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## Reply on RC2

Pablo Noever-Castelos et al.

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Author comment on "Validation of a modeling methodology for wind turbine rotor blades based on a full-scale blade test" by Pablo Noever-Castelos et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-24-AC4>, 2021

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*Dear Martin Eder,*

*Thank you for taking the time to study this paper and provide valuable constructive criticism, which we believe has helped developing and strengthening this work significantly. Please find our answers to all your comments, by either corrected or added text sections or comments on your suggestions/concerns. We hope to have adequately accommodated all your concerns and thank you again for your contribution.*

### **General comments:**

**The introduction should contain a short paragraph (e.g. bullet point format) highlighting both the novelty and the significance of the proposed approach compared to readily established classic approaches.**

*The introduction was rearranged into subsections. Subsection '1.2 Objectives of this paper' contains the following paragraphs:*

*"Though some of these model creation frameworks may work with functions or splines describing the blade's geometrical or layup information, most of them work with a reasonably high number of airfoils/stations that in addition to the blade's geometry yield the outer blade shape by a global linear or higher-order interpolation between the airfoils.*

*The presented method combines and extends several aspects of the different aforementioned software packages. The benefits are:*

- *it generates airfoils independent from any neighboring geometry and uses the relative thickness distribution to position these along the span. This assures the geometry distribution, as this avoids any overshoot due to spanwise geometry interpolation.*
- *any parameter which may vary over the radius can be defined as spline, e.g., relative blade thickness, layer thickness, material density or stiffness.*
- *it enables flexible and easy parameter studies due to the simple parameter variation based on splines.*

- *it is designed for research, as different modules can be easily replaced by an alternative code, e.g., airfoil interpolation, adhesive modeling.*
- *it generates an FE-Model in MATLAB and provides already an interface to ANSYS APDL and BECAS, however, interfaces to other FE-software can easily be implemented."*

**The authors conclude that shell elements do not lend themselves to predicting the torsional twist distribution along the blade. This problem is well-known to persist (also) in blade models and has been previously reported in the literature by many different researchers (see attached literature for inspiration). The problem is related to the midplane offset function and the integration scheme of the shell elements. The offset is used to form a smooth outer surface defined by the airfoils.**

*This problem is already shortly discussed at the end of Section 5.3. It was now extended by two further references:*

*"Such high errors during torsional loading may base on the shell element with a node offset to the exterior surface used for this model. Greaves.2021, Branner.2007, Pardo.2005 and especially Laird.2005 already stressed the high inaccuracy of shell elements with node offsets from mid-plane to predict the structural behaviour of hollow structures subjected to torsional loading. However, the twisting is generally overestimated throughout the three torsional tests, which is inline with the aforementioned references."*

**It is commonly accepted that the accuracy of the torsional stiffness prediction can significantly be improved when adopting layered solid elements or continuum shell elements. This solution of course comes at a price of computational efficiency. However, the pros and cons of this solution should be highlighted in the conclusions.**

**&**

**The authors state that the measured and predicted strain distribution in their torsion tests is largely at variance. This statement is actually quite significant and maybe alarming: numerical fatigue damage prediction, continuum damage models, equivalent strain envelope for fatigue tests to name a few. The authors should provide a more thorough explanation for possible root causes. Is it deemed to be a measurement error or rather a simulation error (i.e. modelling artefact)? Did the predicted strains improve when using a different element type?**

*We fully agree to this statement and the necessity with concerns to:*

*"However, modern flexible blade design, which is driven to it's material and structural integrity limits and includes intentional torsion for load alleviation, requires accurate predictions for all load cases in order to be reliable. Looking a step further, especially fatigue damage calculation, therefore, needs correct strain or stress predictions of the models."*

*This is specially important, with respect to the error in torsional prediction.*

*"As literature reports, all this may be traced back to the shell elements being inappropriate to model torsional behaviour, due to the offset of the nodes to the element's*

*mid-plane.”*

*And this is why:*

*“The authors currently work on evaluating blade modeling by means of solid elements and/or solid shell elements. Although, we loose computational efficiency of the shell element models, this way, the accuracy in torsional response should be improved significantly, as well as the correct representation of geometrical shape and 3D tapering can be realized. This should shed light on the discrepancy in torsion and some of the bending load cases, where we were unable to identify their origin, for instance wrong curvatures in the strain distributions or numerical steps/peaks at material tapering. However, such very local effects as material discontinuities and numerical strain/stress peaks, probably require a global-local approach to capture every smaller scaled detail. Subsequently, a sensitivity study of relevant geometry, material or modeling parameters can enhance further the understanding of local inaccuracies.”*

**The authors have measured the torsional twist through application of a force couple on the saddle or yoke mounted on the blade. It is not entirely clear how the sway induced by the bend-twist coupling was considered in the measurement. Secondly, the bend twist coupling response might entail a rotation of the cross section closer to one (or at one) of the two loading points rather than the centre. Moreover, if the load was applied through cables, the bend-twist coupling effect might have changed the direction of the two force vectors. The authors should provide more detailed information regarding the test and measurement procedure. One (maybe of many) alternative testing method could have been to attach a transverse lever to the yoke. One test LC1 is performed by conducting a pure flapwise test by loading the yoke centrally. A second test LC2 is performed by a combined flapwise-torsion test by loading the yoke at the tip of the lever. Using the superposition principle, the torsional twist can be found by subtracting LC2 from LC1. However, the authors should explicate their reason for choosing their method and why they consider it superior to other existing approaches together with possible limitations of their approach.**

