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WIND ENERGY SCIENCE DISCUSSIONS

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Interactive comment

# *Interactive comment on* "A Model to Calculate Fatigue Damage Caused by Partial Waking during Wind Farm Optimization" *by* Andrew P. J. Stanley et al.

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# Response to Reviewer 2

Andrew P. J. Stanley, Jennifer King, Christopher J. Bay, and Andrew Ning

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First, we would like the express our sincere thanks for reviewing our paper. We know it is a time consuming process, and we are extremely grateful for your thorough and thoughtful review. We have structured this response to be clear and easy to follow. Each of your original comments will be shown in red, immediately followed by our response in black.

**General comments:**

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The authors present an interesting approach to make efficient fatigue estimation for wind farms viable; in particular for use in optimization-based design approaches or possibly in future systems for operation and maintenance. By utilizing analytical models and empirical surrogate models, the study provides a simplified, but transparent methodology for estimating the blade fatigue and shows how this can be used in a simplified optimization context to make decisions about the wind farm layout. A few of the subsections could use additional details and explanations and, as will be explained below, there is a common theme of error estimation that could be considered and/or underlined to strengthen the paper.

**Specific comments:**

What exactly is shown in Fig 2? The y-axis label is not clear and hence it is not clear what is even measured on the y-axis. The caption indicates that this is an "example set" of turbulence samples, but the text on the same page indicates that the figure shows exactly the samples used in the paper. Which is it?

We agree, this was unclear. We added to the caption of Figure 2 to clarify this. The new caption reads:

“The set of turbulence samples, $S$, used in this study. Turbulence intensity is defined as $TI = \sigma_u/\bar{u}$, where $\sigma_u$ is the standard deviation in wind speeds over a given time, and $\bar{u}$ is the mean wind speed. These turbulence samples are used in a future step to calculate an instantaneous wind speed adjusted for turbulence as $u_i = U_{steady}(1 + S_i\, TI)$.”

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Any further comments on the Gaussian Wake model (Page 8-11)? It is indicated that the authors "found good results" with it, presumably when comparing with the SOWFA data (?). Are there any effects that this model is expected to miss? Do you have error estimates for the tuning constants in Tab 1? It would be instructive to include these in the table (as +/-) or show the overall effect on the surrogate fit by having error bars in Fig 4 and 5. If the errors are very small, a short comment to this effect in the text would suffice.

Excellent, we agree this could use some more clarification/justification. We have done the following to address these comments:

- After initially introducing the wake model, we added: “Overall, this model performs very well at capturing the velocity profile in the wake of a turbine, matching high fidelity data very well. For our purposes, the most important physical effects that this model does not capture is inflow flow heterogeneity, which can affect power production and loads.”
- We added a small mention of the comparison of the fit for the different turbulence cases that we considered.
- We added the $R^2$ value of the curve fit to Table 1, indicating how well each model profile fits the SOWFA data.

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Page 11, eq 10 and below: The authors provide a reference to justify the use of the linear wake summation model, but a few more comments here would be instructive (whether the content of these can be found in the reference or not). E.g. what is the motivation for using the linear model over others besides the fact that it "works well with the Gaussian wake model"? Is it used for superior accuracy alone or is it more a case of a simpler model that works acceptably well without introducing further complications? Is there any downside to using this model?

The following text was added to clarify this point:

“This wake combination method has been shown to compare well with experimental data when combined with the Gaussian wake model we used (citation). Additionally, this combination method is equally computationally efficient wake combination methods, such as taking the two-norm of the wake deficits.”

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Similarly as above, any further comments on the turbulence intensity model (Page 12-14) chosen and any possible impact of this choice? Any error estimates for the tuning constants in Tab 3 (or possibly the overall effect on the error level of the model shown in Fig 9)?

The results are fairly sensitive to the turbulence, as you might expect in a scenario of partial waking. In our damage calculations, this means that yes, the results are sensitive to the turbulence model. This is hard to do, as turbulence data is typically quite noisy. With the model, we simply want to get as close as we can to reality with a simple analytic expression. To show the accuracy of the turbulence model that we used, we included the $R^2$ values for each of the fits and for each distance downstream of the waking turbine. A really interesting area for further work would be to explore the sensitivity of the damage with respect to ambient turbulence intensity and wake turbulence behavior.

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What is the expected error level of the surrogate model described in Section 2.9 on Page 17?

We have added the $R^2$ value of each fit to the caption of the figure visualizing the surrogate to give an idea of the error.

