Review of the manuscript:

David Bretos-Arguiñena and Beatriz Méndez-López:

"Aerodynamic simulation of rough and eroded blades, AEP effect and mitigation using low drag vortex generators"

General comments:

The manuscript presents calculations on the influence of leading edge erosion on the performance of blade segments and discusses the effect of vortex generators (VGs) to mitigate the performance reducing erosion effects. Aerodynamically shaped VGs are presented, for which a higher performance is postulated compared to conventional VGs. The authors do not analyze the reason for this advantage and some questions remain unanswered (see below). For a blade segment with these VGs, polars are calculated by means of RANS simulations, which are then used to determine the influence of erosion and VGs on the AEP for a generic wind turbine.

The topic is relevant and the approach to reduce erosion-induced power losses by VGs is interesting. Unfortunately, the manuscript as presented does not meet the requirements for a scientific publication in WES. The flow physics analyses and explanations of the effects of the suggested VGs are almost completely missing, some results seem unplausible without further explanations and analyses, the manuscript is obviously written quickly and contains many typos, linguistic errors and only few literature sources. Overall, sound analyses have to be supplemented and the manuscript has to be completely revised to be considered for publication in WES.

Specific comments and remarks:

- The manuscript contains many unclear and linguistically incorrect formulations and needs to be completely revised.
- Abstract: The abstract is essentially a short summary and only the last sentence mentions a finding from the study. The presentation of the findings should be strengthened. In the abstract the calculation method (steady RANS?) should also be mentioned and it should be explained which "different technologies" are used in the calculations. Furthermore, it should be clarified in which respect the study goes "one step further".
- Line 7: "Conventional VGs" should be further specified (height, shape, …). In the remainder of the manuscript, conventional VGs are not distinguished regarding their shape although they have specific characteristics according to the current state of research.
- Line 16: Some references on VG studies done in the last 20 years should be provided.
- Line 21: In which way does roughness modify the flow characteristics of a fully turbulent flow? I would rather use "impacts" or "affects" instead of "modifies".
- Line 22f: Can the authors provide evidence (in form of references) that roughness elements act like first stages of blade erosion?
- Linie 29: In the manuscript often just the name of the CFD code is mentioned (L.35, L.47). The authors should also mention the method used (steady-state RANS? turbu-

lence model?) to enable the reader to better assess the work. The term "CFD" is too general.

- Line 32: The statement of a power reduction of 6-7% is too general. For which roughness height was this value determined and was a fully turbulent flow or a natural transition scenario considered?
- Line 37: „on the field“. Does this refer to “field measurements” or investigations in the area of simulating roughness effects?
- Line 45: Please insert a reference for the "Knopp" model.
- Line 51: What was varied with the 1000 different protuberances? Height, position, spacing, shape? The sentence is grammatically not correct.
- Line 64: The reference is not up to date, compare current status on WES (published 2023).
- Line 64/65: What are "combined transition and rough CFD simulations"? What does "actual eroded blade" mean? Was the actual shape of the leading edge erosion measured and taken into account in the CFD simulation? Which transition model was calibrated?
- Linie 70ff: The wording implies that VGs were developed for erosion problems. This sentence should be revised.
- Line 73/74: Please add a reference for this statement.
- Line 74: I suggest: “streamwise” vorticity
- Line 74: „on the blade surface“ → “within the blade boundary layer“
- Line 77/78: „Increment in drag“ compared to what? Compared to a natural tranistion scenario?
- Line 78/79: Sentence is unclear. Is meant that drag is not increased when VGs are placed on the lower side (which would surprise me) or is meant that the drag of the lower side is not increased by VGs attached to the upper side? The dot at the end of the sentence is missing.
- Line 79-81: The authors should describe in some more detail how Hansen et al. designed their aerodynamically shaped VGs and why the efficiency increase is spread over such a wide range. As a part of the literature review of the present manuscript it should additionally be mentioned that the $(L/D)_{max}$ of the clean airfoil is significantly higher than that of the airfoil equipped with VGs (even for the aerodynamically shaped ones).
- Line 82: Dot missing at end of sentence. Please use "Bay" or "BAY" consistently. What are the advantages of the fully resolved simulation of VGs and why did the authors choose this approach?
- Linie 82: The co-author published a study on low-drag VGs in 2018. Why was this paper not referenced and used as a basis for the present study? (doi: 10.1088/1742-6596/1037/2/022029)
- Line 86: Please be more precise. The power estimation was performed with BEM using (2D? calculated or measured?) polars for the blade sections without and with VGs/gurneys.
- Line 89//90: Please revise this sentence.

