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

Anonymous Referee #2

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General comments: The authors are investigating if the quasi-static rotor thrust can be estimated from SCADA signals of a wind turbine. They present interesting studies based on simulated and measured data which are worth to be published. The paper itself is written in appropriate style and form. Nevertheless, there are several issues and questions which should be addressed in more detail before publication.

As a general comment, the authors state correctly that the assessment of fatigue load history is important for the estimation of the remaining useful lifetime of existing wind farms and also to gather knowledge for the optimization of future wind farms. They also mention that fatigue is driven by cyclic loading mostly (page 10, line 2). At the same time the authors are presenting a method for the estimation of a quasi-static thrust load, where most of the load cycles have been removed. This inevitably leaves the reader

with the question why the authors have chosen to estimate the quasi-static thrust load if it is not that relevant for fatigue. The authors should clarify this contradiction and explain in more detail their motivation for the estimation of the quasi-static thrust load.

Moore detailed comments are as follows:

Page 1, Title: The authors may want to revise the title of their paper. I am not a native English speaker, but shouldn't it read 'High frequency...' instead of 'High frequent...'? The authors should further discuss, if 'high frequency'/'high frequent' is not misleading for this paper, as they are modelling the quasi-static component of the thrust only and have removed all 'high frequency' content from the signal. Maybe 'Modelling of quasi-static thrust of wind turbines based on SCADA data' is more accurate?

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.

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.

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.

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.

Page 2, line 21: It is unclear how the downsampling 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?

Page 4, line 4ff: The Pearson correlation coefficient is a measure for the linear correlation of two signals. If the relation between two signals is non-linear the correlation coefficient may be small. In that respect, a small correlation coefficient could simply indicate that there is no linear relation between the independent and the dependent variable, but it does not mean that there is no relation at all. Hence, even if the Pearson correlation coefficient between the thrust and a SCADA signal is small, the signal still may be valuable input to the neural network. Cuold the authors please explain in more detail why they have chosen to use the Pearson correlation coefficient for the identification of suitable input signals for their estimation of thrust loading with neural networks? Furthermore, on page 3, line 9-11 the authors mention correctly that there may be time delays between the thrust load and the SCADA signals. Can these time delays also result in reduced correlation coefficients for the 1s operational data and if so, how have the authors dealt with this in their investigation?

Page 6, line 6: Similar comment as above: The authors state that the relation between thrust load and SCADA signals is non-linear. At the same time they use a linear correlation coefficient to test is a relation exists. The authors should clarify this contradiction.

Page 6, line 9: Why have the authors chosen this network topology?

Page 7, line 11-18: Figure 4b is not explained in the text. The authors may consider adding a sentence here.

Page 8, Caption of Figure 4: It is unclear why the validation set is denoted as “long-term”. Has that data been recorded at a different period of time, e.g. some month later than the training data?

Page 8, line 1-5: It is not clear why Figure 4b is explained here. In addition, Figure 4c has been explained on page 7 already. It may not be necessary to repeat the

explanation here again.

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 over-represented 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?

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?

Page 9, line 16ff: It is unclear why the authors describe the content of Figure 6 again and also after the content of Figure 7 was explained. The content of Figure 6 was discussed at the beginning of the page before and any additional information in this section may also be moved to the beginning of the page. 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?

Page 10, line 18: It is unclear what the authors mean by “default settings of the neural network”. Have they chosen a default network topology for this study? Please explain in more detail.

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