Response to the Review on the Paper wes-2022-13

A WaveNet-Based Fully Stochastic Dynamic Stall Model

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We thank the reviewer for his detailed evaluation of our manuscript. Below we respond to his comments and explain the changes resulting from his comments. Please note that in the revised version of the manuscript, all changes related to reviewer #1's comments are highlighted in red if uploading a revised version is possible.

Comment 0: The paper aims to provide an alternative for dynamic stall prediction to classical (semi-) empirical methods. The proposed method was constructed based on data driven approaches, adopting the DeepMind's WaveNet architecture. Overall, the paper was written well and can be followed easily. The model also produces good results with sound discussion. I enjoyed reading the whole content of the paper. Despite that, I found several issues with the paper which I would hope could be considered in the revised version of the paper.

Thank you for evaluating our work, we appreciate the effort made and your detailed insights and suggestions

Comment 1: Although this is minor, the usage of English needs to be checked appropriately. I found some grammatical mistakes, especially on the usage of mixed tenses.

The authors will correct these grammatical mistakes in the revised manuscript.

Comment 2: For a paper, the words "Chapter" does not feels right, please use "Section" instead.

Indeed, we changed the word in the revised manuscript.

Comment 3: Motivation to adopt a data-driven technique for dynamic stall modelling is lacking in Introduction.

We have added a paragraph highlighting the motivation of the data-driven models compared to the physically based models (l. 60ff).

Comment 4: Another type of simple data driven technique for optimization (such as standard gradient method, GA, etc) has been proven powerful and is practical enough to use in industry. Our group has demonstrated in (Herrmann and Bangga, J. Renew. Sustain. Ener. 2019) that this is practical enough for wind turbine design. How can we justify the real potential implementation for this approach?

The mentioned article describes an optimization of a wing profile with various optimization methods. In our case optimization only happens during the gradient descent optimization of the weights and biases of the neural network. Strictly speaking, however, the whole problem is a complex regression analysis and GA etc. is not really suitable for that. So, the rationale is that only a few methods can deliver our generative properties in the first place.

Comment 5: Please clearly mention the novelty of the paper.

In the introduction, we have added a paragraph that sums up the novelty of the paper and lists all the advantages.

Comment 6: How does the proposed model perform compared to a more established time series prediction models like Bi-LSTM? Or a combination of CNN-Bi-LSTM?

Based on current trends in the machine learning community, LSTMs have not received further attention. They require more memory and are considered difficult to train. It has been shown that the autoregressive Wavenet or (more recently, after writing this paper) Transformer are even better for time series prediction[1]. Due to our limited hardware, the decision to use Wavenet was quickly made. Nevertheless, we cannot definitively assess the performance of an LSTM.

Comment 7: What is the size of the time series width for the selection of the window sliding method? We demonstrated in our soon to be published paper that the size of the window width plays a decisive role in the accuracy for a time series prediction. Have you made an initial study?

Yes, the size of the sliding window was part of a grid search for the best score. The vector encompassed [16, 32, 64 ,128, 256] time steps. Ultimately a receptive field of 128 steps into the past gave the best score.

Comment 8: The Reynolds number is fairly low for wind turbine applications. Can the model be scaled to a higher Reynolds number case?

Yes, the experiments were performed at relatively low Reynolds numbers. However, there is nothing to prevent feeding the model with further data from experiments performed at higher Reynolds numbers. If such experiments are not available, it might be possible to modify a subset of the raw data by other methods to roughly correspond to higher Reynolds numbers.

Comment 9: Figure 4 is not useful, please use log scale for the y axis. The magnitude of the oscillation amplitude also does not look right, a lift coefficient amplitude as large as 40 does not feel like a right value to me.

Indeed, we will use the log scale for the revised manuscript. I did not perform the experiments, but such large magnitudes can be seen in many testcases.

Comment 10: What is the impact of the experimental data downsampling? How if all the high frequency data is included? Will it crash due to instability? Loss in accuracy? Please justify.

