Authors' Note to the Associate Editor and Reviewers

Title: Sensitivity of Uncertainty in Wind Characteristics and Wind Turbine Properties on Wind Turbine Extreme and Fatigue Loads

Ref. No: wes-2019-2

The authors thank the reviewers for their thorough assessment, comments, and insights. Revised text in the manuscript is marked in red.

Reviewer's comments are shown in blue. Authors' responses are shown in black.

1 General comments
1.1 Summary of key points
The paper is well written and its topic is relevant. The goals are clearly stated: sensitivity analyses for the NREL 5 MW turbine. They are ambitious because a large number of input and output variables are involved and a computationally demanding model (OpenFAST-based) is used. The choice to reduce the complexity of the analysis by using the relatively simple Elementary Effects approach is judicious. However, it seems to me that nevertheless, the problem is still too complex. To be able to tackle it, the authors work with a relatively small set of input vectors. This is the main weakness I find to be present in their analysis and they have not convincingly argued that the number of input vectors is sufficient.
A.   The authors parameterized inflow inputs based on the capabilities of TurbSim and parameterized the turbine inputs to represent the main physical effects where uncertainties were probable.

A further issue is their adaptation of the Elementary Effects approach in a way that is insufficiently justified. The current exposition leaves me doubting that it is really a consistent sensitivity analysis approach. This does not mean that all their conclusions are arbitrary. On the contrary, I would guess that many conclusions about sensitivities are correct due to their broadly consistent nature over the whole input space and remain unaffected by details of the sensitivity analysis. (This may mean that they would also appear in simpler analyses and could perhaps be obtained from expert elicitation.)
A.   Elementary Effects at its fundamental level can only be considered a screening method. However, the introduction of the use of Sobol numbers and radial trajectories increases its efficacy as a method for estimating sensitivity, not just as a screening method. Campolongo empirically demonstrated that the results obtained by EE can converge to a variance-based sensitivity index with increased number of Sobol points. In this work, the authors increased the number of Sobol starting points until the EE-based sensitivity metrics had shown convergence.

Despite my rather negative judgment about the method and assumptions, there are some gems in this paper. Notably, the authors' efforts in obtaining useful ranges for input variables have resulted in overview tables that are more broadly useful in their own right.
A.   Agreed.

1.2 Overview of review aspects

My judgments here are based on my current understanding of the work. Brief justifications are given, but detail and nuance for the negative comments are postponed to the 'Specific comments' section. They may change due to clarification by author comments.

1. Does the paper address relevant scientific questions within the scope of WES?
Yes. Knowledge about sensitivities is of great value to wind turbine design and selection.

2. Does the paper present novel concepts, ideas, tools, or data?
Some. Novel variants of Elementary Effects sensitivity analysis are presented. A lot of interesting, new simulation data was generated and used.

3. Is the paper of broad international interest?
Yes. The discussion is relevant for all locations and in various wind energy research subdomains.

4. Are clear objectives and/or hypotheses put forward?
Yes. To provide sensitivities of relevant output variables relative to coherent sets of input variables.

5. Are the scientific methods valid and clear outlined to be reproduced?
Validity may be tenuous and reproduction would be difficult. (i) I have my doubts that the novel variants of Elementary Effects sensitivity analysis is a proper sensitivity analysis.
A.   See above discussion.

(ii) The determination of input variables is not discussed in sufficient detail for them to be even approximatively recreated.
A.   All input parameter ranges are referenced where possible. The authors believe this to be sufficient.

6. Are analyses and assumptions valid?
One important one is not. I find it doubtful that the set of input vectors is large enough to warrant conclusions as concrete as the ones drawn, even more so given that a nontrivial number of them may not correspond to physical situations.
A.   The authors attempted to select turbine input parameters that are independent from each other. To minimize correlations to inflow input parameters, the results were binned by mean wind speed.

7. Are the presented results sufficient to support the interpretations and associated discussion?
No. This is a consequence of my evaluation of the two preceding points.
A.   The authors have addressed these points.

8. Is the discussion relevant and backed up?
Yes. Based on the results the authors present, the conclusions are reasonable.

9. Are accurate conclusions reached based on the presented results and discussion?

Ambiguous. Given the presented results and discussion, the conclusions are accurate, but I think that flaws in the analysis and assumptions cast doubt on that accuracy.

A.   These concerns have been addressed above.

10. Do the authors give proper credit to related and relevant work and clearly indicate their own original contribution?
Yes. According to my knowledge they certainly do. And notably, they make very good use of information in the literature for ranges of input variable values.

