

Interactive comment on “Wind tunnel tests with combined pitch and free-floating flap control: Data-driven iterative feedforward controller tuning” by S. T. Navalkar et al.

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Response to reviewers

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To
Reviewer 1
Wind Energy Science

Dear Reviewer,

First of all, the authors would like to thank the reviewer for their positive and constructive feedback. We believe that the comments have helped us improve the quality of the paper. In our attempt to account for the comments, we plan to revise different aspects of the paper. The objective of this document is to respond to the points raised by the reviewer and to provide a detailed overview of the changes being made to the paper.

Yours sincerely,

Response to comments of Reviewer 1

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1 Response to comments of Reviewer 1

1. Page 1, line 7. "The inertia of the flaps was tuned..." Nowhere in the paper the authors present this inertia tuning. Please comment on this.

During the design of the experiment, the flutter analysis conducted on the numerical model of the blade showed high sensitivity to the exact value of the flap inertia about its free hinge axis. As such, a sensitivity analysis was conducted to ensure that the blade does not enter the flutter regime prematurely, and that the system is stable in the intended regime of operation. This sensitivity analysis was omitted from the paper, but, as per the feedback of the reviewer, could be included in the revised version of the manuscript.

2. Section 2. The authors write they have scaled the INNWIND.EU rotor, but the scaling laws are not reported (time ratio, length ratio, Lock, Reynolds, etc.). Moreover, the scaled model is a 2-bladed rotor, the INNWIND.EU a 3-bladed. The only information provided is (page 3, line 24-25) is on the first blade freq. wrt the 1P (>3.5) which is a standard value for all 3-bladed rotor (to avoid intersections in the Campbell diagram between the blades and the $1/2/3P$). It looks like the scaled model is a proof of concept of technology more than a scaled model of a full scale wind turbine, so that the table 1 and the reference to the INNWIND rotor should be removed. Otherwise the authors must give more info about the scaling.

The authors agree with the reviewer that the description of the scaling, especially with relation to the INNWIND.EU rotor, is inadequate. However, due to practical considerations, scaling was only done to ensure that the frequency ratio of the first blade mode with respect to 1P remained constant. As such, the authors agree that this scaled turbine simply represents a model for the proof of concept, and the reference to the INNWIND.EU rotor will be removed.

3. Section 2.1. No information is provided about the aerodynamic design: how the authors have chosen airfoils, chord and twist distribution. May the authors include

some more info about this and some more data about the overall performances (power coefficients VS TSR, for instance)?

The reviewer raises an important point, it needs to be mentioned that the aerodynamic design of this specific rotor follows the design approach of Van Wingerden et al. (2011), where a similar rotor, but with conventional flaps (and no pitch control) was tested under similar experimental conditions. The authors of the current paper will provide the necessary references, for instance "Design of a scaled wind turbine with a smart rotor for dynamic load control experiments" by Hulskamp et al, 2011, and related, where a detailed analysis of the aerodynamic design and other performance characteristics can be found.

4. Page 5, lines 5-10. The authors present a mismatch between the measured and calculated structural behavior. The reason of this has been identified in the anisotropic behavior of the real blade not modeled in the isotropic FEM model. The authors should comment why they have not tried to identify this anisotropic behavior on some specimens (as done for Figure 1...) and then used an anisotropic FEM model.

As noted, there is indeed a difference between the measured and calculated structural behaviour. The manufacturer-published values of anisotropic material properties were used to estimate the stiffness of the blade. However, it was observed that the stiffness of the blade is not significantly affected by using an anisotropic model. The authors retested the stiffness of the blade, and it was found that the clamp used for fixing the blade root was not ideal, but allowed rigid-body rotation of the blade. The rotation of the blade root with increasing load was measured, and compensated for in order to correctly estimate the actual stiffness of the blade. [IF THE FIGURE IS NOT VISIBLE, PLEASE REFER TO THE SUPPLEMENTARY PDF ATTACHED]. The corrected figure will be used in the revised manuscript.

5. Page 7, line 10. "...adds an additional rigid-body degree of freedom": this com-

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stiffplotmod.png

ment is unnecessary because this is well-known; moreover this comment does not need to be supported by two (auto)citations.

[The authors agree that the statement is self-evident, and will be removed.](#)

6. Page 7, lines 18-22. The discrepancy (about 20%) between the two mathematical models is an error in the modeling: a correct FEM model and a correct cross-sectional code + beam model can give the same (correct) results. In a journal paper this should be correct. Moreover, both the models return huge error wrt the real one (see previous point...).

[Indeed, there is a large difference in the modal description of the two mathematical models. This was a result of a difference in the clamping conditions: while the blade root section was completely clamped in the NASTRAN model, only the con-](#)

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necting nut was clamped in the Solidworks model. When this discrepancy was removed by correctly clamping the Solidworks model, the difference between the two models reduces to a large extent; the recalculated difference being less than 2.5%.

