

Interactive comment on “High frequent SCADA-based thrust load modeling of wind turbines” by Nymfa Noppe et al.

Nymfa Noppe et al.

nymfa.noppe@avrg.be

Received and published: 22 December 2017

Thank you for your nice and interesting comments. We agreed to most of them and already discussed them during the project. Following you can find our answers and how we suggest to adjust the manuscript.

Both reviewers suggested to revise the title of the manuscript. We agree with them and therefore the title will be modified to “Modelling of quasi-static thrust load of wind turbines based on 1 second SCADA data”.

From several comments of both reviewers, we realized the motivation behind the thrust

C1

load estimation and the use of 1s SCADA data is not obvious. As 1s SCADA data was accessible for this research and is becoming the standard in industry, the main goal was to explore its possibilities to estimate fatigue loads. Given the sampling frequency of 1 Hz, only low frequent loads (lower than 0,5 Hz) can be estimated. In addition it was soon clear the first order tower dynamics were not present in the SCADA signal. Therefore the load spectrum that can be estimated based on 1s SCADA data is limited to the quasi-static thrust load.

Previous research within our group showed fatigue loading can be estimated using a combination of strain gauges and accelerometers. For several reasons accelerometers are preferred over strain gauges, but they are not suited for quasi-static loads. The strain gauges are thus crucial to capture the quasi-static part of the loading. The research presented in this manuscript aims to replace the strains gauges by the SCADA-based approach. In future research the proposed thrust load estimation can be combined with the use of accelerometers to estimate the total fatigue loading. This motivation will be explained better in the introduction.

Both reviewers asked for clarification regarding the time delays mentioned in the paper. During this research, an autocorrelation was performed between a thrust signal and several SCADA signals for multiple periods. Results showed the observed time shift changed for different signals but also for different periods. The first attached figure (CorrelationCoefficient_vs_timeshift) shows the correlation coefficient between a thrust signal and 4 SCADA signals of 2,5 months, where a time lag of -15 to 15 seconds was considered between the signals. In general, the differences in correlation coefficient are fairly low. We decided the added value of including this figure to the manuscript was small, but based on the comments following sentence will be added to page 5, line 20: *When calculating the autocorrelation between the thrust load signal and shifted SCADA signals, the biggest time shift was found for the pitch signal and corresponded to -3 seconds.*

Since the maxima never exceed 5 seconds, it was chosen to include the 5 previous

C2

timestamps for every selected SCADA parameter as an input to the neural network. Moreover a small sensitivity analysis (trial and error) revealed that including less previous timestamps in the input set of the neural network made the obtained results worse, there was also no gain beyond 5 seconds.

Both reviewers pointed out that Pearson correlation only applies for linear correlations. We fully agree and calculated mutual information too during this research to consider non-linear correlations. Results were comparable to those obtained with Pearson correlation. Since results were comparable and for sake of simplicity it was decided at the time to only include Pearson correlation in the manuscript. Since this rightfully raises questions, the results for mutual information will be included as well in the revised version of the manuscript. The most important observation in these results is that all selected parameters are clearly correlated to the thrust load based on the complete dataset, whereas the Pearson correlation of the pitch angle to the thrust load was low when all data was considered.

Some comments given by both reviewers concerned the topology of the neural network. This topology was chosen by the authors in the beginning of the project. Three hidden layers were chosen because three operational states can be distinguished for a wind turbine ('non-operating', 'operating below rated power' and 'operating at rated power'). Moreover 4 neurons were chosen in each layer because 4 input parameters were selected. The second attached figure (MeanRelativeError_vs_different_topologies) shows the possible gain in mean relative error of the test set by using a different topology. In this case only topologies are considered with the same number of neurons in each layer. These results show the error is not influenced a lot by the topology, as long as more than one neuron is used. The topology was not optimized afterwards because results were already satisfying. Following sentence will be added to the contribution: *By choosing a different topology the mean relative error*

C3

of the test set improved with maximum 0,2% if more than one neuron is chosen in each layer.

All the other needed settings for the neural network are kept as suggested by the Neural Network Toolbox of MATLAB.

Based on several comments, we understand that the motivation behind the use of simulations is not fully clarified in the manuscript. During the project we decided to use simulated data, which provided a controlled and reproduceable environment, in order to understand the real measurements better. However the simulated turbine (and site) is not the same turbine, although comparable, as the real one. Therefore the simulated thrust load cannot be compared to the measured thrust load. We included the results based on simulated data to show the method works for different types of turbines and to provide a reproduceable environment to optimize the technique in the future. If desired we can make the simulation data publicly available. Several adjustments will be made to the manuscript. The abstract will be modified to motivate the two-step approach of both simulation and real-world measurements. Moreover section 5 and section 6 will be merged to one section "Results". This section will start with following text:

The proposed approach is validated using two different datasets. The first one is obtained by simulation in FAST, while the second one is obtained thanks to a measurement campaign performed at an offshore wind turbine. The dataset obtained by simulations was included to illustrate the approach in a controlled and reproduceable environment.

