Response to Reviewers

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

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Again we would like the express our gratitude for the reviews of our paper. We realize that many people took time out of a busy schedule to read this manuscript and provide feedback, for which we are very grateful. As with our original response to the previous round of comments, we have structured this response to be clear and easy to follow. Each of the original comments will be shown in blue, immediately followed by our response in black.

Reviewer 1

No reviewer comments

Reviewer 2

Some of the R^2 values that were added in the revised manuscript are negative and in one case the negative value is less than -1. Usually R^2 values are in (0,1), but values such as those reported can occur e.g. when fitting non-linear models. The authors note that some of these fits are not as good as others, but make no explicit mention of negative R^2. A small comment noting/explaining these entries in Tables 2 and 3 would be helpful for any reader who might be confused at the presence of negative R^2 values.

In the last manuscript, we had incorrectly calculated the R^2 values separately for each of the separation distances, when only one set tuning parameters was calculated for the wind speed. This has been corrected, and the tables now report one R^2 value for each wind speed instead of 3. All R^2 values in the updated manuscript are between 0–1.

Reviewer 3

Important Comments

1. Figure 5 and related analyses: The performed analysis is correct to evaluate the goodness of the wake model. I was wondering whether it is possible to use directly the profiles generated by LES in a look-up table fashion. In fact, from Tab. 2, it seems that the model is tuned separately at each speed and each turbulence intensity. The estimated parameters do not show a clear behavior with respect to the speed, even in the low TI case, which is the one associated to the best agreement. This is an index of the poorness of the tuning process. Sentence of lines 241-243 ("the damage model is ... while demonstrating our damage model.") offers another link to what I suggested at the beginning: why not using directly the profile extracted from LES?

We have added the following text to the paper to respond to this question:

"Because we have compared all of our intermediate models, as well as the final damage calculations, to the high-fidelity SOWFA and OpenFAST data, one might wonder why we did not directly use some surrogate of the SOWFA and OpenFAST data instead of the lower-fidelity intermediate models, or even create a surrogate directly of the final fatigue damage. These possible methods would likely provide accurate results, and a surrogate would be computationally efficient for use during an optimization. However, a primary purpose of our model is to provide a method to estimate fatigue damage while leaving open the possibility of using computationally efficient analytic models. Our model does not require the user to run computationally expensive, complex, and high-fidelity simulations, although they certainly could. With our method, simple analytic models can be used to sufficiently estimate fatigue damage from partial waking, given that the intermediate analytic models are sufficiently accurate. For this paper, we use tuning constants to improve the comparison of our analytic models to our SOWFA data, which did require us to generate the high-fidelity data. However, for many or most applications where this fatigue model, such model tuning and exact match of previously generated data would be unnecessary."

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2. Figure 7 represents an analysis of paramount importance for the paper, as it provides the justification for the use of a low-order representation of the wake shape. By the way, it is hard to understand what is meant with "final damage values", especially because the damage is addressed in a subsequent part of the paper (see Sections 2.10, 2.11, 2.12). A better explanation is needed. Moreover, why is the comparison made only between 4 and 300 samples. A fair comparison should have been done between these two cases and the SOWFA data (considered as the ground truth).

Great comment and questions. The following sentence has been added to address the issue of "final damage values" being presented at this point of the paper before the rest of the fatigue model has been described:

"The damage values shown in this figure are calculated with the model fully presented in the rest of this section. Even though the full details of this model are presented in the subsections below, we determined that it is appropriate to present this information here to demonstrate the minimal effect that the number of wind speed samples across the swept rotor area has on the final result."

As for the rest of this comment, Figure 7 is meant to demonstrate the effect of the number of wind speed samples used to calculate the effective wind speed across the rotor has on the final damage value, which we still believe it accomplishes. What are now Figures 10 and 11 provide what is suggested in the rest of your comment. These figures show the damage predicted by our reduced order models compared to the "ground truth," which we assume is sufficiently represented by the SOWFA data.

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3. Line 320 to end of Section 2.6: in my opinion this period is not entirely free of errors and may be prone to misinterpretations. I will list my doubts in the following.

a. "... two azimuth angles of 90 degrees and 270 degrees are sufficient to predict the fatigue damage". This sentence comes "out-of-the-blue", first because there is no demonstration for that throughout the paper, and second because, at least at a first sight, it seems a simplistic view. I may frankly say that one is not able to capture a single load cycle with only two samples. To have a complete picture one would need a third piece of information (cf. the Coleman transformation). In any case, even considering three or four samples, the picture results incomplete as well since the higher frequency content may have a significant impact on fatigue.