- 1st Flapwise frequency: 18.97 Hz (Solidworks), 19.44 Hz (NASTRAN)
- 1st Edgewise frequency: 78.37 Hz (Solidworks), 76.67 Hz (NASTRAN)
- 2nd Flapwise frequency: 84.8 Hz (Solidworks), 87.88 Hz (NASTRAN)

The revised manuscript will contain this updated modal analysis. As far as the difference between the numerical and experimental model is concerned, it should be noted that the numerically calculated values are blade frequencies; the rotor modal frequencies measured in practice are also influenced by the blade connection flexibility, motor stiffness and hub flexibility, and as such are necessarily lower than the numerically calculated blade modal frequencies. The difference, or uncertainty, in rotor modal frequency estimation is a powerful motivation for the choice of a self-tuning regulator as the load controller, since it can adapt to the true system parameters automatically. This explanation will be included in the manuscript.

7. Page 9, lines 1-2. This is not clear. The airspeed 36m/s refers to the scaled or the full scale model (it looks the full one...)? 340rpm is the scaled one. Probably the authors should present a regulation trajectory of the (scaled) wind turbine (i.e. rotor speed VS wind).

The terminology "air speed" does appear unclear, it is the resultant air speed incident on the blade, which is a combination of the inflow wind speed and the wind speed induced by the rotation of the blade. This definition will be made clear in the revised manuscript. The regulation trajectory of the scaled wind turbine is linear, and as such can be described by a single coefficient denoting its slope,

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which is 51.1 rpm/(m/s). This value will also be stated clearly in the revised manuscript.

8. Page 9, figures 13-14. The flutter analysis presented here looks more the one used for fixed wing (i.e. uniform airflow on the blade, constant AoA, no rotation). Is it also applicable on a rotation blade? Please add some comments in the paper about this flutter analysis. The wind speed on the x-axis refers to the scaled model?

As pointed out by the reviewer, the flutter analysis presented is indeed carried out for a fixed wing, with uniform airflow and constant angle of attack. The effect of rotation is only included in the sense that the incident wind speed is not the inflow wind speed, but rather, the total air speed as defined in the previous comment. This forms a first approximation to the true flutter behaviour of the rotating blade, the inflow conditions along the span of the blade are held constant to those obtained at the tip section. While the error in the aerodynamic forces increases for the sections radially inboard, these sections also undergo smaller structural motions and hence contribute to a progressively smaller extent to the aeroelastic behaviour and hence the modal analysis of the blade as a whole. Further, the blade is typically twisted such that the angle of attack remains constant throughout the blade span; this is approximated by using aerodynamic panels with no twist, and with a constant angle of attack, along the span of the blade. As such, it is assumed that the flutter analysis of the non-rotating blade is a good approximation of the dynamic behaviour of the actual experimental blade. This explanation will be included in the revised manuscript. Finally, it should be pointed out that the “air speed” in the Figures 13 and 14 refers to the total air speed, as defined in the previous comment; this will also be clarified.

9. Page 16, lines 4-7. Again more information about the operation of the model is necessary: if the rated speed is 4.5m/s at 230rpm, at 6m/s the rotor speed of a classical variable-speed pitch-regulated wind turbine is again 230rpm (i.e. in the

above-rated region the rotor speed is kept constant). The authors must better define the regulation of the model.

Thank you for this remark. As described in the second paragraph of Section 4, the load resistance connected to the wind turbine generator is kept constant; this constant load operation implies that the rotor speed increases linearly with the wind speed at a rate of 51.1 rpm/(m/s). This is indeed not similar to the operation of a classical variable-speed pitch regulated wind turbine, where collective pitch control is used to ensure that the rotor speed remains constant (above-rated) irrespective of the change in wind speed. However, the constant resistance operation of the turbine serves three main purposes: it describes the effect of (temporary) overspeeds which may cause the turbine to enter flutter, it emulates the behaviour of the turbine under below-rated conditions, and it describes the potential of an adaptive control strategy that may be required to retune itself under varying operating conditions. This description and motivation of constant load operation will be clarified in the revised manuscript.

10. Some figures may be more readable if different line styles are used (i.e. solid, dash-dotted, dotted, etc.). This helps if read on black/white copies or by color-blind person. Page 6, line 19. Add extra space: "Finally,an".

The figures and the typo will be revised as advised.

11. Page 8, fig 9-10. Please check these figures, because they look inverted (Fig. 9 looks the first edgewise mode...).

The figures were checked and found to be correct. The flapwise modes display deformation perpendicular to the aerodynamic panels, while the lead-lag mode displays deformation in the plane of the aerodynamic panels. This description will be updated in the revised manuscript.

12. Page 12, line 7, correct "currrent". Sections 6.2, 6.3, 6.4. In the titles the word "tuning" should be removed since already included in the acronym "IFT". Figures

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21-24: why the words PRE-POST FLUTTER are uppercase?

The corrections will be made as advised by the reviewer, we thank them once again for their constructive feedback.

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