Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-44-RC3, 2020

WIND ENERGY SCIENCE DISCUSSIONS

WESD

Interactive comment

Interactive comment on "Virtual full-scale testing for investigating strength characteristics of a composite wind turbine blade" *by* Can Muyan and Demirkan Coker

Anonymous Referee #2

Received and published: 12 May 2020

In the paper, the authors investigate the static strength behavior of a 5 m wind turbine blade by means of finite element simulations. For this purpose, they utilize the well-known Puck failure criteria for the composite parts, both in a linear formulation and a non-linear degradation version. The linear version simply evaluates the strength criteria without stiffness degradation. The non-linear degradation version reduces the material stiffness in each ply whenever the Puck criteria are fulfilled and identifies laminate failure when three plies are failed. The authors analyze static extreme loads in flapwise, edgewise, and combined flap/edgewise directions and compare the results.

The topic of full-scale failure analysis of wind turbine rotor blades is generally interest-

Printer-friendly version

Discussion paper

ing and important for the wind energy research community. However, the manuscript does not represent a substantial, but rather a minor contribution to scientific progress within the scope of WES. The scientific approach and the applied methods are generally valid, but have weaknesses. At this point, the reviewer refers exemplarily to the incomplete iteration in the degradation model and the missing mesh convergence study (see below). The work is not reproducible, as there is no blade data available. The work can neither be repeated nor be verified by other scientists. The discussion of results is in parts not comprehensible, and does not include enough findings of other authors. The presentation quality is good in general. The text needs some revision due to typos and other minor language errors (that are too numerous to list).

In the following, the reviewer lists specific points of criticism that need to be addressed thoroughly in a potential revised version of the manuscript.

- The paper is an application paper rather than a research paper, as the utilized failure models are mainly already published elsewhere. The paper thus lacks novelty in the methodology development. The results are the novel content, but the basis for them, the blade design, is not publicly available due to confidentiality reasons. The added value of the paper for the research community is thus very limited. The fact that the results are neither reproducible nor verifiable is a strong weakness of the paper. A further weakness is the short length of the blade under investigation, which is not representative for modern wind turbines.

- Lines 16-17: The damage evolution is not necessarily linked to stiffness, as it is about stress and strength. It is easily possible to design stiff blades without damage, especially in the small size of the blade studied in this paper. The explanation via stiffness appears repeatedly throughout the text and should be discussed in more detail – or changed by more comprehensible argumentations.

- Equations (3) and (4): There are three equations, but only two numbers.

- Line 125 ff.: Why is the degradation model formulated in such a way, that the element fails in case that 3 plies fail? Why 3 plies? That sounds unphysical to the reviewer. Wouldn't a relative number with respect to the overall number of plies be more meaningful? As after the modification of the stiffnesses the load is incremented, the authors do not perform a full iteration for the material degradation in each load step, which also seems unphysical or illogical from a numerical point of view. The authors should explain in more detail why the degradation model is formulated in this form.

- Figure 4: The reviewer suggests to change the line formats in order to highlight the own results: Black dots for experiments, red line with dots for the simulation.

- Line 161 ff.: The argumentation on the finite element mesh is weak from a scientific point of view. The mesh is actually quite coarse in some regions of the blade, which should be avoided. The reviewer recommends to perform a mesh convergence study, which should generally be done for finite element simulations, especially in science. The simulation time of 4 hours is nothing spectacular from the reviewer's point of view, so there is definitely room for further mesh refinement.

- Line 168: What is the reason for the limitation of the simulations to geometric linearity?

- Figure 5: The mesh looks weird in some regions. At the trailing edge, there are strange curves in the first element edges. One is well recognizable at the bottom of Fig. 5 (b). What is the reason for those?

- Figures 6-7: What is the coordinate system underlying the moment and force directions? Are the moments extreme for all positions, or just one position along the blade? Is it a mixture of different DLCs and time instances? Which DLC is the basis for the extreme loads?

- Figures 8-9: The load introduction is strange. Why didn't the authors use contact elements (MPCs), which is the standard way for distributed load introduction in 3D structures in ANSYS? The way the authors realize the load introduction may lead to spurious and erroneous local deformations. Please comment on that.

- Figures 10, 20, 24-26: Which load and deflection components are plotted? Which direction of load and deflection? Total load vs. total displacement? Which point exactly is traced in the deflection? It is just stated “a point close to the tip”, which is imprecise.

- Figure 11: The quality of the text in the figure should be improved.

- Section 3 in general: The explanations of the results should be more precise. Why is the damage development as it is?

- Section 3.4: The reviewer does not understand why the combined loading is not the most severe one, as it should kind of add up the damage of flapwise and edgewise loading, especially in the linear part of Fig. 24. That holds for the entire section. The explanations have to be improved.

- Line 410: The authors state that the simulations have been carried out “before testing”. Are tests planned? If so, it will be interesting to see if the simulations match well with the test.

- Conclusions, point 2.: Please add comprehensible explanations for the findings. Otherwise, there is not added value for the research community.

- Conclusions, point 3.: This finding is natural, as the fibers have the job to carry stresses and to provide stiffness. The stiffness contribution of the matrix is very limited – that’s the nature of fiber composites.

- Conclusions, point 5.: How are the authors intending to increase the moment of inertia? By additional material or modifications in geometry?

- Conclusions, point 6.: The authors did not study the adhesives. How do they come to the conclusion that the main failure mechanism is expected to be linked to the adhesive joints?

- There are numerous language errors. No severe errors that make the paper unreadable, but the entire text needs revision.

- The reviewer highly recommends to make the blade data publicly available (including geometry and material layup), and also potential test results in the future. Otherwise, the value of the manuscript is, if present, very limited.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2020-44, 2020.