Dear Reviewer,

We would like to sincerely thank you for taking the time for this in-depth review of our article. We really appreciated it. By addressing your comments, we think we have improved the technical quality of the paper.

You can find our answers below each of your comments. In *italic blue*, you can also see how we addressed the comments within the manuscript. The references we have included in our answers can be found in the revised manuscript.

Regards,

The Authors

Anonymous Referee #1

General comments:

The authors investigate the ice accretion on a wind turbine. The article seems original because it allows to quantify the effect of the ice surface roughness on the performance losses of the wind turbine. For this, the authors use numerical simulations. Being well aware of the strong uncertainties on the roughness input data, they performed an interesting parameterization. Moreover, they have performed quite fine simulations of the ice accretion on each studied section by a time-efficient multi-step approach.

Specific comments:

1. Introduction

Reviewer Comment

[Line 101] "The icing event was long enough for ice horns to form, to combine the effects". The author are supposed to address rime-ice conditions from an earlier comment. The term "horn" is more often used for glaze-ice shapes.

Authors Response

Thank you for noting.

We have replaced "ice horns" with "streamlined, protruded ice shapes" (ll. 121-122).

2. Methodology

Reviewer Comment

[Line 129] What is the "wind shear exponent"?

Authors Response

The wind shear exponent models the vertical velocity profile $V(z)$ of the wind. Specifically, it is the parameter α in the definition of the Normal Wind Profile model of the DNV-GL Guideline for the certification of wind turbines:

$$V(z) = V_{\text{hub}}(z/z_{\text{hub}})^{\alpha}$$

We have defined the quantity in the text (ll. 154-157).

Reviewer Comment

[Line 131] What does the OpenFAST simulation imply for the ice accretion simulation? For instance, do the wind turbine operation data account for the retroaction of the ice shape growth? (rotational velocity, etc.)

Authors Response

Within this work, OpenFAST computed the correct equilibrium condition of the whole system,

specifically of the blade sections, taking into account the wind shear and blade deformability. As later mentioned in the text (ll. 329-333 and ll. 428-431), we did also include a system for retroaction (which may work with any BEM method and does not explicitly require OpenFAST), based on the one proposed by Zanon et al. (2018). We decided to update the aerodynamic coefficients when the estimated difference in the AoA was higher than 0.5°. However, ice accretion was computed considering the W_{std} case, and the angle of attack increased approx. by only 0.4° at the end of ice accretion. Thus, the retroaction system has never come into operation during this specific simulation, and the description of the method was omitted in the paper. It will be interesting to analyse the effect of roughness on the ice shapes themselves through this retroaction system. However, such analysis should be carried out once more accurate roughness and impingement models have been developed.
We have provided more details where OpenFAST is first mentioned in the text together with ice accretion (ll. 158-161).

Reviewer Comment
[Line 146] It seems to me a good idea to define an average power value. But why use the Weibull distribution rather than another one?
Authors Response
The Weibull distribution that we use, with $k = 2$ to match a Rayleigh one, is the one advised by the DNV-GL Guideline for certification of wind turbines in the standard wind turbine class.
We have specified it in the text (ll. 190-191).

Reviewer Comment
[Line 206] This is not clear to me how and why this extrapolation is performed. If it is common practices, is there any reference available?
Authors Response
Extrapolation is standard practice and is carried out for two reasons, i.e., to guarantee the convergence of the BEM method, which is iterative and may require data outside of the range provided without extrapolation, and to provide high-AoA aerodynamic coefficients for the root sections. We used the Viterna Method for extrapolation (Viterna and Janetzke, 1982). We also forgot to mention the correction of the 2D polars for 3D effects, which modify the behaviour in post-stall conditions. In this case we used the corrections by Du and Selig (1998) with Eggers CD adjustment (Eggers et al., 2003). These were only mentioned at the beginning of Section 2.
We have included the references to the methods, the extrapolation tool used and the inputs in the text (ll. 146-150; 171; 249-250).

Reviewer Comment
[Line 209] "the average flow field is resolved down to the Kolmogorov length scale." This seems misleading to me. This looks more like the definition of DNS simulations. The low-Re approach is related to the description of the turbulent boundary layer structure and requires y+=1 to capture the region of the viscous sublayer.
Authors Response
Thank you for noting.
We have corrected the text (ll. 255-258).

