Review of the paper entitled: "A Model to Calculate Fatigue Damage Caused by Partial Waking during Wind Farm Optimization"

General comment:

The paper can be divided into two parts. The first deals with the development of a simplified model to evaluate the fatigue loads of turbines in wind farms, with special emphasis on wake-induced fatigue. In the second part, the simplified model is used to perform farm design optimizations constrained by fatigue.

The idea behind this paper is well worth considering and the manuscript can be certainly cited in future publications in this field.

While the second part is well-done and easy to follow, the first part, related to the development of the simplified fatigue model, deserves some modifications. I indicated some "Important comments" mainly connected to the first part of the paper.

I also had the opportunity to check the previous review run along with the modifications implemented by the Authors to accommodate Reviewers' comments. In this regard, I would like to stress that an indication of mine (see in my review "Important comment #3") refers to a previous reviewer's comment that, in my opinion, was not adequately addressed during the first review round.

I recommend acceptance with minor revisions, but I strongly hope the Authors will pay great attention to "Important comment #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?
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).
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 in-plane 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.
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.

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.
2. The Authors used "Flapwise" (1 time) and "Flatwise" (10 times) throughout the paper. Is this correct? Could it be uniformized?
3. Figure 6: a reader should benefit from the knowledge of the location of the single and 100 points.
4. Figure 7: missing x-label.
5. Figure 9 and 10: unit of measure in the y-axis?
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?
7. Figure 9 and 10: it is not clear whether the plots refer to flap- or edge-wise loads.
8. Problem 38: last constraint: it is not clear whether the Authors are constraining the edge- or flap-wise moment.