<u>RC2</u>

I thank the authors for satisfactorily addressing or answering part of my comments. I would like to report the following points.

1) In my previous review I wrote: Page 2, Line 34. The novel aspect of this work is the modeling of high-resolution LE surfaces of an actual blade with LEE damage, that 35 was captured in the field using state-of-art optical 3D scanning technologies. This makes this study different from numerically assumed damaged blades with standardized damage profiles on the LE. There are some more published studies using CFD to study field-recorded erosion geometry or realistic patters, e.g. references 1-3 below. The geometry used in the CFD simulations of this report were also obtained with laser scanning of eroded leading edges. This type of studies should be mentioned in the literature survey. 1— Meter Forsting et al., A spectral model generalising the surface perturbations from leading edge erosion and its application in CFD, 2022. 2— Veraart M, Deterioration in aerodynamic performance due to leading edge rain erosion, MSc thesis and Delft University of Technology, Technical University of Denmark, 2017. 3—Ortolani et al., Multi-scale Navier-Stokes analysis of geometrically resolved erosion of wind turbine blade leading edges, 2022.

Authors replied: All of these article show extensive effort on generalisation of geometry based on scanned or numerically assumed geometry, which we have now included and referenced as such. The mentioned references do not report a 3D simulation of actual pure scanned geometry, as we have done on this study.

I would like to report to the authors that, despite what they stated above, they have not included the mentioned sources in their revised article. Perhaps the omission is accidental. If so, please amend. Please also state which of the three articles uses the 3D scan from a real turbine and which ones used scaled erosion profiles from a swirling arm rain erosion test.

In general, as I noted in my first review, the literature survey of this article is quite limited, and the authors are invited to make it a bit wider to better cover the state-of-the-art in the areas of the reported research.

Apologies, this was in fact an accidental omission. We have now included the refences to these works and indicated the differences on the testing methods.

2) In my previous review I wrote: Page 8, line 134: This was accomplished by increasing the constant from 2.193 to 3.29 in the onset equation.... This sentence cannot be understood without providing further detail, e.g. which constant of which equation is affected. ...

Authors replied: Further details on changing the onset constant has been referred to (Khayatzadeh and Nadarajah, 2014) ...

Yes, this was already written in authors' original submission. The point is that, in this reviewer's opinion the equation containing the altered constant and possibly a couple of other ones linked to the affected equation should be reported in this article and briefly discussed. This part of the transition model is relatively complex in terms of number of equations and constants involved. Providing the few equations I am referring to would improve the effectiveness, as CFD practitioners could more quickly and with no ambiguity identify the part of the transition model affected by the constant change. It would also help CFD practitioners who do not develop this transition model but use it, to more easily identify the affected parts of the model and test the change reported by these authors and the McGill colleagues. This is why I am suggesting again to report the affected equations and constants.

We have now included the onset equation and additional text to clarify this further (Page 9, line 165).

3) In my previous review I wrote: Fig. 12. Why are lift and drag curves of rough airfoil computed only up to about 8 degrees, whereas those of the clean airfoil extend to higher values of AoA? Same question applies to subsequent figures.

Authors replied: The transition location data was only available up to 8deg, thus the rough airfoil simulation were only conducted at these angles of attack. We have explained this on line 221

I do not think the authors have answered my question. I did not ask about the transition location, I asked about the lift coefficient in top-left subplot of Fig. 12. Experimental data for both clean and rough cases are plotted up to AoA 16 or thereabout. CFD results of the smooth airfoil also go up to 16 deg, but rough case stops at 8 degrees. The same happens for the drag coefficients. Why was this done? Were the rough wall CFD simulations not run for AoA between 8 and 16 degrees? Could you please explain this aspect in the manuscript?

We have now reworded the text, which reads (line 227):

The calibration study was conducted by using a cost function that minimizes the errors between the calculated and measured transition locations across the experimented range of angles of attacks (-4 to 6°). Transition location data was only available between -4 to 6deg within the LEES dataset, thus the rough airfoil simulations were limited within this range of angles of attack and not higher to reduce the extensive number of simulations for the calibration data. As a result, the following equation was established *for k_s as a function of roughness height (R_h) and density (R_D). This calibration equation (Eqn 10) was explicitly used to generate the results presented in Figure 12 - Figure 17.*

4) In my previous review I wrote: Was a mesh sensitivity study performed for the transitional analysis of the eroded airfoils? Please add a statement on this.

Authors replied: 2D mesh sensitivity studies were performed for the transitional analysis of the clean airfoil (figure 5-6), while 3D mesh sensitivity study for the scanned eroded geometry was only performed with SST model.

OK. However, I am doubtful that for this particular problem, the results of the mesh sensitivity analysis performed on the 2D transitional problem can be ‘extended’ to the 3D counterpart of this problem, particularly if the 2D mesh sensitivity analysis was performed with airfoil geometries which were not slices of the 3D scan. Moreover, in the case of the 3D scan of the airfoil, there are strong geometry and flow gradients also in the third direction. In our experience, achieving grid independence for the 3D transitional case for problems of this type is not straightforward, and it requires very large HPC resources. I would recommend to mention in the manuscript what the authors write above on mesh sensitivity analyses, because the results of the 3D transitional analyses may be affected by some uncertainty they may affect the quantitative estimates of the AEP losses.

Agreed and we have now explicitly added the following text (on line 385) to reflect on this:

The 2D mesh grid refinement studies were performed for the transitional analysis of the clean airfoil (Figure 5). However, due to the limited computational resources, the 3D grid refinement study for the scanned eroded geometry was only performed with SST model.