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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Wind tunnel comparison of four VAWT configurations to test load-limiting concept and CFD validation" *by* Jan Wiśniewski et al.

Anonymous Referee #1

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The paper has a clear abstract with limited objectives enabling systematic investigation concerning VAWT rotor configurations and their influence on the cyclic bending moments seen at the base of the support tower/main shaft. In general nice connections are made in references to relevant previous work.

The swept area of a VAWT is in general rectangular. The configurations shown appear to have a height, H to diameter, D ratio of about 2.5. If the design tip speed and rated power are fixed and we compare with say height to diameter ratio,H/D of 1, the design with H/D= 2.5 has the advantage of lower rated torque but disadvantages of more blade area required and higher base moments. I don't think this impacts too much on your study as I would expect that the results in terms of comparing loads in the K,A,B,C configurations would be similar at other H/D ratios. However this is not proven and it would be good to recognize it as another variable affecting in principle the generality of your conclusions.

There is no mention of spiral bladed VAWTs. The idea that distributing the position of blade elements around the rotor circle will smooth torque and loads is already well appreciated and this should be acknowledged. The spiral bladed VAWT is the ultimate in that respect doing it continuously. Your study is a special case where the distribution is in only two discrete blade sets. The case for your idea could then be that while the spiral blades are structurally efficient at small scale, they would be problematic at large scale.

As a general point on presentation, ahead of section 4, I find too many figures showing the configurations. In the space to the right of Fig 2 for example two vertical schematics of 3 blades for K and 6 for A,B,C would show all the configurations more clearly. Perhaps 4,5,6 could be collapsed into 1 or 2 composite figures. On the other hand, figures with graphical display of the results of Tables 1,2, 3 and 4 would be rather helpful.

The model testing lacks mention of Reynolds number effects until line 209 starting the conclusions. The comment is out of place there . Its not really a conclusion and should be discussed with the experimental results. How low was Re or the range of Re in the model tests?

In line 99 "started oscillating" . What kind of oscillations, bending, torsion?

English in the paper is generally good but from line 114, the word "growth" is not at all wrong but reads rather strangely. Better is "increase in bending moment values". The way it is written "growth" sounds as if the increase is unusual behaviour when, until stall and unsteady effects occur most significantly, we would expect increase in moments (perhaps as square of wind speed). The graphical presentation of Table 2 would definitely help here. The mention of the effects of resonance here is not telling us much with no definition of its nature or suggested explanations.

Finally in your conclusions I think it is pushing it to say more "cost efficient". The results show how bending moments can be reduced and this is certainly useful information for a designer that may assist design optimisation. In a fully engineered system it is unclear how the cross arm structures (sizes and drag impacts) for K will compare with the cross arm structures required for blades in a sense cut into two , A, B,C.

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