Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-36-RC1, 2019

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DISCUSSIONS

Interactive comment on “Cartographing dynamic stall with machine learning” *by* Matthew Lennie et al.

Anonymous Referee #1

Received and published: 4 September 2019

Dear authors,

I think the article is quite interesting and it seems that the machine learning techniques can indeed be used to analyse dynamic stall phenomena. In my opinion, the article should be shortened by removing some Figures and potentially also removing the towing tank experiments that are almost not used in the article except for showing an outlier (if I understand correctly). I find it very hard to judge how relevant the work presented in this article actually is. The authors suggest that the presented machine learning work flow can be used to analyse CFD results or experimental input. The results from this analysis could then be used to further train a model that is based on a massive amount of cheap data from empirical models. This might be very difficult due to the following reasons:

- The cheap models do not contain any unsteadyness for steady state inflow at constant angle of attack. This unsteadyness is exactly what would be needed to for example use a dynamic stall model for vortex induced vibration in deep stall.
- Since this unsteadyness is not included in the empirical models, all the necessary information needs to come from CFD or measurements.
- Vortex induced vibrations are a 3D phenomenon and depend on the blade shape and the aeroelastic mode shapes. So 2D computations or measurements will probably not be sufficient and the models would have to be trained on full blade CFD or measurements.
- This doesn’t seem to be feasible due to the very high cost of training data.
- The dependency on inflow turbulence and stall delay/rotational augmentation increase the amount of necessary training simulations / measurements also for normal operation.

So I am not sure how realistic the training procedure outlined in the conclusions actually is.

More specific comments follow below.

Figures:

- Sometimes the Figures are placed far from where they are referenced.
- Figure 4 contains Figures 7 and 8 and thus Figures 7 and 8 should be removed from the article.
- Does there need to be an additional Figure 9 with an example of the clustering when there are plenty of figures with clusters later on in the article?

- Are Figures 10 and 11 necessary for the article?
- A color map for the pressures in Figure 12 is missing.
- I can't find a reference to Figure 13 in the text. Is it necessary?
- Plotting Figures 14 to 16 each on 'regular' line plots with one y-axis (instead of 4 y-axes) would probably be more clear.
- Some Figures, for example Figure 18, have very large font sizes. Please adjust.
- The two clusters in Figure 22 look virtually identical. This should at least be commented on in the text.
- You could consider plotting the clusters in a single plot so that the differences are easy to see, for example in Figure 21.

Other points:

- Please change the first sentence of the abstract: 'Airfoil stall is bad for wind turbines.'. Without airfoil stall limiting the lift coefficients, the extreme loads of the turbines would be much higher, so this sentence is simply not correct.
- It seems that the towing tank profile is only instrumented on one side, see Figure 2, and thus dynamic lift coefficients can't be computed from pressure integration of the surface. I can see that the towing tank experiment is used for the outlier detection in Figures 23 and 24. But maybe such an outlier detection could be performed on wind tunnel experiments instead and then the towing tank experiments could be removed from the article. At least for me it is difficult to see the value of these experiments in the context of dynamic stall.

- You write on page 3: 'In short, this phase difference can lead to single degree of freedom pitch flutter also known as stall flutter.' To my knowledge, the term 'stall flutter' is typically used to either describe instability due to vortex shedding (see for example 'a modern course in aeroelasticity') or so-called stall induced vibrations (vibrations with negative damping due to negative lift gradient (see for example Hansen 'Aeroelastic instability problems for wind turbines'). Both of these phenomena do not occur due to dynamic stall phase lags. For stall induced vibrations, dynamic stall can even reduce or reverse the effective lift gradient and thus stabilize an otherwise unstable operating point.
- Also on page 3: 'If the unstable nature of separated flows leads to the extent and phase of light stall to be variable between cycles of pitching, then it follows that the aeroelastic stability of the airfoil will also be variable between cycles.' Typically stability refers to the stability of an operating point. Thus if on average many cycles remove energy from vibrations the operating point is stable, if they add energy then it is unstable. Stability is not determined from cycle to cycle. Otherwise operation in turbulent inflow would switch between stable and unstable all the time.
- On page 8 you write '1-2 workers (nodes)'. How many cores per node?
- A large part of Section 4.1 is actually a description of the method, not a presentation and discussion of results. As such it should be part of section 3.
- On page 15 you write: 'The first case is relatively complicated, as it features, an oscillating inflow velocity, pitching into the dynamic stall range and leading edge blowing.' I think it would be a good idea to start with a much less complex case.
- On page 18 you write: 'Interestingly the higher reduced frequency in Figure 22 seems to suppress the secondary vortex with only 23.5% of the cycles having a secondary vortex where as Figure 22 shows strong secondary vorticity 51.8% of

the cycles.' It can't be Figure 22 you are referring to twice and I can't find the numbers 23.5% and 51.8% in the Figures.

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