Including the higher frequency components in the model would work and the model is not expected to crash. However, since above a certain threshold the main components seem to be noise, we did not expect any added value.

Comment 11: “Therefore, the model can only work with a constant time step of 0.01 s” I see this as a drawback. What if the user would like to choose a larger or a smaller timestep?

It would mean a slight modification of the model and a new training, but in principle it is no problem to introduce the time step as a global variable, as described in Section 7. The high sampling rate theoretically leaves a lot of room to represent significantly smaller and larger time steps in the training data. For our paper, however, this possibility was not particularly important, since it is trivial and does not add any value because the model is not intended to be used directly in practice. It would, however, imply a higher training effort.

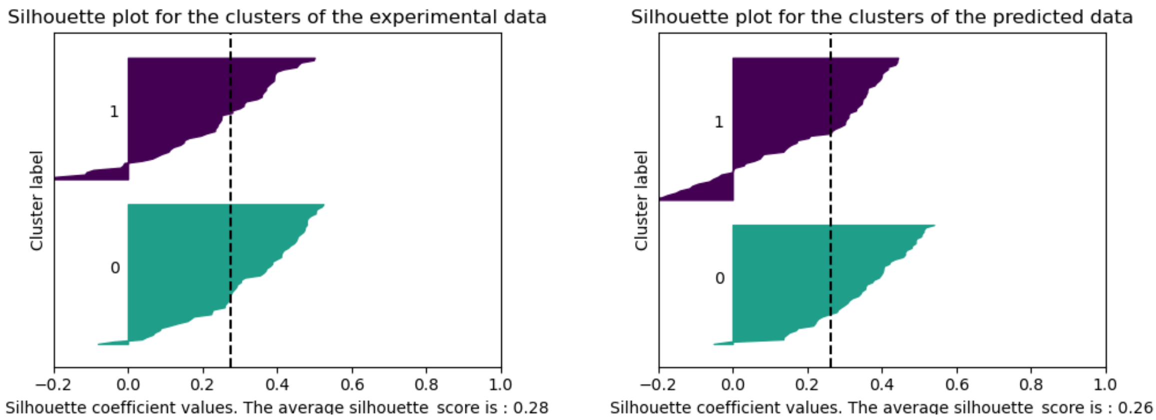
Comment 12: Please check the FFT for Fig 11 (see above comment)

Fixed it in the revised manuscript as well.

Comment 13: As the author mentioned, the prediction is slow compared to semi empirical models, will it hinder implementation in a real wind turbine simulation tool?

While stepping forward in time is relatively “slow”, the prediction can be done in parallel on large amounts of airfoil sections or even multiple turbines at once. Which should reduce the gap to the semi-empirical models considerably. The performance of the forward step can be improved by using a more powerful GPU than the consumer card from 2016 used here.

Comment 14: When you do the clustering using hierarchical clustering, is it possible to show the silhouette plot?



Here you go. However, I would prefer not to include them, as they lead to more clutter and even more diagrams. Mainly because I don't feel it adds much value next to the existing dendogram. If you feel it is still worthwhile, I have no problem including them in the final manuscript.

Comment 15: Any tests for different airfoils? Are the weights obtained here still valid?

Since we only had access to a sufficiently large data set for the S809 airfoil, the weights are unfortunately only suitable for this one. However, as we noted in Section 7, if more data were available, it would not be a problem to extend the model with airfoil-geometry related global parameters.

Comment 16: Last, but the most important comment, how could we adopt the model in a real wind turbine simulation tool (like Bladed, FAST, HAWC2)?

Implementing a Tensorflow model should not be too difficult. The existing programs would only need an extra interface to Python if necessary.

Then, at each time step, the recorded motion data of all airfoil sections to be simulated could be passed to the prediction function simultaneously. The turbine simulation tool then simply receives the corresponding aerodynamic coefficients and can continue to work as usual.

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[1]https://bair.berkeley.edu/blog/2018/08/06/recurrent/