Several comments concerned the order of the figures addressed in the text of Sections 5 and 6. We agree this could be confusing. The order of the figures was chosen to be able to easily compare training and validation phase. However the order of explanation in the text was chosen to make the distinction between the training and validation

C4

phase. Since this confuses the reader, the order of explanation will be adjusted. First the procedure will be explained, being a training phase and a validation phase. Afterwards the resulting relative errors during both phases will be discussed together. Moreover, in case of the simulated data, the wording "long term validation data" was chosen to correspond to the measurements as the complete set was used to validate. However the wording will be adjusted. To end, some timeseries will be discussed to illustrate the obtained relative errors.

Smaller comments are addressed in the following.

Page 1, line 23ff: *"It sounds as if the thrust load would not show oscillations with the multiple of the rotor frequency (1P, 3P, 6P. . .) or with natural frequencies of the support structure. However, Figure 1a shows that these frequencies are clearly visible in the spectra of the thrust load. The authors should explain this in more detail."*

Figure 1a shows the spectrum of the measured bending moment, this indeed contains the contribution of the rotor harmonics. However the primary target of this paper are the quasi-static loads up 0.2Hz (Filtered). For these specific loads the rotor harmonics do not contribute. To avoid confusion, the legend of Figure 1a will be adjusted. Moreover the sentence "the targeted quasi static load (filtered) does no longer contain the effects of rotor harmonics" will be added to the text and the caption.

Page 2, line1: *"Typically there is a correlation between wind speed and wave height or wave period for example. Hence it is not true that wave induced loading is unrelated to any SCADA signal, e.g. the measured wind speed. Please clarify."*

We agree a correlation between wind speed and wave height or wave period exists. However this correlation only holds for e.g. 10 minute statistics. However, in timeframes of a couple of seconds, wind speed and wave height or period are not be correlated one on one, e.g. a gust does not imply a higher inbound wave. The

C5

sentence will be rephrased in the paper to clarify this subtility.

Page 2, line 16: *"In Figure 1a it is shown that the 1P rotor frequency is at 0.2Hz. It is unclear if this is the 1P at rated speed or minimum speed, for example. If it is assumed that this is the 1P at rated speed, then the 1P would be lower for partial load operation, e.g. 0.1 Hz at minimum speed. In this case, the applied filter would not remove the 1P frequency content from the thrust signal. The authors should elaborate on this and explain their approach for selecting the filter frequency more clearly."*

In general, turbines are well balanced and a 1p rotor harmonic does not exist. The filter frequency was chosen in such a way the filtered signal was not influenced by the first resonance frequency, since this is unrelated to any SCADA signal anyway. However this filter frequency can be adjusted once the approach is combined with accelerometers. In that case the lower frequency bound of the accelerometers (0,06Hz) can be set as the filter frequency for the SCADA-based thrust load estimation. We considered this path outside the scope of this contribution. The motivation behind the filter frequency will be clarified in the paper.

Page 2, line17: *"It is more common to write 'first natural frequency' instead of 'resonance frequency of the first order'. Please consider changing the wording."*

True, the wording will be adjusted.

Page 2, line 21: *"It is unclear how the down sampling has been performed. Are signals averaged over 1 second and 10 minutes or are data points simply removed from the signal to achieve the desired resolution?"*

The sentence will be adjusted to: the obtained thrust load is down-sampled using an antialiasing filter to a time frame of 1 second and additionally averaged over 10 minutes.

C6

Page 9, Chapter 6: *“Is it possible to show the probability distribution of wind speeds in the measured data? And have the authors investigated, if the relative error is somehow related to the probability of wind speed? The distribution of the relative error looks similar to a flipped Weibull distribution. Possibly the network was able to learn the relation between thrust load and SCADA data for those wind conditions that were overrepresented in the raining data and the relation at very low and very high wind speeds was not learned that well. Have the authors tried to use training data featuring different wind speed distributions and compared the graphs of the relative error?”*

This is an interesting comment. We decided not to change the wind speed distribution of the training set since we considered the common wind speeds more important. However, during the project we trained a neural network by using high windspeed data only. This yielded similar results as shown in the contribution. Moreover in case of the simulated data, the wind speed was uniformly distributed and we observed the same distribution of relative error.

Page 9, line 10-11: *“The explanation for the errors at low and high wind speeds is unclear. What are the “offsets in the results” and what is meant by the “variability in the tail of the thrust curve”? Could the authors please explain this in more detail?”*

We called the mean difference between measured or simulated and modelled values the offsets in the results. Moreover for windspeeds higher than ca 15 m/s, the difference between the 5th and 95th percentile of modelled thrust is lower than the difference between the 5th and 95th percentile of measured thrust. The wording will be adjusted in the contribution.

