Review of "From wind conditions to operational strategy: Optimal planning of wind turbine damage", manuscript id: wes-2022-99

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Summary

The article suggests and evaluates a novel operational method for wind turbines within a wind farm. The long-term fatigue consumption and energy production of each turbine is managed individually by adapting the power output level according to wind conditions to reach lifetime objectives. The motivation for such an adaptation is explained along with all the steps of the process including the controller re-design, the surrogate modeling and the relevant optimization techniques. The results are demonstrated with example use cases, showing the potential applications for equalizing the fatigue consumption among the turbines and a Pareto analysis on fatigue consumption reductions which are then converted to lifetime extension potential and eventually translated to potential economic benefits.

General Comments

This is an interesting article with a novel approach to wind turbine operation showing an alternative to the single-dimensional current operational paradigm. The authors have put a lot of thought and effort into the manuscript addressing a multidisciplinary topic. It has a thorough literature review, the research question is framed sufficiently, the methods used are explained adequately, the language used in terms of syntax and typos is in general good and the topic is relevant to the community and the scope of the journal.

I have concerns about some of the assumptions used, especially for the economical evaluation of lifetime extension, but I reckon that this is a proof of concept and I suggest that these are discussed rather than directly addressed. My major concern is about the readability and clarity of the manuscript. The article is too long with a lot of repetitions and overlaps between the sections making it cumbersome to understand and difficult to navigate through. Moreover, the authors explain extensively methods that are not in the scope of the article or give much more detail than required in places. I think the article has to be restructured, sharpened and significantly decreased in length to be suitable for publication as a journal article.

I believe the manuscript is relevant and should be published after addressing the issues mentioned here through a major revision. Find more specific comments below.

Specific comments

1. Sections 1.3, 2 and 4 have a lot of overlaps, repetitions and some general discussions being more than 26 pages combined. I suggest merging them and discussing each topic only once. E.g.: Show the general method briefly (similar to section 1.3) as an introduction and merge there sections 2, 4.1 and 4.2. Then make a new section only with the results of the use cases (sections 4.3, 4.4). This is an example of possible restructuring, maybe the authors can come up with something different

but in any case, I strongly believe that the methodology (including a much shortened "theoretical background" section) and the formulation of the steps should be discussed only once. E.g. section 2.2 discusses the controller design theoretically and has a kind of literature review which is then partially repeated in 4.1 where the actual controller re-design is shown. Similarly for the surrogate model sections (2.3 and 4.2) and for the optimization objective functions sections (2.4 and 4.3). I suggest merging these and removing more general discussions for other methods or other possible approaches to sharpen the article and help the reader understand the main contribution. These can be discussed in the discussion section and only once throughout the article.

2. Section 5: I recommend revising this section according to the previous comment. The overlaps and repetitions make it lose its focus and it is difficult to read. E.g.: l 951-964 is the summary of the method and is not needed here, l 997-1000, 1026-1031 have been discussed already earlier in one way or another I think they should only be discussed here. Following the previous comment, I recommend moving in this section all the relevant discussions (uncertainties, possible extensions, alternative methods, limitations), removing summaries and making sure that the points mentioned here have not been discussed earlier.
3. A general comment applicable to most of the sections is that they don't require to have an individual introduction and conclusion. I recommend revising and removing/editing these parts throughout the manuscript to shorten the text and help with the flow. Some examples (not exhaustive): L48-50, 119-127, 243-244, 416-419, 456-459, 613-619, 670-673, 756-760, 849-850, 852-853, etc.
4. There are a lot of sentences throughout the article that state that something will be/was discussed in another section. I recommend removing these as they are too many and make reading difficult. Some examples (not exhaustive): L 116-117, 125, 193-195, 370, 466, 489-491, 516-517, 537, 598, 697-698, 717, 843-845, 852, 868-870, 872-874, etc.

Section 1

5. The first two sub-sections do a good job framing the idea, which is multidisciplinary in nature and novel so it requires context to be explained. My only recommendation would be to discuss also [1] which deals with a similar topic from the perspective of varying prices and fatigue budget management.
6. Section 1.3: I think it is useful but should be considered whether it can be merged with some of the following sections (2 and 4) focusing on the description of the methods. See previous comments.

