

## Response to RC1

We sincerely thank Reviewer 1 for the detailed, constructive, and technically insightful comments. These comments have been very helpful in improving both the scientific clarity and the completeness of the manuscript.

In the revised manuscript, we have carefully addressed all comments and substantially improved the paper. The main revisions include:

- (1) expanding the Introduction and literature review to better position the novelty of this work.
- (2) revising the citation style and ensuring that all references are properly cited in the main text.
- (3) adding detailed descriptions of the field-test setup, instrumentation, measurement locations, and non-acoustic parameters.
- (4) clarifying how the serrated trailing-edge configuration is represented in the BEM-based aerodynamic analysis.
- (5) improving the clarity of figures, symbols, equations, and operating-condition definitions.
- (6) adding further explanation of the acoustic spectra, load cases, and limitations of the comparison between field measurements and simulations.

Our point-by-point responses are provided below.

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### Comment 1:

**- Introduction: please call your references in an appropriate way (name and year or number, the journal usually has an expectation).**

**+ the literature review in the introduction is extremely short. You must give a larger overview in order to show what is new in your work.**

### Response

We agree that the original manuscript was too concise and did not provide sufficient detail in several sections. In the revised manuscript, we have substantially expanded the Introduction to provide a broader literature review on trailing-edge serrations for wind turbine noise reduction and aerodynamic performance, with a clearer statement of the novelty of the present study (**Section 1, pp. 1–3, especially lines 28–80**). We have also checked all references and corrected the citation style throughout the manuscript, ensuring that all retained references are cited appropriately in the main text.

In addition, we have significantly expanded the description of the experimental setup and the simulation methodology, including the serration configuration, spanwise installation range, measurement arrangement, instrumentation, data acquisition system, and the structure of the

serrated blade (Section 2.2 (lines 103–130) and Section 3.1–3.3 (lines 133–185)). Our aim in the revision is to make the methodology sufficiently transparent and reproducible for readers.

- Fig. 1: -  $u$ ,  $U$  and  $V_0$ , please be consistent (later in the text as well).

- I guess it is  $\omega r(1+b)$  in (b) (according to the arrow that we barely see)

- your choice of  $L/D$  leads to a case where the turbine would need to be motored to rotate. It is fine for the visualization of the velocity triangle, but not that nice in terms of illustrating the operation of a wind turbine (we would expect FT to be in the opposite direction).

- Eq. 5: I guess it should be "b" instead of "a" for  $V_{rot}$  (according to Fig. 1)

### Response

We agree that the original version of Figure 1 and the associated text were not fully consistent in notation, and that the force-direction representation in Figure 1(b) could be improved.

In response, we have revised Figure 1 and Section 2.2 lines 103–125. Specifically, the notation has been unified throughout the manuscript:  $U_\infty$  is now used for the free-stream velocity,  $U_\infty(1-a)$  for the axial velocity component,  $\omega r(1+a')$  for the tangential velocity component, and  $W$  for the relative velocity. The inconsistent notations  $u$ ,  $U$ ,  $V_0$ , and  $b$  have been removed.

The corrected expression for the relative velocity is now given in Eq. (5), and the definitions of the induction factors are clarified in Eqs. (6)–(7) and the surrounding text on p. 5, lines 115–125.

- line 83: "axial velocity factor" => do you mean "axial induction factor"?

### Response

Yes, "axial velocity factor" was not the appropriate term here. It has been corrected to "axial induction factor" in Section 2.2, p. 5, lines 122–125 of the revised manuscript.

- It is really not clear how the serration will be taken into account in your BEM simulations. Explain this. You give some detail at line 105 but we still do not know how the  $C_L$  and  $C_D$  are affected. Please explain it.

- line 100: "The findings are intended to provide a valuable reference for future research in the field of wind turbine noise control" => wait at least the conclusion before writing such statement.

### Response

Thank you for this important comment. In the revised manuscript, we now clarify explicitly that the aerodynamic effect of the trailing-edge serrations was represented in the BEM-based blade model by adjusted sectional lift and drag coefficients ( $C_L$  and  $C_D$ ). This clarification is now given in Section 3.1, lines 146–150. We also state there that the modified aerodynamic

data for the serrated blade sections, located **0 m, 10 m, and 20 m inboard from the blade tip**, were obtained from wind tunnel measurements provided by the blade manufacturer and implemented in the DNV Bladed model.

- **line 102: what do you mean by "which has a good power characteristic for capturing the optimal  $C_p$  value"? It is not clear to me (it would need to be rephrased).**

#### **Response**

In the revised manuscript, this sentence has been rephrased to clarify that the variable-speed, pitch-regulated turbine can operate close to the optimal power coefficient  $C_p$  under below-rated conditions. This clarification is now given in **Section 3.1, lines 141–145**.

- **line 103: "the fine pitch angle is uniformly set to 0deg" => what do you mean by "fine" here?**

#### **Response**

Thank you for pointing this out. The term “fine pitch angle” has been replaced by “**optimal pitch angle**” in **Section 3.1, lines 143–145**.