Reviewer Comment
[Line 236] Is there any reference for uhMesh? What kind of mesh generation technique is used?
Authors Response
We forgot to add the reference. The O-grid surrounding the airfoil is generated with an advancing-front technique, with the possibility of adding triangles locally. The unstructured grid around it is generated using a Delaunay triangulation, which was computed with a Bowyer-Watson algorithm.
We have added the reference and included details on the generation algorithms when the grid was first introduced (ll. 262-263).

Reviewer Comment

[Line 247] "The output had a 1P component". What does that mean?

Authors Response

It is a periodic oscillation with its main frequency corresponding to the rotational frequency of the rotor. The text also contained an inaccuracy since the 1P frequency is caused not only by blade flexibility, but also by the wind shear and by the tilt and cone angles of the rotor.

We have specified the meaning of the term in the text, fixed the inaccuracy, and added quantitative data about the oscillations of α *and* V_{rel} *(ll. 330-332).*

3. Validation

Reviewer Comment

[Line 273] Since the description of the setup is diluted over several sections, it is not fully clear to me what experimental conditions are simulated in section 3.1.

Authors Response

Thank you for your comment.

We have reported the experimental conditions and references at the beginning of the section (ll. 371-375).

Reviewer Comment

[Line 298] Since the residual seems to be of importance for the methodology, it would be worth describing exactly how it is computed.

Authors Response

It is first worth spending some words on the iterative process to compute the collection efficiency. We used a strategy to automatically refine the seeding region by adding new particles where needed. A uniform seeding front was initialised as a linear grid with equally spaced elements. At the first iteration, the parcels not hitting the airfoil (except for the two innermost ones) were identified and removed so that the seeding front was reduced in size. The first two parcels flying just above and below the object were not removed, so the impingement limits were also refined. Then, elements were incrementally split at each iteration, evolving the current cloud front and computing the collection efficiency β on the target surface. The simulation stopped when the difference in the $L2$ norm between two consecutive iterations of computations of β is below a user-supplied threshold. This is what we meant by "residual" of the k[th] iteration. The term residual is indeed inaccurate. We will refer to it as "error":

$$\|\mathrm{err}\beta\|_2 = \left(\sum_{j=1}^{n} \left[\left(\beta_j^{[k-1]} - \beta_j^{[k]} \right) \Delta s_j \right]^2 \right)^{\frac{1}{2}}$$

We have added a paragraph in Section 2.3 describing this process. We have defined the quantity $\|\mathrm{err}\beta\|_2$ *and replaced the misleading term "residual" accordingly (ll. 307-318; 405; 406; 413; caption of Fig. 12 and 13).*

4. Results and discussion

Reviewer Comment

[Line 327] Does the roughness always cover the whole ice surface (in the std and ext case)? On the contrary, can it cover the blade surface further than the ice?

Authors Response

Roughness always covers the ice surface. In the *ext* case, it covers both ice and goes 0.44m further than the end of ice impingements limits.

For clarity, we have rewritten Section 2.4 including more details, added Figure 4 defining the std and ext cases, and added Table 3 defining the test matrix (ll. 349-369). Then, in Section 4.2, we have referenced the updated section, the table and the figure (ll. 449-450).

Reviewer Comment
[Line 338] "the ice shape was mainly responsible for the aerodynamic penalty". It would be interesting to know the polar for the smooth-wall simulation of the iced shape to support this assertion.
Authors Response
The plots of the aerodynamic coefficients already contain five curves each and we think that adding one may lead to a lack of readability. After evaluating if adding the smooth-wall-iced coefficients in either a $C_L(C_D)$ curve or a $C_L/C_D(\alpha)$, we opted for the latter. The figure was added for Section B, being between Section A and Section C. Results for the other two sections are comparable. For completeness, we have included the results of the clean section without transition modelling in the figure, to partially address the next Reviewer comment.
We added Figure 21 in the manuscript and modified the text accordingly (ll. 463-468).

