Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-132

Anonymous Referee #1

Referee comment on "Flutter behavior of highly flexible blades for two- and three-bladed wind turbines" by Mayank Chetan et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-132-RC1, 2022

Dear authors, thank you for a nicely written article on a very relevant topic. The paper reads well and I recommend publishing it, pending some key improvements. The biggest feedback is that the article falls short of what I was hoping to find in it. First, the paper is a little blurry on what is novel in terms of theory and models. I am confused whether the models presented in section 2 are any different than the models available in literature and correctly cited in the bibliography. This should be made clearer. In addition, the exploration of the design space is very limited. Why only LE and TE reinforcement thickness? What about outer shell skin thickness, or spar cap placement, or relative placement of LE and TE? Overall the reader is left with many unanswered questions, and this is a pity, since you have all the tools to answer these questions. Finally, I would like to see what it takes to get the designs up to an acceptable flutter margin. It would be good to define this acceptable limit rather than increasing the margin to an arbitrary amount. Then it would be good to see the feasibility of the blade from a design/manufacturing/cost perspective.

Below you find a longer list of comments pointing to specific parts of the text that have margin for improvements:

Title: the title does not seem to be the most accurate: none of the 3-bladed rotors is highly flexible. On the contrary, the 3-bladed designs are actually fairly stiff

Figure 2: This is a nice image, but it is slightly confusing. It reports the WindPACT rotor, which is not listed in 3.1. The IEA10 is instead listed in 3.1, but is not included in Figure 2 nor in Table 1

Table 1: columns on the right miss units

Table 2: wouldn't it be more appropriate to use the digital twin described in https://doi.org/10.1002/we.2636?

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line 168: please quantify "slightly higher"

Line 170: I think it's a little hard to make this claim without evidence, especially since the flap and torsion are coupling in this instability.

Line 172: don't all these references use the same exact formulation? I find this misleading throughout the paper, see points above

Table 3: It might be nice to add the rated rpm for each design

Figure 6 and 7: please add torsional stiffness

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Line 227: solidity of the rotor, not of the turbine

Line 228: "larger margins between the EA and CG locations in the chordwise direction." This is not necessarily true, rather an artifact of some simple scaling steps

Table 5: 13A and 13B have a super low margin. Perhaps these (and the SUMR50) cases would be most suitable for additional analysis in the next section since they have the lowest margin. Also, the flutter margin of SUMR50 is much too low to be a reasonable design. The authors should make some note of this.

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line 262: what about the aerodynamic center as an important design driver?

Line 272: “real designs”, This is confusing. These aren't real designs except for the SUMR-D blade, which is a blade designed with scaling laws, so not at all close to industrial products.

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Line 273: I question the ability of the SUMR-50 blade to meet these requirements given the low flutter margin.

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Section 5.1 would benefit a revision. This study doesn't use any of the (very nice) features listed in the text, while it would be more useful to know how the blade structures and aerodynamics are modeled

Line 298: I wonder if these analyses consider the blades to be rotating

Figure 9: please focus on the aspects related to this study. For example, how have you been computing blade stiffnesses? With PreComp? Or BPE? Or maybe Becas?

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Line 308: I was hoping to understand much more about the solution space. For example, what about the effect of chordwise placement of spar caps and shear webs. And what about chordwise placement of the blade axis? This is only one of many design choices that blade designers need to take. The feeling is that you've not moved the needle on this design exploration to a sufficient amount. Much can and should be done.

Line 309: Related to the point above, outer and inner skins are much more important for the torsional behavior than LE and TE reinforcements.

Line 317: One tricky aspect here is that you're likely using shell elements in modeling of the blade structure. Shell elements accept any thickness, although fin-shape trailing edges cannot physically fit many composite layers

Line 320: point #1 is kind of obvious... because of the airfoil shape, moving away from the LE moves you from the neutral axis faster than at the TE. LE is therefore better suited to increase flap stiffness, although this is probably not very relevant. You don't add material at the LE to increase flap stiffness

Line 322: flapwise frequencies are more (not mostly) sensitive

Line 323: point #4 seems just a natural consequence of points #1 and #3

Line 326: please explain the sentence “for the flutter speed, this occurs at lower TE and higher LE reinforcement”

Figure 11 in the top left corner I’d recommend plotting flutter margin. In the bottom right, it’s unclear why the space is so non linear

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Line 339: I have the feeling that what's truly happening here behind the hood is a change in the positions of EA and COG. I'd find that analysis much more informative than a parametric study on LE and TE thicknesses. It’s hard to draw any general conclusion out of this parametric study

Line 346: please spell out acronym MAC

Line 349: The analysis seemed to show some correlation for "longer" blades, should use that instead of "larger"

Line 350: rotor instead of turbine

Line 352: The difference between points 1 and 2 is subtle and having a single point would improve readability

Line 359: flutter margins are reduced as well, which is likely more important than flutter speeds

Line 366: This was already said in line 355. The whole paragraph seems convoluted and could be shortened focusing on the key conclusions, which I understand as:

1) Flutter margins decrease with blade length

2) 3-bladed rotors show edge instabilities at lower rpm than flap-torsion flutter

3) The opposite holds true for 2-bladed thanks to larger chords generating higher edge stiffness

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Line 380: All these works use the same theory. This seems to me a point of great confusion. The methods for flutter prediction used in this work seem no different than the works cited from literature. It is certainly reassuring to see that the results presented in this work match older results, but the paper should make clear that the theory is not a novel contribution, only the parametric studies and the comparison 3-vs 2-bladed rotors.

Line 382: I don't think your paper supports this conclusion, aside from the findings already available in literature