Page 10, line 6-8: *“What is meant by “the present scatter will partially hide the correlation”? Do the authors want to state that the correlation coefficient between measured and modelled thrust load is smaller if only one independent variable is used compared to multiple independent variables?”*

C7

Yes, however the variables we used are not independent. In this case the thrust load depends on multiple parameters. These parameters show dependencies among them as well. When looking at only one parameter, the difference in value for thrust measurements occurring for the same value of that parameter cannot be explained. When considering more parameters, this difference might already be explained by a different value of another parameter. Therefore the resulting correlation between the measured thrust load and only one parameter is lower than between the measured thrust load and a combination of multiple parameters, as done by the neural network.

Thank you very much for your nice comments and helpful review!

Interactive comment on Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2017-46>, 2017.

C8

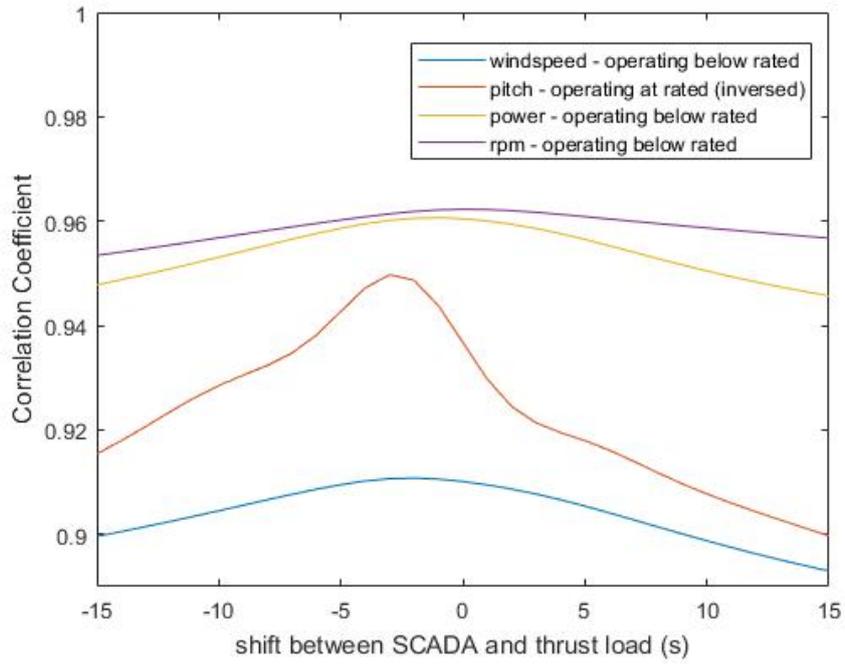


Fig. 1. CorrelationCoefficient_vs_timestep

C9

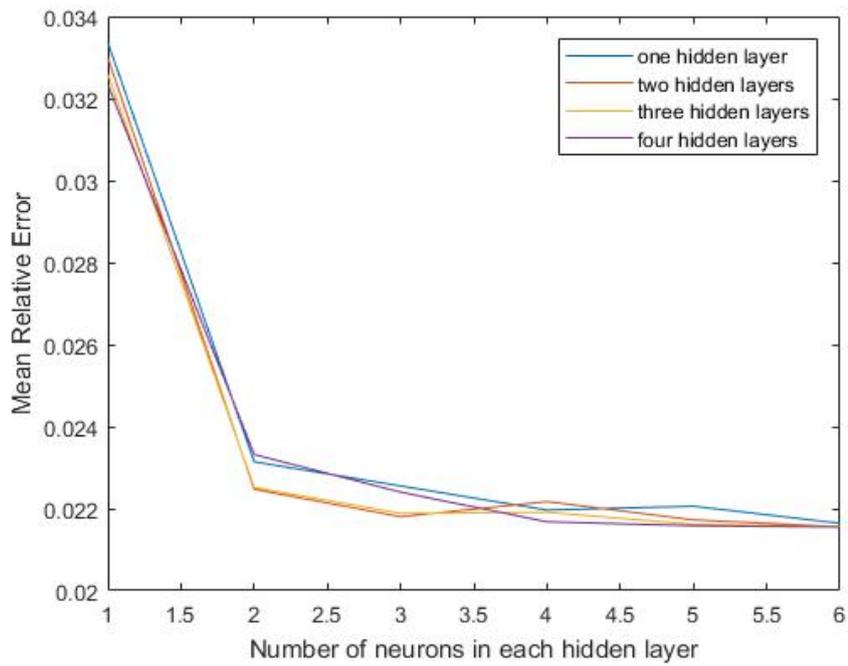


Fig. 2. MeanRelativeError_vs_different_topologies

C10