this issue clearer. We acknowledge the potential for misunderstanding and shall take steps to clarify this issue. The primary message we intend to convey here is that power curves, could be understood as steady-state performance, but the unsteadiness is changing this and that is why the figures are presented to make it clear that power efficiency apparently can be very good but that the reason - in this case - is the turbulence level and that one can make false conclusions.

  To address this, we shall rephrase section 3.2.3 to make this clear for the reader.