11. Does the title clearly reflect the contents of the paper and is it informative?
It can be improved. Currently, the title implies a scope that is larger than in actuality and is a bit long and complex. Suggestion: "Elementary effects sensitivity
analyses of the NREL 5 MW turbine"

A.   The title has been modified.

12. Does the abstract provide a concise and complete summary, including quantitative results?
Yes, but. (i) Quantitative results are not given, but neither are they appropriate. (ii) The future applications listed are not sufficiently discussed in the paper to be included.

A.   The future applications provide the justification for doing the research in the paper and are therefore included in the abstract.

13. Is the overall presentation well structured?
Yes.

14. Is the paper written concisely and to the point?
Yes. There is a bit of repetition in the presentation of the second case study, but this redundance may actually make the paper easier to read.

15. Is the language fluent, precise, and grammatically correct?
Yes.

16. Are the figures and tables useful and all necessary?
Not all. I found Figures 5–8, 10–11, and 15–22 of limited usefulness; the information should be more filtered and summarized to be useful. In contrast to these stand especially Figures 9 and 23.

A.   Some figures have been moved into an appendix.

17. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used according to the author guidelines?
Not all or in all aspects. Standards about notation of variables and constants are not followed and a mixture of fonts is distractingly used for mathematical notation.

A.   The authors have attempted to improve on this. Additionally, the paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

18. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
Yes. For example, Figures 5–8, 10–11, and 15–22 as mentioned above.

A.   Some plots have been moved to the appendix.

19. Are the number and quality of references appropriate?
Yes.

20. Is the amount and quality of supplementary material appropriate and of added
value?
No supplementary material has been provided. It would have been useful if the simulation data
(inputs, outputs) were made avaiable, but it is of course the prerogative of the authors not to do
so.
A.   The authors believe that this is impractical, especially for an already lengthy paper.

2 Specific comments
2.1 Your modified EE formulae
There is insufficient justification of your modified formulae. You indicate why you add Yob in
Eq. (3) and use a dimensional version, but just mentioning it is insufficient as an explanation. To
me, adding a constant term to a set of sensitivities will substantially distort the information
present in the quantity; it is not a sensitivity anymore. I can sense a reason for making it
dimensional, but I can think of other reasons why this is a bad idea; you should pre-emptively
remove such doubts. Currently, I am not convinced at all that what you call a sensitivity here can
be interpreted and used as such.
A.   The inclusion of Yob was necessary to properly compare ultimate load consistently
across the bins because it is only the maximum ultimate load that matters.

In Eq. (5), you multiply by a probability and again indicate why, but do not explain it at all.
Here, I can guess at the reason.
Part of my reticence here is due to the fact that I am skeptical of your approach to the
identification of most sensitive inputs; this is discussed next. As your modifications here are, I
assume, related to your non-standard approach, it may be good to explain them concurrently in
the text.
A.   The scaling by the probability was necessary to properly compare fatigue loads
consistently across the bins because it is the cumulative effect of all bins that matters for fatigue.

2.2 Your approach to the identification of most sensitive inputs
First of all, by looking at plain means, you implicitly assume that your samples are uniformly
distributed over the input space. Even if you cannot justify this, you should at least discuss the
implications. Related to this, you apparently do not calculate the expectation over wind speeds.
Of course, for fatigue loads this is actually what happens because you have included the
probability in the EE value. For ultimate loads, it does not, where I see no reason why you
should throw away the probability information that you do have here and replace it with a
uniform distribution (implicit by taking the plain mean, as said before).
A.   The authors looked at the convergence of the number of starting points. Starting points
were added until adding points no longer changed the mean of the EE values. Therefore, while
there may be improbable events included in the parameter space, they do not affect the overall
conclusions.

Second, you say that you do not use the standard approach, but do not really justify that. You refer to an appendix and there is information about that there, but take into account that appendices are meant for skippable material. When reading your approach and the standard one as sketched in the appendix, the latter was more appealing to me. The reason is that there the rankings are primarily based on the means.

A.   The authors found that the exceedance approach was more justifiable and intuitive for the purpose of this research. The standard approach results was included in the appendix for potential readers who are interested in such results, but were not found to be the most beneficial presentation of this research.

You base your rankings essentially on tail behavior of the EE value distribution. Tail behavior says very little about what happens in the bulk of the distribution. It may well be that a distribution with a low mean but a fat tail generates more instances above your somewhat arbitrarily defined threshold than a distribution with a high mean but a skinny tail. Based on this reasoning, I fear that your approach may unduly rank higher inputs that generate fat tails, based just on the tail and not on the mean. It is possible that the tail behavior is similar over the inputs and then your criterion will work, but you do not show or discuss that. Even in that case I see no reason not to focus more on the means. So, a good start to convincing me is to explain why a ranking of means is not appropriate here and how your criterion overcomes the problems apparently present in other approaches.