Section 2: Many of the discussions are too broad and theoretical and in some cases out of scope. At the same time, I found a lot of overlaps with the rest of the manuscript. Many ideas are introduced theoretically and then discussed again in the following sections. As per the previous comments here are some example points to be considered

7. L 248-253: Is this discussion relevant to the topic and the section? Since certification is not touched I suggest removing and discussing it briefly only in the discussion section.
8. L 280-288: Same as before there is no need to explain the IEC standard and refer to the probabilistic approach here
9. Section 2.1.2: Too much detail on the definition of linear damage and DELs and the concept of linear damage, Goodman corrections, etc. L 314-329 could be completely removed and the rest shortened to the derivation of eq. 19 which is relevant for the rest of the work.

10. L 396-401: The different load regions don't need to be discussed here. They are already shown and discussed in the controller design section
11. L420-434: General discussion on surrogate modeling that is out of the scope and overlaps with 4.2. Can be removed or significantly reduced.

For practical reasons, the rest of the comments are focused mostly on the technical side and not on highlighting repetitive parts or discussions that can be removed, which is left to the authors.

Section 3

12. Figure 3: The fonts for the turbine numbering are very small and not readable. I also recommend changing the spacing units to rotor diameters instead of meters.
13. Section 3.1.1: Some more information regarding the simulations is needed. E.g. degrees of freedom used etc. Moreover, some information about the aeroelastic code as the site cited does not include technical details. E.g. Are the aerodynamics calculated with BEM? How is the structural modeling (modal, beam theory)?
14. Section 3.2.1: Show a plot with the wind rose and wind distribution derived from the dataset used. Could be combined with figure 3. This would allow a better understanding of the principal directions and the site-specific wind speed distributions. I think this is important as the whole study depends on the binning of the probabilities but there is no information on how these distributions look anywhere in the article although discussed later (l 823-826). Figure 9 has some of this information but it is much further away and it is too finely binned.
15. L 541-550 too much discussion that is out of the focus of the article, just state the sensors used briefly
16. L578: I am curious about the selection of a constant TI for all ambient wind speeds. According to IEC (conservative) but also from my experience with real data the distribution of TI is correlated to wind speed. Why did you choose not to assume such a correlation? To my understanding, this would not affect the computational time as you already include the TI dimension in the surrogate but would give a more realistic distribution of the mean loads over the wind speed bins.
17. Regarding the conditions considered, I could not find the assumed value of shear for the simulations in the manuscript. Are you using a constant value? Maybe add it to table 2?
18. Section 3.2.2: Is the software also taking into account meandering? How is the superposition of wakes treated? Please clarify briefly.
19. L610: 'This is however neglectably small'. I suggest avoiding such qualitative/subjective statements (eg: clearly, significantly, negligibly, definitely, it is apparent, etc.) and replace with quantitative statements or remove. There are similar expressions throughout the manuscript that should be revised. Other examples: L 799, 801, 805, 831, etc.

Section 4

20. L 686: Using polynomial regression ensures smoothness and continuity by default, which as explained in the previous section is required especially for the gradient-based optimization. The main issue I see with using gradient-based optimizers is the possibility to get stuck on local minima. Did you do something to address this (e.g. varying initial points)?
21. L692: What does "maximum degree of 5" mean in this context? Please clarify

