

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

Report #1: 'Comment on wes-2024-35', Submitted on 04 Jul 2024 - Anonymous referee #1

I thank the authors for their effort in addressing the required changes, and for their explanation about those suggested changes that they decided to do not apply. The rebuttal is satisfactory, but some of the new text included in the revised manuscript needs attention.

Introduction: The text has been significantly altered and revised. However, the new text sometimes shows poor English, unclear information, and confusing sentences. It still does not clearly explain the novelty and impact of the presented work and results compared to the state of the art.

- Thank you for your comments, you are right!
- The novelty of this work has been further emphasised especially in the introduction of the paper.
- Language sharpened and clarified to reflect the same throughout.

Lines 1-5: The text is complicated. Please simplify the sentence and clarify its meaning.

- Abstract improved

Lines 40-43: What do the authors mean by measuring AEP losses in a wind tunnel? Please revise this part.

- Text corrected

Lines 50-52: "Therefore, this study aims to address the following question: 'What makes power losses due to erosion so challenging to detect in operational wind turbines and how can these challenges be more effectively addressed?' The current work addresses this by incorporating a certified model of an operational turbine's controller in the full aero-servo-elastic simulation loop." This text is written with very poor English.

- You are correct, in response the language has been clarified

Line 55: "The obscurity of erosion's effects" – I am not sure this is the right term to use.

- Sentence revised

Lines 77-82: Please revise the text and write it in better English (e.g., "it is considered key that a model of a real wind turbine is investigated" is not properly formed).

- Sentences revised

Figure 1, 2, 5: Revise the captions (e.g., I do not think P40 can be called a "blade condition").

- Caption text replaced with "blade roughnesses"

Lines 134-135: My understanding is that the data for the clean airfoil of the real WT is available but not disclosable, so the Cl and Cd curves for the original airfoil are available and used to model the nominal WT in HAWK2. The data for the airfoil with roughness are obtained by applying the delta Cl and delta Cd measured on a NACA63 to the polar of the original airfoil. If

this method was applied, I have no further questions on the method, but I would like the authors to revise the text as it is not clearly explained.

- This is indeed the applied method where multiplication factors are applied to the original aerofoil.
- The has been improved in response to your comment. The original text could indeed have misinterpreted. Thank you for pointing that out.

Line 195: What are the "factors"?

- Correct to "shear"

Line 457: Please be more specific about what the "values" are.

- Specific values included in text. Thank you.

RC2: 'Comment on wes-2024-35', Submitted on 24 Jul 2024 - Anonymous referee #3

I find the article has improved with respect to the first version. Particularly the motivation is much clearer in the introduction. To better clarify my previous comment, I was challenging the archival value of this study not because the topic is not a significant issue in the industry, rather because the paper did not provide clear suggestions or best practices beyond presenting the issue. This has now been somewhat addressed in the conclusions, which present some suggestions and future actions on the topic. I still have some comments:

1) Please revise English carefully throughout. For instance, the first sentence of the revised abstract could be improved “[...] and obscures the underlying reasons.” – the subject is missing.

➤ Thank you are very right! Abstract improved and other corrections made.

2) Authors are certainly well aware, but perhaps it would not hurt to stress that the influence of turbulence and averaging intervals has been studied previously in the literature even at a rotor level and not only at an airfoil level (for example see <https://doi.org/10.1016/j.renene.2020.04.123> (Saint-Drenan et al. (2020)) for a summary of some sources). In addition, varying turbulence levels are often cited as a significant source of uncertainty in power curve estimation (IEC 61400-12 and similar). This study does however expand on the topic introducing averaging on different time intervals.

➤ This is indeed valuable feedback that is sincerely appreciated. And the work in the field is acknowledged. The following are referenced as important contributions in our paper, particularly emphasizing the role of turbulence on turbine performance.

- *“This challenge stems from the complex interplay of factors affecting the turbine’s performance (Barthelmie and Jensen (2010)), making it difficult to isolate the effects of ...”*
- *As have the following, in the introduction: “Furthermore, turbulence is a well-known atmospheric condition that significantly impacts wind turbine performance (St. Martin et al. (2016); Saint-Drenan et al. (2020); Kim et al. (2021); Cappugi et al. (2021))*
- *This alignment with preceding studies \cite{wagner2010simulation} and \cite{saint2020parametric} further validates the critical nature of TI in such analyses.*

➤ Additionally, the novelty of this work has been further emphasised especially in the introduction of the paper and in the language.

➤ In addition to time averaging the study importantly expands **erosions** relative impact, an aspect that has not been extensively addressed in previous work.

3) I understand why the Cp graphs are presented the way they are, but I still find it a bit misleading. I think this discussion could be avoided completely if the equation through which the Cp is computed is reported, clearly showing that averaging is performed on wind speed and power separately, before Cp is computed based on average values.

➤ You are correct. The text has been updated for clarity on method of computing Cp, including the used equation and explanation.

4) Section 3.4 is very hard to read, I would strongly suggest revising it completely. It contrasts quite a lot with the introduction, which flows much better and is much easier to understand. The influence of averaging interval on performance decrease comes across quite effectively. It is unclear to me why the 1 second averaging interval seems to greatly reduce the variability in mean power output caused by TI for the clean blade but not in the eroded blades. Elaborating more clearly on this is crucial for the conclusions (particularly in their revised form), as, if I'm not mistaken, authors suggest to use one-second averaging interval

- Your comment is fair. In response:
- The sections are better, structured, organised and titled and the baseline for each Figures case has been more clearly articulated.
- **Importantly**, I had overlooked the 0.01 second time interval for Figures 15, 16, 17, 18, 19 and 20. These have now been added to the figures – **apologies** for the oversight.
- These figures have been improved using colormaps to show the trend and differentiate between sets of figures and dimensions. Roughly (not strictly) speaking e.g. Copper for turbulence, Winter (blue to green) for time interval, Abyss (blue variation) for roughness
- **The text has been majorly revised for clarity.**

It is hope that correction of the omissions and improvements in text add to the clarity that caused misunderstanding and correctly support the conclusions. Thank you again for catching this!