Comments on the revised manuscript “An improved second order dynamic stall model for wind turbine airfoils”
by G. Bangga, T. Lutz and M. Arnold

Khiem V. Truong (Reviewer 1)
helires@gmail.com

1. General comments:

Following my first review, the authors Drs. Galih Bangga, Thorsten Lutz and Matthias Arnold have taken into account some of my critics and have made some improvements in their model predictions. However, the main critics are still unanswered.

I will follow the same steps as in the first review.

2. Technical comments:

2.1. Analysis of the various stall models:

The authors spent a great length of time in analysis of the existing stall models that does not present a great interest for the manuscript objective.

They state in their introduction (lines 45 - 46):

“The main purpose of this paper is to document widely used state-of-the-art dynamic stall models in research and industries”.

I would disagree with such assertion of the above sentence:

(1) Is it “one” of the purposes of the paper and not the “main” purpose?

(2) the claim about the presentation of the state-of-art dynamic models in research and industries is exagerated:

- The BL model presented in the manuscript corresponds only to the second generation model. There is a third generation model that is in use in UK universities and industries, see Reference “Advancement of aerofoil section dynamic stall synthesis methods for rotor design” by Sheng et al., The Aeronautical Journal 2012, v. 116 no 1179, pp. 521 – 539. Furthermore, it is not presented under the state-space formulation, different from the formulation of the other models.

- The ONERA model presented corresponds to the ONERA- Edlin model, the first generation model at ONERA.

(3) The application of the dynamic models is incomplete.

For the BL model, it seems that there is no contribution calculated for the vortex component of the aerodynamic coefficients: see Figure 8. For the ONERA - Edlin model, the authors state that “the original ONERA model cannot predict the drag and moment coefficients” (see Figure 9). An experienced user of the ONERA – Edlin should be able to calculate these coefficients.

2.2. Values of constants used in the IAG model:

- About the value of the critical stall angle α_{CRIT} of airfoils:

To avoid any confusion about the values indicated in Table 5, it is preferable that the authors state that the values of the critical stall angle are related to airfoils with leading edge grit roughness and Reynolds number of 750k in paragraph “2.6” at the position of line 369, the lines 375-381 in the paragraph “3 Results and Discussion” to be moved to there.

- About the values of kS:

It is misleading to call kS as “Strouhal frequency”, while it does not have its value (around “0.1”). I suggest to simply call it a constant with value equal to “0.2”.

2.3. Sensitivity of the results against applied time step of the solver:

There is no interest in studying the effects of sensitiveness of the integration of models against the step size. The choice of a ODE solver with automatic step variation would do the job better than a ODE solver with fixed time step. It would allow the choice of output at time values requested by the structural code for coupling fluid-structure.

2.4. Quality of the IAG model:

The authors claim the superiority of their model over the others, but their model errors are not quantified. Following my first review, the authors have included the calculation of the L2-norm error, but only for their own model. I would expect such calculation for the Snel, Adema and IAG models in the cases analyzed in Figures 10, 11 and 12. Regarding these cases, I suggest that the values of pitch angles to be used in computation are to be obtained from a fit of experimental values and not from experimental values provided by experimenters. This could lead to a lower L2-norm error and a better graphical visualization of the correlation between experiments and predictions.

3. Technical corrections:

The various points raised in the first review have a satisfactory answer, except for the effects of increasing frequency k.

Line 549: “Increasing k above 0.1 reduces the viscous effects and vortex shedding influence”.

It is well known that the increase of k leads to a more important variation of the pitching moment (that is not shown in your simulation in Figure 21) and it translates into more structural fatigue.

Additional remarks:

* Figure 21: Effects of k:

I would suggest studies of k = 0.03, 0.05 and 0.10 instead of high k (0.015 and 0.21): there are few experiments at such high values and the model seems unable to capture effects at high k.

* Line 540: assertion without proof for the BL model.

* Line 542-543: “Despite that, only the Adema model and the present IAG model are able to demonstrate the higher harmonic effects”.

The ONERA – Hopf bifurcation does it too!

* Line 546: typo “comparsion”.

* Line 548: “without changing the constants”

Be more precise: by changing only the values of αCRIT.

* For references: delete line 623, the reference in line 622 is the same.

4. Concluding remarks and suggestions for revision:

There are some improvements for the model predictions, particularly for the drag coefficients. However, the main critics from the first review are still unanswered satisfactorily.

I suggest that the authors leave out the BL and ONERA – Edlin models, unless if the authors are willing to spend more effort for studying these models. It appears better to center the effort on the second order model, as stated in the abstract.