22. L694: If I understand correctly you did 6 simulations of 10 min and used the mean value. Is the assumption here that the 10 and 60-minute load is the same or am I missing something? Can you explain this choice?
23. L 701: “Within this section, two things are presented and discussed.” Was it supposed to be a new section here? This sentence seems off (also unnecessary similar to the main comments).
24. L 708: Please explain shortly why you use the training error instead of a test error here. Since the surrogate is going to be probed in any arbitrary value by the optimizer, wouldn’t it make sense to show how it performs for inputs outside of the factorial sampling used for training?
25. L 761-764: I understand that the focus of the work is not on the optimization algorithms, but some more information on the application would be useful for the general understanding. What starting values were used? How many iterations did it require to converge? How fast was it?
26. Figure 6,7: Add in the caption and maybe in the plot the units for the y axes. Meaning, explain it is normalized and state the values used for normalizing. Additionally, it seems the factors for normalizing are randomly chosen. E.g. why not make the power ND with the nominal value (100%) so that the levels of down-regulation are directly seen in the plot? Similarly for the loads.
27. L 773-776: IEC class IA is the highest turbulence class. Is this a typo? In general the sentences “Due…Sect.2” seem to have some text missing or be misplaced. Please revise for clarity
28. Figure 8: This plot is difficult to read due to small fonts and graphic size. I suggest making the turbine markers bigger so that the colour differences are distinguishable. Also, the fonts of the number next to the turbine can be bigger and outside of the turbine circles.
29. Table 3: what does ”with reference from turbine 4” mean? Are these relative damage values? If so the energy has to be also relative. Please clarify in the caption and adjust the table accordingly
30. Table 4: what does “relative damage and energy production compared to operation without derating” mean? Are these values for each turbine relative to its own baseline or to some other turbine? If so, how come the damage values of turbine 4 are the same as table 3? Please clarify in the caption and adjust the table accordingly
31. The previous comments for the relative values are valid in general for the text in section 4. When interpreting the results (e.g. l 782, 783, 789, 790, 803) the notation of values with decimals (e.g. 0.7) are used interchangeably for relative differences and for absolute damage values (which also scale to 1). This can be confusing. My suggestion is to change the notation throughout the manuscript and state percentages when talking about relative values and absolute numbers for the damage.
32. L 822-826: In figures 10b,c and 11b,c it seems that the highest damage/frequency for turbine 4 comes from the wind direction 180. Looking at the previous plots this sector seems to have a low probability of occurrence and low wind speed magnitudes in general. I would expect the sector 200-315 deg to be the most influential. Please comment on this.
33. L 839-845, 891-895: These are not part of the discussion of the results, are overlapping with other sections and can be removed.
34. Figures 12 and 13: As per previous comment I suggest using percentages for relative values to distinguish with the absolute values mentioned in L.860-863, 884, etc.
35. L 913: ‘The numbers are actually valid for a full wind farm’ What does this mean? Including also other farm-related costs? Please clarify
36. Energy and financial benefits of lifetime extension l805, 873-877, sections 4.4 :
    I think this calculation of extra energy production through lifetime extension (and subsequently revenue) has a lot of underlying assumptions. Subsidies are over after the nominal life (usually even earlier) and the selling prices would be reduced or be subjected to the volatility of the market. There are many other factors to lifetime extension like permitting, land costs, inspections/certification and wear and tear of components besides the main ones (bearings,

actuators, gearbox, etc.). Other failure modes, like leading edge erosion, could also lead to either reduced power production, extended downtime, or even make the lifetime extension financially infeasible in total. Additionally, the assumption that all blades would be replaced for all turbines in a farm to operate for a few years more is not realistic. I understand that this is a first approach and a proof of concept but I think these should be clearly stated and discussed more (briefly mentioned in l946-947) as the results can be misleading for actual decision-making.

Section 5

37. I think the assumptions of constant price and costs for the whole operational time including LTE has to be discussed here. As mentioned in the previous comment I think these are the highest contributors to uncertainty and should be emphasized to give the correct perspective for the monetary results presented here.
38. Similarly, inter-annual variability is an important factor of uncertainty based on the assumptions of the study and has to be discussed. Due to the non-linear relationship between loads and conditions, it is hard to predict what the effect would be. A good starting point for the topic could be [2].
39. L 1076-1077: This is not clear to me. Are you referring to the mismatch of actual conditions to the mean wind distribution? Since the wind distributions are pre-defined in the optimization logic how would the actual conditions change the result with the current approach? Please clarify

Minor corrections

- L259: lateron
- L603: bins.D?
- L 775: however?
- L 776: introducing?
- L 785: for instance?
- Tables 3 and 4: Adjust all the values in the table to the same decimal. I.e. 0.7-->0.700 etc
- L1050 many-objective→multi-objective?

References

[1] Kölle, K., Göçmen, T., Eguinoa, I., Alcayaga Román, L. A., Aparicio-Sanchez, M., Feng, J., Meyers, J., Pettas, V., and Sood, I.: FarmConners market showcase results: wind farm flow control considering electricity prices, Wind Energ. Sci., 7, 2181–2200, https://doi.org/10.5194/wes-7-2181-2022, 2022.

[2] Pryor, S. C., Shepherd, T. J., and Barthelmie, R. J.: Interannual variability of wind climates and wind turbine annual energy production, Wind Energ. Sci., 3, 651–665, https://doi.org/10.5194/wes-3-651-2018, 2018.