A.   The authors believe that the exceedance probability plots and histograms show such behavior and therefore allow for such concerns to be addressed in the analysis.

2.3 Your selection of input vectors
You use thirty input vectors times three—one for each wind speed bin—for both case studies. The quality of the selection of these points is essential, but the way in which you choose them is dealt with only in a single sentence where you claim that by using Sobol numbers—without providing details—you can ensure a wide sampling of the input hyperspace. Furthermore, you choose to ignore the dependencies between the input variables, but directly sample from the Cartesian product of the individual ranges you have defined, so non-physical input vectors could be included. Finally, your input spaces are very large, 18-dimensional and 40-dimensional respectively.

A.   As mentioned above, a convergence study on the number of starting points was performed and the authors attempted to minimize correlations between inputs.

In such high-dimensional spaces, even a few dependencies can cause the subset of physical vectors to be 'small' relative to the Cartesian product. So it may be difficult to actually land on a physical vector, when those dependencies are not taken into account. (I do not see how using Sobol numbers can help with this issue, as more even sampling cannot correct for ignored dependencies.) This may mean that I cannot exclude the possibility that the majority or even all of the input vectors you use is non-physical. The fact that 90 input vectors in such high-dimensional spaces is a very small sample, only makes this issue more problematic. (I understand that for computational time reasons you cannot increase this number by orders of magnitude.) Because a priori I must assume that non-physical input vectors may result in non-realistic output sensitivities, this issue undermines your results.

In the conclusion, you state that "The combinations of parameters in this study spanned the ranges of several different locations." Given the reasoning I developed above, you will understand that I am skeptical of this. But this can be tested: how representative are your input vectors of existing locations? This may provide an avenue to reduce my worries about your selection of input vectors.

A.   These concerns have been addressed above.

2.4 Your presentation of applications and future work
In the conclusions, you present a number of possible applications of your work and also ideas for future work. I feel that some—about error bars and insight—are described to briefly or too vague to really know whether indeed, your results provide a useful starting point.

A.   The authors believe it is important to focus on what was done in the paper. Since this resulted in a lengthy paper, it was decided to simply highlight potential future work to save space.

3 Technical corrections
3.1 General
• In mathematical notation, the following standard conventions are prescribed: variables are written in a cursive/italic/slanted font and constants are written in a roman/upright font. https://www.nist.gov/pml/nist-guide-si-chapter-10-more-printing-and-using-symbols-and-numbers-scientific-and-technical#102 Please do so throughout, as currently this is not adhered to and most every symbol is put in italic font, even word abbreviations (which are certainly constant).

A.   The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

• As per the SI standards, between a value and a unit there should always be a normal (unbreakable) space. So do not write '5-MW', but '5 MW'; there are other examples in the text where no space is present. Also, composed units should not be separated by a hyphen, use a centered dot instead; e.g., 'kN·m' instead of 'kN-m'.

A.   These corrections have been made throughout the paper.

• You abuse the same symbol for functions and variables: for example Y and Yo instead of f and fo; avoid that, as it causes confusion.

A.   The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

• Your presentation of the EE approach is symbolically far more verbose than it needs to be. Suggestions:
–Make effective use of vectorial notation. For example, Eq. (2) could be written as
$$EE_i = f(\sim U + \Delta \sim e_i) - f(\sim U)\Delta.$$
(Arrows instead of bold here only because of reviewing system limitations.)

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

–Use consistent notation for elements (lower case) and sets (upper case). For example u∈U, y instead of Y.

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

–Make subscripts more 'direct' by avoiding index layers and using variables directly. For example, v instead of both b and vb, u instead of both r and ur.

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

–Put commas between subscripts and do not overburden subscript/superscript locations.

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

–Predefine some recurring fragments, such as $\Delta v_i = maxU_{vi} - minU_{vi}$ and $\delta v_i = \Delta v_i/10$.