*We hope the following paragraphs describes the torsional test accurately enough and give an explanation why we have chosen this method:*

*“Because the blade is still mounted at a block angle of  $7.5^\circ$  the torsional moment is not fully aligned with the pitch axis, as the forces do not act exactly in the cross-sectional plane. The load cable oriented upwards was attached to a ceiling crane and to the load frame at approximately the shear center position. As the ceiling crane location is hard to record, but the load rope is perpendicular to the ground it was assumed that the location is 18m above the corresponding load point ( $y$ -direction, approximately crane height). By this, deflections parallel to the floor, due to load application, would only result in small angle deviation of the perpendicular force. The force facing downwards was applied onto the load frame corner to create the lever with respect to the shear center. Our procedure is similar to a combination of the pure torsion and locked torsion test presented by Berring,2007. However, this method may imply some errors from:*

- *numerical shear center calculation*
- *not suspending exactly at the cross-sectional shear center, but on the frame, which leads to an offset of the suspending force when the blade is twisted and thus an induced counteracting torsion.*
- *no exact perpendicular downwards facing force*
- *inclination of the blade*

*Regarding point 2, the offset of the load application point of the suspending cable from the numerically calculated shear center after twisting the blade, yield to 0.9%, 3.0% and 5.3% of the respective lever for the downwards facing force on LF2, LF3 and LF4, respectively. Theoretically, expecting a similar force pulling upwards as downwards, the induced torsion is reduced by the same relative values for the respective load cases MZLF2, MZLF3 and MZLF4.*

*The magnitude of the induced torsion was designed to be the maximum allowable torsion (respecting safety margins) at the particular cross-section, rather than a possible bend-twist induced torsion magnitude. This was motivated by the certification idea of proofing the maximum torsion.*

*Nevertheless, overall the aforementioned errors do not practically affect the validation of the model, as all DWS and load cables are modeled as LINK11 elements. With all attachment points modeled at their correct global location of the test setup. This assures that the forces and displacement measurement direction is always correct throughout the test, all under the assumption that the model behaves the same as the real blade. Thus no corrections of any kind to measurements or FE results were applied."*

#### **Detailed comments:**

**Line 26-27: The authors state that 3D modelling is required to obtain a more reliable blade design. Please emphasise which structural behaviour cannot be captured by cross-section analysis tools – compelling the use of 3D FE models. For instance the inability of the stepwise prismatic approach to capture longitudinal geometric variations, particularly the effect of taper or any other local discontinuities, local buckling analysis, decoupled cross-sections etc.**

*"Nevertheless, at a final stage 3D FE analyses have to be performed in order to obtain a reliable blade design and account for structural details, e.g., adhesive joints, longitudinal geometric discontinuities, ply drops, or local buckling analysis, which are not considered in a 2D-FE-analysis."*

#### **Line 69: Typo / reference missing**

*Corrected.*

**Line 184: Please explain the meaning of a qualitatively satisfying mesh density. Especially in a validation process of local strains predicted by a FE model as presented in the paper, a mesh convergence study is crucial. How can the authors be confident that the deviations owing to local mesh discretization are indeed negligible?**

*"A mesh convergence study was performed in advance to ensure a satisfying mesh density. As stated before the purpose is to validate primarily the global blade behaviour and only secondary the local response. Therefore, no local mesh refinement will be performed, but the overall mesh density should yield acceptable convergence even at local level. Taking this into account, the convergence was first based on the global blade response in terms of total mass, center of gravity, tip deflection and the first 10 natural frequencies. Secondary the nodal strain results are examined for convergence at several position covering the whole blade. The element dimensions are halved each step. At the finally chosen mesh size the deviations to the next step are: global responses < 1.5%,*

*strains < 2.1  $\mu$ -strains. It has to be stated, that for exact local strain measurements a modeling approach with solid-shells or layered solid elements is required to replicate the correct and detailed geometry of the structure."*

**Caption Fig.5: It is not entirely clear what an erroneous shear web adhesive joint indicates. Please explain more thoroughly.**

*The figure is extended by a marking in the picture highlighting the failure in the bonding. Additionally, the caption is extended by:*

*"The width should cover the complete web flange and the designed thickness is 9 +- 3 mm, however, the real thickness is measured to 33 mm."*

**Lines 424 – 435: The authors state at several places that they do not have a feasible explanation for the strain deviation. It would be important to get the authors opinion which strategy they suggest in order to shed light on this issue (maybe in the conclusions). The authors state that the vanishing sandwich core material caused a numerical strain peak. It would be interesting to know the authors opinion on how to practically deal with local material discontinuities in numerical modelling strategies.**

*See the comment to your last general comment.*

**Figures 12 and 13: Needs a drawing indicating the location of the SGs and their direction.**

*The cross-sectional sensor position is illustrated in two figure for each cross section in the appendix.*