- **Fig. 2 and Tab. 1: are "h" and "H" the same parameters?**

#### **Response**

Thank you for this comment. This typographical inconsistency has been corrected in the revised manuscript. The serration geometry is now shown consistently in **Fig. 2** and **Table 1** in **Section 3.1, lines 151–157**, where the parameter is given as *h*.

- **Fig. 2: for more clarity, it would be good to add a figure where you illustrate on a schematic the extent of the serration structure compared to the blade length.**

#### **Response**

Thank you for this helpful suggestion. In the revised manuscript, we have added a new schematic figure (**Figure 3**) to illustrate the spanwise extent of the serration arrangement relative to the full **76 m** blade length. The corresponding explanation is now given in **Section 3.1, lines 155–157**, where we state that the serration structures are installed along the trailing edge near the blade tip and are distributed sequentially from the blade tip toward the blade root, labelled as **S1 to S4**. Their detailed dimensions are provided in **Table 1**.

- **section 3.2: give more technical details about the "test software, microphones, tuners and other writing and data acquisition modules".**

- **section 3.2: give the distance from the turbine to the measurement location in turbine diameters as well.**

#### **Response**

Thank you for the valuable suggestion. Additional technical details of the acoustic measurement system have been added in **Section 3.2, Table 2, lines 160–170**. These include

the measurement software, microphone, acoustic calibrator, DC junction box, connection box, data acquisition module, power supply module, and anemometer. We have also added the turbine-to-measurement distance, which is now stated as **218 m, corresponding to approximately 1.4 D**, in **Section 3.2, lines 167–170**.

**- section 3.3: the picture (left) is not fully explicit. Add arrows to identify what component is what (including the "reflective hard board")**

#### **Response**

Thank you for this helpful suggestion. In the revised manuscript, **Figure 4** has been updated with labels identifying the main components of the field-test equipment, and **Figure 5** has been added to show the microphone mounted on the reflective board and the view from the microphone position toward the wind turbine. These revisions are now shown in **Section 3.3, Figures 4–5, lines 175–185**.

**- section 3.3: what are the "non acoustic parameters" (line 127)?**

#### **Response**

Thank you for this helpful comment. In the revised manuscript, the sources of the different datasets have been clarified in **Section 3.3, lines 178–183**. Specifically, acoustic data were acquired by the noise measurement system, meteorological data were obtained from the on-site anemometer, and turbine operating data, including power output, blade pitch angle, and rotor speed, were obtained from the turbine SCADA system.

**- table 2 and 3: Make clear what "serration option" (Tab. 1) is presented in these tables.**

+ why is the maximum wind speed not the same in both cases? => you cannot make conclusions over the range 5.5 to 11 m.s-1, it should be limited to the lower maximum wind speed (9.5m.s-1).

#### **Response**

Thank you for this valuable comment. In the revised manuscript, the field-test tables are now clearly separated as **Table 3** for the turbine **with** the serration structure and **Table 4** for the turbine **without** the serration structure. This is stated explicitly in **Section 4.1, lines 190–195**. In addition, we now clarify that, because the measurements were conducted under natural field conditions, the valid wind-speed ranges were not identical, and the direct comparison has therefore been limited to the common range of **6.0–9.5 m s<sup>-1</sup>**. This limitation is stated in **Section 4.1, lines 190–195** and reflected in the subsequent discussion and **Figure 6**.

**- line 148: "a detailed comparison was made of the noise spectrum" => you must show the spectrum if you write about it.**

+ some analysis based on the aerodynamics would be interesting: does 400Hz correspond to something in particular?

#### **Response**

Thank you for this valuable comment. Following your suggestion, a one-third-octave spectrum comparison has been added as **Figure 7**. The selected operating point is now stated explicitly as **8.0 m s<sup>-1</sup>** in **Section 4.1, lines 210–217**, and the corresponding discussion of the spectral reduction range and the maximum reduction at **400 Hz** is given in **lines 212–217**. As revised, the interpretation is kept cautious and does not attribute the 400 Hz feature to a specific tonal or structural source without further supporting evidence.

**- section 4.2: how are the serration structures taken into account in the BEM simulations? Are the CL, CD modified? Based on what information?**

#### **Response**

Thank you for this important comment. This point is now clarified in **Section 4.2, lines 220–225** of the revised manuscript. We state there that the original and serrated blade configurations were represented by two integrated manufacturer-supplied Bladed model packages, and that, according to the manufacturer, the aerodynamic characteristics of the serrated blade were derived from wind tunnel measurements and incorporated into the corresponding blade model. We also clarify that the detailed internal implementation of the aerodynamic corrections was not directly accessible to the authors.

**- Fig. 5 and 6: I do not understand the differences between the actual turbine and the simulations. Are the CP = f(TSR) curves not the same in both cases?**

#### **Response**

Thank you for this comment. This issue is now clarified in **Section 4.2, lines 226–230**, where we explain that the field measurements and the simulations are not based on the same wind-speed definition. We now explicitly state that the comparison between measured and simulated curves is intended to illustrate general trends and relative differences between the two blade configurations, rather than to provide a strict point-by-point validation. The corresponding measured/simulated curves are shown in **Figures 8 and 9**.