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

Combined, you could write Eq. (3), (4), and (5) as

$$EE_{vi,o}(\sim uv) = \bar{y}_{vo} + 10|f_o(\sim uv \pm \delta v_i \sim e_i) - f_o(\sim uv)|$$
$$\text{with } f_o(\sim uv) = 1|S|\sum s \in S_{maxt}|f_{o,s}(\sim uv,t)|,$$
$$EE_{vi,o}(\sim uv) = 10|g_o(\sim uv \pm \delta v_i \sim e_i) - g_o(\sim uv)|p(v)$$
$$\text{with } g_o(\sim uv) = ...$$

A.   This has been somewhat addressed. The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

• You use 'parameters' where I would use 'variables', given that the focus is on varying those quantities. For example, in your setup, I would call wind speed a variable, as it parameterizes some sub-cases, but is not varied as, e.g, wind shear is.

A.   The authors chose to use "parameters" because that is the term used to define inputs in the aero-elastic software used in this research and makes the most sense in the context of this work.

• Throughout the paper, the formulation of sensitivities are according to my ear often reversed. In the paper, input variables are called '(most) sensitive', whereas I would apply such language to output variables only. For input variables, words like 'impactful' and 'influencing' come to mind, although these do not seem ideal. Consider changing the language, but for the input side feel free to find a better word than the ones I came up with.

A.   The authors prefer to keep the language as is, which has precedent in related publications.

• Do not break tables over multiple pages; if you must, repeat the header row.

A.   This is absolutely true. However, as formatting will change significantly with final publication, this has not been specifically addressed here.

3.2 Local
p.2, l.33 OpenFAST is software, not an approach.

A.   This has been changed in the text.

p.3, l.6 What does 'down-selected' mean?
A.   This has been changed to "selected".

p.3, l.13 You must say how this was assessed.
A.   This is thoroughly addressed in Section 3.

p.4, Table 1 This nice table can be improved by adding symbols and a clear indication of which variables are used for which case study (wind vs. turbine and ultimate vs. fatigue).
A.   The authors believe that the table is best presented as is. All QoIs are used for all the studies.

p.4, §3.1 Paragraph way too long; split in two or three.
A.   The paper will go through a formal review process with an editor. Any remaining issues will be addressed at that time.

p.5, l.20 Elaborate, the current explanation about the 'radial approach' is too limited.
A.   More explanation was provided in the paper.

p.5, l.22 Elaborate, the current explanation is too limited. How did you obtain the Sobol numbers and how did you use them.
A.   One can use Sobol sequences to fill up a space with "random" points. The nice thing about it is that the points distribute themselves fairly evenly and therefore sample the space uniformly (to a good extent) without having a pattern per se.

p.6, l.1 Say how and why it was modified or at least reference forward.
A.   A reference note was made in the paper.

p.6, l.8 'nondimensionalizing' to 'making dimensionless/adimensional'
A.   This change has been made.

p.6, l.9 'derivative' to 'finite difference'
A.   This change has been made.

p.6, l.10 'IEC turbine class I and category B' to 'IEC turbine class I and category B' (to avoid confusion with index sets I and B)
A.   This change has been made.

p.6, Eq. (3) and (5) $ib\pm1$ to $bi\pm1$ (but better still follow my general suggestions regarding math notation to avoid making such mistakes)
A.   The paper will go through a formal review process with an editor. Issues such as this will be addressed at that time.

p.6, Eq. (4) In principle, the LHS depends on the set of seeds

A.   The authors performed a convergence study on the number of seeds used in the study to ensure that the solution converged independent of the seeds.

p.6, l.20 Why choose the sign randomly? Does this matter?
A.   This allowed for more of the multi-dimensional input parameter space to be considered.

p.6, l.20 and elsewhere 'IEC-Class IB' to 'IEC Class IB'
A.   This change has been made throughout the text.

p.6, l.20 move 'The added term . . . ' forward in section.
A.   This section has been rewritten.

p.6, l.23 Define DEL (as you should all letter words); do not assume all your readers will be familiar with this.
A.   This term was previously defined on page 3, line 22.

p.7, l.6 Your notation includes indices that are in fact averaged away (r, i, b) for the mean EE value, but for the (sample) standard deviation, you include not even the one that isn't gone (o). Some consistent, standard notation would be appropriate here; consider something like the sample mean mo and sample standard deviation so, perhaps.
A.   The authors believe that once EE has been clearly defined, it is acceptable to simply state that the EE values have a mean and standard deviation.

p.7, l.8 The part about 'stratification' requires more explanation, certainly because it is not immediately clear in what way this differs between the two case studies. (Perhaps you can reference to a relevant earlier part of the text.)
A.   This has been clarified.

p.7, l.8–12 The way in which you decide to include or exclude some wind speed bins comes accross as somewhat arbitrary here. More explanation may be needed.
A.   This section has been clarified.