Reviewer Comment
[Line 343] "due to the supposed early transition", I do not understand this early transition. Is the transition modeled for the rough-wall simulations? If not, wouldn't it be fairer to compare against the clean simulations without transition?
Authors Response
Transition is not modelled for rough wall simulations. However, we believe that the assumption of fully-turbulent flow is reasonable and produces more accurate results than considering a transition model not accounting for roughness (such as the one available in SU2). In reality, the presence of roughness causes an almost instantaneous transition of the flow. However, the transition region can rather long. According to Feindt (1957), $Re_{k_s,cr} = \frac{\rho U_\infty k_s}{\mu} = 130$ is the critical k_s Reynolds number for roughness to affect transition. With Re_{k_s} increasing, the width of the transition region decreases, and the transition point is moved upstream. Re_{k_s} on the outer half of the blade rotating at 11 rpm ranges from 750 at mid-span considering $k_s = 0.3 \cdot 10^{-3}$ to 15000 at the blade tip considering $k_s = 3 \cdot 10^{-3}$. For this reason, we think that it is correct to compare the fully-turbulent rough simulations with the simulations of the clean airfoil accounting for transition. However, this shouldn't be the case at the beginning of ice accretion. Roughness should be small and the effect of the ice shape almost negligible. For this reason, in Figure 21 we have included also the efficiency of the fully-turbulent airfoil, to add the effect of the anticipated transition in the qualitative "superposition of effects" proposed in the figure.
We have added a comprehensive motivation of this fully-turbulent hypothesis at the end of Section 2.2 (ll. 270-287).

Reviewer Comment
line 356, "This effect is peculiar since roughness should have little effect on the aerodynamic coefficients when ice horns are well developed." Is there an explanation?
Authors Response
The extended roughness region caused a high increase in skin friction in a geometrically smooth region, increasing the viscous drag. Moreover, the flow expansion on the suction side of the sections and compression on the pressure side were lower, causing a noticeable reduction in lift. The effects were more and more noticeable as k_s increased.
We have added this explanation in the text (ll. 487-490).

Reviewer Comment
[Line 399] "Once more, our results agree with those by Etemaddar et al.", in which sense do the results agree? They may be consistent with each other but they are not in agreement (except if the figures in the table are wrong).

Authors Response
Thank you for noting. We wanted to highlight the consistency between the results, indeed.
We have corrected the text (ll. 536).

Technical Corrections

Reviewer Comment
Lines 62 and 66: 20 microns, 25 microns
Authors Response
We prefer to keep the metric system unit symbol for consistency with the rest of the paper.

Reviewer Comment
[Figure 10, page 14] Why are there systematically 2 curves for "Clean" (and the slope is not recovered)?
Authors Response
The two red curves represent the analytical relations for the viscous sublayer ($u^+ = y^+$) and the logarithmic region of the boundary layer $u^+ = \frac{1}{\kappa}\log y^+ + 5.1$ and were included for reference only. The label of the first subplot was wrong as well (it was supposed to be $k_s^+ = 282$).
We have updated the figure (now Figure 11): (1) by merging the two red curves into a single one; (2) by correcting the value of k_s^+ on the first subplot; and (3) by replacing the lower row with results from another simulation with $k_s/c = 0.005$, i.e., one order of magnitude greater than the first row. The last point was made to be consistent with both the "W" and "S" cases analysed later. The text was modified accordingly (ll. 392-401).

Reviewer Comment
[Line 370] define what TSR and Cp are
Authors Response
Thank you for noting that the definition was missing.
We have added a brief description of the CP-TSR curves in the Methodology and defined the quantities (ll. 172-179).

Reviewer Comment
Reference Lavoie et al (line 494): The journal article https://doi.org/10.2514/1.C036492 is probably more accessible to most readers
Authors Response
Thank you for noting.
We have updated the reference (ll. 653-654).

Reviewer Comment
Reference McClain et al (line 498): The journal article https://doi.org/10.4271/2019-01-1993 may also be more accessible to most readers than the conference article referenced. However, this is not exactly the same topic (although the main information that roughness evolves both in space and time is also given).
Authors Response
Thank you for noting.
We have kept both references to provide the more accessible article as well (ll. 661-664).