**- lines 165-170: I am not sure that I understand correctly: in the field tests, the wind speed is the wind speed in the near wake of the turbine? Make clear where the anemometer is located (with a schematic and the distances). If this is the case, as you mention, the measured wind speed is lower than the far upstream wind speed (the one used in the simulation). So how do you compare the curves?**

#### **Response**

Thank you for this important comment. We agree that, after re-checking the available test records, the explanation given in the original manuscript was not sufficiently accurate. In the revised manuscript, we have avoided overinterpreting the field wind-speed measurement and clarified the test arrangement using the available information. The turbine dimensions and mast height are now stated in **Section 3.3, lines 172–177**, and field photographs of the setup are provided in **Figure 4**. We also clarify in **Section 4.2, lines 226–230** that the field and simulation wind speeds are not defined in exactly the same way and that the comparison

should therefore be understood as a qualitative comparison of trends and relative differences, rather than a strict point-by-point validation.

**- line 170: I do not understand how you determine the number of hours of operation of the wind turbine in both cases. Make it clear.**

#### **Response**

Thank you for this helpful comment. A separate subsection, **Section 4.3 “Annual equivalent power generation hours”**, has been added in the revised manuscript. The procedure is now explained in **lines 245–270**, including the adopted Rayleigh distribution, the calculation of the bin probabilities, the continuous and discrete AEP formulations, and the derivation of the annual equivalent power generation hours. The numerical results for the two blade configurations are reported in **Table 5**.

**- Fig. 7: make clear the meaning of the terms of the x-axis: R-Hub, S-hub, etc... (an additional schematic would be welcome: you must show the coordinate frame that you use) + make clear what is the operating point considered (before calling Fig. 7).**

#### **Response**

Thank you for this helpful comment. In the revised manuscript, a new introductory subsection, **Section 5.1**, has been added to define the load-coordinate systems and the statistical treatment of the load results. The meanings of the labels **Blade-Root, R-Hub, S-Hub, T-Top, and T-Base** are now given in **Section 5.1, lines 275–295**, and the corresponding coordinate frames are illustrated in **Figure 10**. In addition, the statistical basis for the ultimate and fatigue load comparisons is clarified in the same subsection.

**- line 183: "The load case corresponded to the pitch actuator fault condition defined in IEC 61400-1" => can you clarify what is this load case in the paper? It would be good to give the information to the reader straightaway, without the need for him to read the IEC 61400-1.**

#### **Response**

Thank you for this helpful comment. In the revised manuscript, this has been clarified directly in **Section 5.2, lines 305–310**. We now state that the governing ultimate load corresponds to **load case DLC 2.2b(c)+4**, i.e. a fault-related design load case during power production, and that the governing load channel is **R-H- $M_{yz}$** .

**- line 185 and around: why would the serrated blade delay the pitch motion? It is not clear.**

#### **Response**

Thank you for this comment. In the revised manuscript, the discussion has been reformulated more cautiously in **Section 5.2, lines 307–320**. We now describe the delayed pitch response of Blade-2 as an **observed response difference under the selected simulated fault condition**, and we explicitly state in **lines 316–320** that the underlying mechanism

responsible for this delay was not resolved in the present study and that no direct causal attribution to the serrated trailing-edge treatment is made. The associated time histories are shown in **Figures 12 and 13**.

**- line 200: "..., the peak value of the serrated blade is relatively large near the first order frequency of the tower." => you did not provide the natural frequencies prior to this statement (nor after), you must provide it if do this kind of analysis.**

#### **Response**

Thank you for this helpful comment. We agree that the original wording was too strong, because it implied a direct association between the observed spectral feature and the first tower natural frequency without presenting the corresponding modal information in the manuscript. In the revised manuscript, the direct modal attribution has therefore been removed. The fatigue-load discussion is now given in **Section 5.3, lines 330–340**, where the spectral result is described only as an observed difference in the simulated rotating-hub response under a representative operating condition. We also state explicitly in **lines 338–340** that no direct modal attribution is made because the structural natural frequencies are not presented in this study. The corresponding spectral comparison is shown in **Figure 14**.

**- you mention a 3% to 4% difference between the serrated and non-serrated blades in terms of  $M_x$ ,  $M_y$  and  $M_z$ . How does that compare with the accuracy of your model?**

#### **Response**

Thank you for this comment. In the revised manuscript, this point is now treated more cautiously in **Section 5.3, lines 326–330**, where we explain that, since both blade configurations were analysed using the same simulation framework and post-processing procedure, the normalized DEL values are used primarily to indicate the **relative comparative trend** between the two configurations, rather than to support a strong absolute conclusion independent of modelling uncertainty. The corresponding normalized fatigue-load results are reported in **Table 6**.