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It might be instructive to illustrate a bit more clearly how a load/moment "history" is obtained via the Turbulence and Azimuth Loop, perhaps through some example. Specifically, how this method produces something analogous to the conventional load time series obtained from simulations that are usually the input to rainflow counting-based fatigue assessment methods.

The following has been added to the introduction of the loop calculating the blade loads (1.6) to orient the reader and explain this:

“The steps in this loop are to: 1.7) calculate the turbine inflow wind speed accounting for turbulence, 1.8) using this inflow speed, determine the turbine rotational speed and blade pitch, and 1.9) determine average turbulent wind speed across a blade, and use this speed and the blade pitch in the loads surrogate to determine the blade loads at the time step. These steps are then repeated for as many azimuth angles and rotations that will be simulated. After each time through the loop, the loads calculated in step 1.9 are added to a loads history. The end result is a history of the flatwise and edgewise blade loads, which is used in future steps to make fatigue calculations. ”

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It is indicated on Page 19 (line 360) that the results in the paper are based on load histories obtained from 50 complete revolutions of the rotor. For the NREL 5MW this would be something like a few minutes of simulation time. Conventional time domain-based fatigue estimates are usually based on at least 60 minutes of simulation. The shorter duration is understandable for the purposes of the paper, but do the authors have any comment on this?

Great point, we agree that this is important to acknowledge in the paper. The following was added on this point:

“Although conventional time domain-based fatigue estimates are generally based on a longer time period of simulation, for the purposes of this paper in which we demonstrate the use of this proposed model, this shorter time was used for decreased computational expense.”

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Fig 10 and 11: The y-axis labels should indicate that the values shown are in fact lifetime fatigue values (which is my assumption, but this is not clear).

That is correct. This point was clarified in the caption of the figures.

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In the Conclusion, on Page 30, the use of active yaw control and its possible coupling to the proposed method is discussed for future research. As noted previously in the paper, the yaw angle was fixed at zero in this study. Any comment on what effect (if any) non-zero yaw angles (or yaw errors even) might have on the proposed method?

Great question. We believe it would be a fairly straightforward addition to add in considerations for non-zero yaw angles or yaw errors. From the loads model, the only things that would be required would be to create a 2-D surrogate of the loads, making the blade loads a function of the inflow wind speed and the yaw. The wake deflection that would occur would be accounted for in the analytic wake model, and we believe every other portion of the model could remain as presented.

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I have made several comments concerning error estimates for various parts of the proposed method. Beyond the general interest of such error estimates as indicators for the validity of each simplification, the analytical nature of the authors' methodology actually makes it possible to potentially propagate these errors all the way to the end fatigue result. The resulting error estimates could be very useful and would in fact be a strength of the method. In particular, for optimization it could provide some manner of error bound or expected uncertainty in the result that would show the level of robustness of the solution. Especially in light of Fig 14 and related results. It could also make optimization approaches that consider uncertainty more explicitly more viable for use in similar wind farm studies. While any larger investigation into this issue or indeed carrying out such an error propagation might be out of the scope of the present work, some discussion of these points would be favorable for the paper.

This is a great comment! We agree, considering error estimates and uncertainty is an is important for a model such as this, which is sensitive to the model parameters and uncertainty in inputs. Although, as you state, any significant exploration of this topic is beyond the scope of this paper, we have added to the future work paragraph in the conclusions the following to at least address this topic:

"Fifth, investigate the sensitivities and uncertainties involved with each of the models and assumptions made throughout the model, and how they impact the final damage calculations. This would be incredibly relevant for future studies that specifically include uncertainty analysis. The method presented in this paper uses analytic models, but we expect that the final results are sensitive to model parameters, tuning variables, and uncertainty in any inputs. A better understanding of these uncertainties would be important in building reliable wind farms."

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Do the authors have any comments regarding the use of SOWFA as a benchmark for the accuracy of the proposed method and to what extent SOWFA itself can be used in this manner (i.e. its accuracy)? It is likely worth pointing out that any relevant experimental data for windfarms, which may not be available at present, could presumably be similarly used to tune the parameters of the method, so it is not reliant on SOWFA as such.

Thanks for pointing this out, we did not provide sufficient justification for using SOWFA as our validation. In the revised manuscript, we added a sentence on how SOWFA has been previously validated along with citations. Additionally, we added to the future work section a portion about comparing our model to real wind farms, rather than exclusively to SOWFA.