i. Suggestion: since azimuth 90 and 270 degrees are strictly connected to fatigue induced by partial wake impingement, maybe the Authors can smooth a bit the sentence and refer explicitly to the impact of wakes and not the fatigue in general. This is actually the focus of the paper, and stressing this here may be fair, otherwise one may erroneously think that the simplified model can be used for accurately predicting the entire fatigue of a turbine for rotor design activity. I guess that this is not the Authors' intention.

ii. Suggestion: for the problem at hand, it is important to capture the trend of fatigue with respect to the impingement level, rather than the "real" fatigue. The authors may play a bit around this concept to stress the adequacy of the approach.

iii. Suggestion: is it simple to extend the methodology including more angles? If so, this should be reported. Moreover, what is the expected penalization in computational time induced by the inclusion of more azimuthal samples?

b. "… at this angle [0 and 180] the moments due to gravity are zero". This can be true for inplane loads. For out-of-plane loads, if a turbine has precone and/or tilt, the gravitational loads are maximum exactly at 0 and 180 deg. Due to dynamics of the rotor there is also a small delay in the response of the blades. Finally, since the blade may pitch, out-of-plane and in-plane loads mix together into blade flap- and edge-wise. Authors' sentence is a good approximation for edgewise, low pitch angles and low frequency vibrations.

c. "However, for most conditions, just considering these two azimuth angles is sufficient as they capture the largest load differences which contribute the most to fatigue damage". Here again as in point a, it is hard to demonstrate that with the analyses hitherto explained. If the Authors refer to the sole impact of wake and for the goal of the work (not for characterizing the whole "fatigue damage"), then the sentence can be acceptable. But in the present form, the text should be amended.

Thanks for this comment and the associated suggestions. We completely agree, it is important that in the paper we discuss and justify our decision to evaluate two azimuth angles during during a full rotor rotation. We have added a figure (what is now Figure 9) and the necessary associated text discussing this figure which explains and justifies our decision to use two azimuth angles, the possibility of evaluating with more azimuth angles, and the computational expense. While we have not followed all of these suggestions exactly, we believe we have captured the most important and relevant parts of this comment, and believe the paper is much improved from the changes.

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4. Table 2 and 3: There are negative values in the R^2 metrics. For decent models, usually, R^2 is within 0 and 1, where 1 is a perfect description of data and 0 refers to a model correctness as good as the data mean value. Negative R^2 values are associated to extremely poor predictions. A better comment should be added to explain the obtained R^2 metrics

Refer back to our response from the comment from Reviewer 2. This has been corrected, and the tables now report one R^2 value for each wind speed instead of 3. All R^2 values are between 0–1.

Minor Comments

1. Line 58: "Current fatigue load prediction models are computationally expensive and not suitable for use in an optimization framework". The Authors may be interested in publication https://doi.org/10.5194/wes-4-549-2019, where it is presented a fast evaluation of fatigue based on pre-computed look-up table. Clearly, define the complete set of LUTs with the different types of wakes and overlapping levels is time consuming, but, once computed and stored, using them in an optimization is straightforward.

Great addition. We have mentioned and cited this paper in the new manuscript.

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2. The Authors used "Flapwise" (1 time) and "Flatwise" (10 times) throughout the paper. Is this correct? Could it be uniformized?

Yes that is correct, the use of "flapwise" refers to the moments returned by CCBlade.

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3. Figure 6: a reader should benefit from the knowledge of the location of the single and 100 points.

We added to this figure showing the location of the single and 100 points, in addition to the 4 point locations.

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4. Figure 7: missing x-label.

The missing x-ticks were added to the figure.

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5. Figure 9 and 10: unit of measure in the y-axis?

We added labels to the y-axis indicating that the figure shows damage values.

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6. Figure 9 and 10: there is a consistent overestimation at about +0.5 D. In the text, the Authors mention that this can be due to gravity. Isn't it possible that shear layer be responsible for that?

There was a numerical error in the previous version of the velocity tuning. After correction, the damage values (especially for the low turbulence intensity examples) from our model match the high-fidelity SOWFA data much better. Even so, we added the following sentence to clarify that wind shear could account for some of the differences between our model and the high-fidelity data:

"Another possible reason for the difference between our model and the SOWFA data is wind shear, which is better captured with the high-fidelity SOWFA simulation."

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7. Figure 9 and 10: it is not clear whether the plots refer to flap- or edge-wise loads.

These figures show the total damage calculated by our model, which takes into account both flatwise and edgewise loads. The following sentence was added to clarify this:

"Remember that the damages shown in these figures is the total damage calculated by our model, which takes into account both the flatwise and edgewise loads."

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8. Problem 38: last constraint: it is not clear whether the Authors are constraining the edge- or flapwise moment.

It is the total damage which takes into account both the flatwise and edgewise moments. The following sentence was added to clarify this:

"This fatigue damage was calculated with our model presented in this paper, which takes into account both the flatwise and edgewise loads."