- Line 91ff: Please refer to the section numbers of your manuscript.
- Line 100ff: The description of the numerical model is considerably too scarce to evaluate its suitability for the present analyses. How many grid cells were introduced across the boundary layer height? Number of total grid cells, mesh topology, far field distance, turbulence model, steady RANS or unsteady URANS, numerical scheme...? In the remainder of the manuscript unstructured and structured meshes are mentioned and a comparison is shown in Fig. 2. To the reader it is not clear which calculations documented in the manuscript were performed with which mesh type.
- Line 101ff: “in clean” → “for clean”; “in the airfoil” → “along the airfoil”; throughout the manuscript: “y+” → “y^+”; “turbulent model” → “turbulence model”; “ICEMCFD” → “ICEM CFD”
- Line 103: Please first mention that a wall model is used for the rough case, then the requirements for y^+.
- Line 108 and throughout the manuscript: „eN” → “e^N”, “k-kl-w” → “k-kl-ω”. Remark: In XFOIL a simplified e^N envelope method is implemented, in which the frequency dependence of the TS amplification is not considered, in contrast to the original method proposed by van Ingen.
- Linie 113: 5e^{-3} → 5 \cdot 10^{-3}. This should be used consistently throughout the manuscript.
- Line 118: The experiments are well reproduced by the calculation except for the stall domain.
- Caption Fig. 1 and Fig. 2: The CFD results are only shown as lines, so that the chosen angle-of-attack discretization is not clear. I suggest to add some symbols.
- Caption Fig. 2: „eN turbulence models“. The e^N method is not a “turbulence model” but rather a “transition prediction model”.
- Fig. 3: In the legend (XFOIL) I suggest to write "transition location upper / lower side (XFOIL).”
- Line 130: „shows the pressure distribution“ or „shows the distribution of the pressure coefficient“
- Line 149: „SST k-w“ → „SST k-ω“. Was this turbulence model also used in the previous calculations with prescribed XFOIL predicted transition location? Then this should be mentioned in Sec. 2.1.
- Figs. 10/15/16/18: Are the velocities in the first grid layer shown in the top views? Is the axial velocities component shown or the absolute value of the velocity? The legends in the top views are too small and partly blurry.
- Table 1 / Fig. 8: (α in [º], $\Delta(C_l/C_d)_{max}$?!). Obviously, the calculations were only performed up to an angle of attack of 14º and at least for the clean and the turbulent case without erosion, the theoretical $c_{l,\,max}$ values are obviously not yet reached. This angle should therefore not be listed as $\alpha(c_{l,\,max})$ in Table 1.
- Line 167f: VGs only increase c_l in case of flow separation for the baseline airfoil, and particularly increase $c_{l,\,max}$ and $\alpha(c_{l,\,max})$. They do not increase lift for the complete AoA range. This should be expressed more clearly.
- Line 168: I suggest: “… added to the blade for attached flow”.

