Comments on “A Model to Calculate Fatigue Damage Caused by Partial Waking during Wind Farm Optimization”

General comments:

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?

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.

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?

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)?

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

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.

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?

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).

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?

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.

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.