Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-75-RC1, 2020

Interactive comment on "An improved second order dynamic stall model for wind turbine airfoils" *by* Galih Bangga et al.

Khiem V. Truong (Referee)

helirestrg@gmail.com

Received and published: 19 May 2020

1. General comments:

The journal Wind Energy Science (WES) has recently accepted the publication of the paper "Development of a second order dynamic stall model" by N. Adema, M. Kloosterman and G. Schepers. This paper is about the improvement of Snel's model on vortex-shedding phenomena. This work answers to the current concern of industry for the design of Horizontal-axis wind turbines, due to the vibratory behavior of rotor blades in parked or idling conditions. The submitted paper for the same journal by Bangga et al. has for subject the same topic, the improvement of the second order equation of Snel's model. As it is submitted after the paper of Adema et al., it is confronted to the challenge of providing significant results. Indeed, it embraces topics not treated in the recent paper quoted above, analysis of all the three aerodynamic coefficients (lift, drag and pitching moment) for four different airfoils in various flow conditions. The manuscript is well organized and clearly written. However, as the authors take on a new study field, they have not made a thorough analysis of the existing literature, and this fact leads to multiple errors in the submitted paper. Despite of these shortcomings, I still recommend its publication but with a major revision that takes into account the following critics.

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. In doing so, the authors have made various mistakes. The Beddoes-Leishman model is not presented under the state-space formulation. Therefore, the sensitivity study of this model against step size of integration cannot be made, as stated in line 393. About the ONERA model, they are not aware that it was renamed ONERA-EDLIN ("Equations Différentielles Linéaires", meaning in English Linear Differential Equations), to distinguish it for the newer model ONERA-BH ("Bifurcation de Hopf", renamed later by his author as ONERA Hopf Bifurcation model). It is usual for researchers in the field of wind turbines to continue to call it with such name; so, this mistake is not serious. The critical error of the authors is to not consider the stall delay in the ONERA-EDLIN model. Without the account of stall delay, this model leads to predictions of the lift coefficient with large discrepancies in correlation with experiments as shown in Figure 9 p.20.

2.2. Values of constants used in the IAG model:

There are two types of constants used for the IAG model that are ill chosen, the critical stall angle and the value of the Strouhal number.

*The critical stall angle αCRIT of airfoils is one key parameter for the stall model. The authors choose this value based on the position of the break of the pitching moment coefficient and the position of the important increase of the drag coefficient. This is not a good choice, as pointed by Sheng et al. in their conclusions (Reference cited on line 598), the best choice is the incidence angle at the maximum chord force coefficient. Led by such bad criteria of defining αCRIT , the authors found very small values for the airfoils S801, S809 and S814 : 15.1o, 14.1o and 10o respectively, instead of the values of 17.6o , 19.2o and 13.9o found by Sheng et al. (Reference Sheng W., Galbraith R.A.McD. and Coton F.N., “Applications of low-speed dynamic-stall model to the NREL airfoils”, Journal of Solar Energy Engineering, 2010, vol. 132, pp. 011006-1:011006-8). The increase of the value of αCRIT would allow a better correlation of their model predictions with experiments, as shown in Figures 10-12, and following.

* Value of the Strouhal number S: the authors following Adema et al. use the value of S = 0.2; they should notice from various references that S is in the range of [0.06,0.13] (see for example “Spectral analysis of New MEXICO standstill measurements to investigate vortex shedding in deep stall” by Khan M.A., Ferreira C.S., Schepers G.J. and SØrensen N.N., Wind Energy, 2019, pp.1-14). When S decreases, the predicted distance between two consecutive extremum (maximum for the lift and drag coefficients, minimum for the pitching moment coefficient) of the aerodynamic coefficients increases. The correlation between model predictions and experiments would be improved.

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

The authors use a rudimentary numerical tool for solving the ordinary differential equations (ODE) with fixed time step, there exist more robust ODE solver with automatic step variation. Therefore, the discussion related to the time step size is irrelevant (section 3.1 and conclusion).

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. Since the study of Holierhoek et al. (cited in line 574), practically all the publications on hysteresis loops in stalled conditions of airfoils provide the values of the error L2-norm, see for instance the publication of Adema et al. I would consider that the predictions for the lift coefficient are reasonable. However, the predictions of the drag coefficient are overestimated and this would lead to under-prediction of the power coefficient CQ. The predictions of the pitching moment coefficient are not right in some cases. For instance for the airfoil S801 in Figure 17, the predictions show clockwise hysteresis sub-loop that correspond to negative aerodynamic damping, while the experiments show anti-clockwise sub-loop leading to positive aerodynamic damping.

2.5. The study of various airfoils:

It would be interesting that the model predictions could show some distinctive features associated with the thickness for the airfoils studied, ranging from thin (S801) to thick airfoil (S814). Thin airfoils are characterized by leading-edge stall, whereas thick airfoils by trailing-edge stall. The choice of the airfoil S801 by the authors of the submitted paper for extensive studies is unfortunate, because it is a thin airfoil of thickness 13.5%, and wind turbine blades have usually thickness larger than 15%.

3. Technical corrections:

Line 4: the sentence “many flow parameters” is not clear.

Line 538: “Increasing k above 0.1 leads to an increased flow stability”: this is incorrect.

Lines 539-540: the assertion is incorrect. For a large angle variation, the variation of the pitching moment coefficient is more important (see Figure 24) and this could lead to more structural damage to the blades.

Imprecision for the section References: - Lines 555, 558, 566, 570, 577, 585, 598 and 604. - Inconsistencies for Lines 568 and 574

4. Concluding remarks and suggestions for revision:

Though the submitted paper is marred with errors, there are two positive aspects. The first one is about the objective of examining Snel's model for various flow conditions and airfoils. This stall model has been around 1997 and no exhaustive evaluation has been made at my knowledge. I feel that Dr. G. Bangga and his co-authors are capable of doing it. The second is about the success of the prediction of the center of pressure (Section 3.8). Despite the imprecision on the predictions of CL and CM , it appears that the ratio XP (= - CM/CL) is well predicted, as though the errors on CL and CM are canceling in the ratio. The improvements proposed so far in the manuscript are not significant to my opinion. I would suggest that the authors look at the model implementation of the stall and flow reattachment delays. For the comparison of the model with experiments based on the first order correction, it would be clearer if the cases of non – stalled conditions are considered, there are no effects of second order for these cases. For the second order model, the main correction to Snel's model proposed by the authors (and Adema et al.) has been to replace the damped oscillator when dα/dt < 0 for a self-excited oscillator of Van-der-Pol type with more damping. The objective has been to capture the oscillatory behavior on the return cycle of the aerodynamic coefficients. However, in Truong's model (see Reference “Modeling aerodynamics for comprehensive analysis of helicopter rotors” by K.V. Truong,42nd European Rotorcraft Forum, Lille, France, September 5-9, 2016 and also published in Aerospace 2017, vol.4, 21), the self-excited oscillator is only replaced by the damped oscillator, when the flow is reattached on the return cycle, i.e. with some lapse of time after the change of sign of dα/dt. Under such circumstances, the oscillatory behavior still subsists in the return cycle, albeit with smaller amplitude. This point has been raised also by Dr. X. Munduate while reviewing the article of Adema et al., but these authors have not provided any answer. If the revised model is capable of taking into account the stall delay and the flow reattachment, the authors could solve this issue and improve other predictions, particularly the dependence on the reduced frequency.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2020-75, 2020.

Printer-friendly version

Discussion paper