- Line 169ff: CENER proposes VGs whose cross-section feature a 10% thick aerodynamically shaped airfoil. Given the very small Reynolds numbers of the VG, flat plates are actually not a bad choice and provide well defined separation locations for the generation of the streamwise vortices. The authors initially claim without evidence that their aerodynamically shaped VG cross section show lower drag than conventional VGs, but it is not clear whether this statement refers to the self drag of the VGs or to the drag of the blade section equipped with these VGs. The authors do not provide an explanation or a reference. Fig. 14 then seems to support the claim. It is surprising, however, that the airfoil with the proposed VGs shows a significantly lower drag than the turbulent airfoil without VGs, even at small angles-of-attack. This is not plausible to me and is also in contrast to previous studies. For example, it was shown by Hansen et al. (doi: 10.1002/we.1842) that for the (aerodynamically shaped) VGs they studied, the L/D_{max} of the blade section with VGs is reduced. In the present manuscript a sound flow-physics analysis and a physical explanation of the beneficial effect of the proposed VGs is urgently needed! Does the turbulent airfoil without VGs show flow separation even at small angles of attack, which are suppressed or delayed by the VGs? If so, how can it be explained that the conventional VGs do not suppress the separation? Are mean momentum and displacement thicknesses of the boundary layer reduced by the VGs? Here, an analysis of the boundary layer development without / with VGs as well as an analysis of the flow field and of skin friction and pressure drag components is necessary.
- Line 174: The aerodynamic behavior of VGs depends primarily on the geometrical parameters of the VG array (i.e., inclination angle, interspacing, intraspacing, VG height with respect to the local GS thickness, VG length on the airfoil surface and at the VG tip, positioning of the VG pairs with respect to each other). This should be mentioned in the paper. If necessary, the influence of these parameters should also be investigated. Hansen et al. showed that the inclination angle still produces a large increase in efficiency. Is this also the case with the CENER VG?
- Fig. 14: In this graph, the difference between conventional (Delta, Cropped and Rectangular) VGs is not visible. Can you zoom in to show the differences? Differences of conventional VGs should be predicted in qualitative agreement to results of previous studies to make the predictions for the proposed VGs more plausible.
- Line 184: Was the boundary layer of the VGs actually resolved in the present calculations? What criteria were used to ensure accurate meshing of the VGs? It is unclear how a meaningful refinement in the VG region was achieved without increasing the total number of grid cells.
- Linie 197f: The parametric study of the chordwise VG position for rough conditions should be shown in this work. Since the boundary layer profiles are significantly changed by roughness, it may not be feasible to use best practices for clean airfoils. A detailed study of the boundary layer parameters would add scientific value.
- Linie 197ff: Regarding their efficiency the different types of conventional VGs are not distinguished in this statement, which does not reflect the current state of research (see Schubauer et al. https://doi.org/10.1017/S0022112060000372 or more recently Baldacchino et al. 10.1002/we.2191).
- Linie 199f: Vortex formation of VGs is a complex topic, which cannot be covered by seemingly arbitrarily placed streamlines. More meaningful properties (velocity profiles in the wake, boundary layer parameters or cross-flow kinetic energy (see dissertation Florentie https://doi.org/10.4233/uuid:704d764a-6803-4cad-991f-45dc4ea38f6d)) should be used here.

- Fig. 15: What is the meaning of this figure, the text does not contain any interpretation? Why is the flow field around the new VGs shown in a different way in Fig. 16? Here, a representation as in Fig. 15 a) b) c) and a direct comparison with the flow fields of the conventional VGs in connection with a flow-physics analysis would make sense. What is the origin of the geometric parameters of the three configurations? Is the thickness of the VGs to scale?
- Fig: 16: The figure does not add value as it is not suitable for assessing the low drag VG. Quantitative analysis should be provided here, e.g. as presented by the co-author at Torque 2018 (doi :10.1088/1742-6596/1037/2/022029).
- Line 208/209: The statement "...recovery of the airfoil efficiency..." is exaggerated. The improvement in performance is actually very small. Furthermore, I cannot understand the values given in Table 2 for $\Delta(C_l/C_d)_{max}$. According to the table, config. 2a should show a L/D reduction of more than 32% compared to turbulent flow, which I cannot see in Fig. 17. I ask the authors to check and correct the values listed in table 2.
- Fig. 18 / 19 and associated text: How were the separation locations (65% and 75%) determined, for which spanwise range are the c_f values shown in Fig. 19 and could you show spanwise averaged distributions? What is the reason for the strongly positive or negative c_f values at 30% chord? How can a single small VG pair have such a strong influence on separation location of a blade section with such a large span? More generally, I also wonder why such a large span was considered in the calculations, but only one VG pair modelled. Would it not have made more sense to use a segment with a smaller width and periodic boundary conditions?
- Line 226: What are the „corresponding polar curves“?
- Sec. 5: Conclusions consisting only of a list are unusual. Some of the aspects mentioned were not discussed in the manuscript and may not be included in the conclusions, for example line 247-249. Other listed conclusions actually represent an outlook: line 255-256, line 262-264. The conclusion line 261-262 was not discussed and verified in the manuscript.

Typos:

- Line 33 and throughout the manuscript: „where“ → „were“
- Line 64: “quatified” → “quantified”
- Line 126: “tree” → “three”
- Line 145: “show” –“shows”, “in the surface” → “on the surface”

Linguistic inaccuracies:

- Line 15 and throughout the manuscript: “over the performance” → “on the performance”
- Line 97 and throughout the manuscript: “typologies” → “types”
- Line 132: “the pressure over the airfoil surface value”. Please revise.
- Line 136/137: please revise “inspired in”, “consist in pit and gauges”.
- And many more