p.7, l.17 'speed bins' to 'speeds'
A.   The paper will go through a formal review process with an editor. Issues such as this will be addressed at that time.

p.7, l.24 'Holtstag' to 'Holtslag'
A.   This has been corrected.

p.7, l.35 'either better optimized or lower risk': optimized for what? lower risk of what? Avoid vagueness here, as concrete applications are a good justification for your work.
A.   By increasing accuracy, you are eliminating uncertainty in your analysis. A system would be better optimized if, based on reducing uncertainty, you are able to narrow your design margin. However, if your uncertainty was actually allowing you to have a design that had a higher probability of failure than expected, lowering this uncertainty would therefore reduce the risk of failure, as the system would be appropriately re-designed.

p.8, Table 2 use the same, correct mathematical symbol, e.g., 'u(σu)'.
A.   This nomenclature is consistent with the IEC wind turbine design standard.

p.8, l.16 Suggestion: for q, use the velocity component name directly, instead of an index.
A.   The paper will go through a formal review process with an editor. Issues such as this will be addressed at that time.

p.8, l.19–21 Giving the exact quantity definitions of the IEC standard here is superfluous, certainly because you do not use it.
A.   The authors believe these definitions are important in the context of this work.

p.8, l.22 'and random' to 'and to the random' (?)
A.   The authors believe the grammar is fine as is.

p.9, l.9–10 'term' to 'factor'
A.   This has been corrected.

p.9, l.11 'in (IEC, 2005)' to 'in the standard (IEC, 2005)' (text should make sense when reading aloud, skipping over citation parentheticals)
A.   This has been corrected.

p.9, l30 'B' to 'B'
A.   This has been corrected.

p.10, Table 3 header
• There is no need for parentheses around units.
A.   The authors disagree. This is standard practice.

•bu,bv,bw are not dimensionless; I suggest using 1/mm for convenient notation of the values.
A.   This has been corrected.

p.10, Table 3 footnote Elaborate a bit.
A.   At extremely negative values of the shear exponent, α, tower blade strikes would often occur. To eliminate this issue, the minimum value of α was changed to -0.75 from -1.5.

p.10, l.7 'unphysical' to 'non-physical'
A.   This has been corrected.

p.11, Fig. 3–4 Suggestion: use a logarithmic axis for the counts, then zooming will not be necessary and more information should become visible.
A.   The authors originally used a logarithmic scale on the y-axis. However, it was found that the figures are easier to interpret without a logarithmic axis.

p.19, Tables 4–5 Make it explicit in the table whether the numbers given are percentages or counts.
A.   This has been clarified in Tables 4, 5, 12, and 13.

p.20, l.8–9 Provide more information about the expert(s) and the elicitation procedure used.
A.   When references were not available for specific parameters, experts in the area of study at the NWTC were sought out.

p.20, Table 6• Do not use the empty set symbol Ø instead of the Greek letter phi φ.
A.   This has been corrected.

•BM,imb to BM,imb (just an example of the type of math font use corrections that you should implement; note that the font of the subscripts, which here refer to words, are upright)
A.   These corrections have been made throughout the paper.

p.21–22, list around page break What are the parenthetical, bold math symbols for?
A.   They are identifying the symbols that are associated with the described parameters.

p.22, Figure 12 Avoid putting figures in the middle of paragraphs.
A.   This is something that will be changed with final formatting. As such, it is not addressed here.

p.23, l.18–19 Avoid line breaking tuples.
A.   Again, this will be changed in final formatting and is not addressed here.

p.23, l.19–20 Use paragraph breaks as defined by the style.
A.   Again, this will be changed in final formatting and is not addressed here.

p.24, Figure 13 •Are curves for Cd,orig and Cd,−10% missing here, or do they just overlap?
A.   The lines overlap. This is explained in the text.

• This Figure is not referenced in the text, I think. (All figures and tables should be.)
A.   This reference was accidently deleted during formatting and has been added back in on page 23, line 17.

p.25, l.8–23 This paragraph is too long; split.
A.   While the paragraph might be lengthy, the authors believe that all of this information belongs in one paragraph.

p.25, l.17 'relevant' to 'relative' (?)
A.   This change has been made.

p.27, l.3–17 This paragraph is too long; split.
A.   While the paragraph might be lengthy, the authors believe that all of this information belongs in one paragraph.

p.37, l 1 'hear' to 'here'

A.   This has been corrected.

p.38, Figures 24–25 I think that plots of σ/μ vs. μ would provide more insight here.

A.   Plots of σ vs mu are standard practice in EE literature. Since these plots were included to show the results in the standard way, they are kept as is.