Comment on wes-2021-24
Martin Eder (Referee)

The authors are presenting a fully automated modelling tool that creates finite element models of wind turbine rotor blades from airfoil and plybook information. The numerically predicted global response of the 3D finite element blade model generated by this tool is validated against a static physical full-scale blade test. The authors compare and validate the predicted global bending displacement response, the torsional response and the strain distribution in the longitudinal and circumferential direction.

The topic of accurate high fidelity blade modelling is relevant. The manuscript is well written and represents a comprehensive treatise of several key topics in blade modelling. The structure of the paper allows the reader to follow the content easily. A thorough literature review is presented.

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 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 mid-plane offset function and the integration scheme of the shell elements. The offset is used to form a smooth outer surface defined by the airfoils. 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 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.

- 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?

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.

Line 69: Typo / reference missing

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?

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

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.

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

Please also note the supplement to this comment: https://wes.copernicus.org/preprints/wes-2021-24/wes-2021-24-RC2-supplement.zip