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WIND ENERGY SCIENCE DISCUSSIONS

# *Interactive comment on* "Sensitivity of Uncertainty in Wind Characteristics and Wind Turbine Properties on Wind Turbine Extreme and Fatigue Loads" *by* Amy N. Robertson et al.

**Anonymous Referee #2**

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## 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 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.)

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.

### 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. (ii) The determination of input variables is not discussed in sufficient detail for them to be even approximatively recreated.

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.

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.

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.

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”

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.

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 infor-

mation should be more filtered and summarized to be useful. In contrast to these stand especially Figures 9 and 23.

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.

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.

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.

## 2 Specific comments

### 2.1 Your modified EE formulae

There is insufficient justification of your modified formulae.

You indicate why you add $\bar{Y}_{ob}$ 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.

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.

### 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).

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.

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.

### 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.

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.

### 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.

### 2.5 Compliments

**p.2, §2.1, list** Nice and clear overview of limitations.

**p.4, §3.1** Nice, compact overview.

**p.4, §3.2** Well-explained.

**p.10, Table 3** Very useful overview table.

**p.18, text; p.19 Tables 4–5** This (type of) summarizing is very useful.

## 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).

- 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'.

- You abuse the same symbol for functions and variables: for example $Y$ and $Y_o$ instead of $f$ and $f_o$; avoid that, as it causes confusion.

- 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

    $$EE_i = \frac{f(\vec{U} + \Delta\vec{e}_i) - f(\vec{U})}{\Delta}.$$



    (Arrows instead of bold here only because of reviewing system limitations.)

  - Use consistent notation for elements (lower case) and sets (upper case). For example $u \in U$, $y$ instead of $Y$.
  - Make subscripts more 'direct' by avoiding index layers and using variables directly. For example, $v$ instead of both $b$ and $v_b$, $u$ instead of both $r$ and $u^r$.
  - Put commas between subscripts and do not overburden subscript/superscript locations.
  - Predefine some recurring fragments, such as $\Delta_i^v = \max U_i^v - \min U_i^v$ and $\delta_i^v = \Delta_i^v/10$.

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

  $$EE_{i,o}^v(\vec{u}^v) = \bar{y}_o^v + 10\,|f_o(\vec{u}^v \pm \delta_i^v\vec{e}_i) - f_o(\vec{u}^v)| \quad \text{with} \quad f_o(\vec{u}^v) = \frac{1}{|S|}\sum_{s\in S}\max_t |f_{o,s}(\vec{u}^v, t)|,$$

  $$EE_{i,o}^v(\vec{u}^v) = 10\,|g_o(\vec{u}^v \pm \delta_i^v\vec{e}_i) - g_o(\vec{u}^v)|\,p(v) \quad \text{with} \quad g_o(\vec{u}^v) = \ldots$$

- 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.

- 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.

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

3.2 Local

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

**p.3, l.6** What does 'down-selected' mean?

**p.3, l.13** You *must* say how this was assessed.

**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).

**p.4, §3.1** Paragraph way too long; split in two or three.

**p.5, l.20** Elaborate, the current explanation about the 'radial approach' is too limited.

**p.5, l.22** Elaborate, the current explanation is too limited. How did you obtain the Sobol numbers and how did you use them.

**p.6, l.1** Say how and why it was modified or at least reference forward.

**p.6, l.8** 'nondimensionalizing' to 'making dimensionless/adimensional'

**p.6, l.9** 'derivative' to 'finite difference'

**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 $B$)

**p.6, Eq. (3) and (5)** $ib \pm 1$ to $bi \pm 1$ (but better still follow my general suggestions regarding math notation to avoid making such mistakes)

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

**p.6, l.20** Why choose the sign randomly? Does this matter?



**p.6, l.20 and elsewhere** 'IEC-Class IB' to 'IEC Class IB'

**p.6, l.20** move 'The added term . . . ' forward in section.

**p.6, l.23** Define DEL (as you should all letter words); do not assume all your readers will be familiar with this.

**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 $m_o$ and sample standard deviation $s_o$, perhaps.

**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.)

**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.

**p.7, l.17** 'speed bins' to 'speeds'

**p.7, l.24** 'Holtstag' to 'Holtslag'

**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.

**p.8, Table 2** use the same, correct mathematical symbol, e.g., 'u ($\sigma_{\mathrm{u}}$)'.

**p.8, l.16** Suggestion: for $q$, use the velocity component name directly, instead of an index.

**p.8, l.19–21** Giving the exact quantity definitions of the IEC standard here is superfluous, certainly because you do not use it.

**p.8, l.22** 'and random' to 'and to the random' (?)

**p.9, l.9–10** 'term' to 'factor'

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

**p.9, l30** '$B$' to 'B'

**p.10, Table 3 header**
- There is no need for parentheses around units.
- $b_{\mathrm{u}}$, $b_{\mathrm{v}}$, $b_{\mathrm{w}}$ are not dimensionless; I suggest using 1/mm for convenient notation of the values.

**p.10, Table 3 footnote** Elaborate a bit.

**p.10, l.7** 'unphysical' to 'non-physical'

**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.

**p.19, Tables 4–5** Make it explicit in the table whether the numbers given are percentages or counts.

**p.20, l.8–9** Provide more information about the expert(s) and the elicitation procedure used.

**p.20, Table 6**
- Do *not* use the empty set symbol ∅ instead of the Greek letter phi $\phi$.
- $\mathrm{B}_{\mathrm{M,imb}}$ to $B_{\mathrm{M,imb}}$ (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)



**p.21–22, list around page break** What are the parenthetical, bold math symbols for?

**p.22, Figure 12** Avoid putting figures in the middle of paragraphs.

**p.23, l.18–19** Avoid line breaking tuples.

**p.23, l.19–20** Use paragraph breaks as defined by the style.

**p.24, Figure 13**
- Are curves for $C_{\mathrm{d,orig}}$ and $C_{\mathrm{d},-10\%}$ missing here, or do they just overlap?
- This Figure is not referenced in the text, I think. (All figures and tables should be.)

**p.25, l.8–23** This paragraph is too long; split.

**p.25, l.17** 'relevant' to 'relative' (?)

**p.27, l.3–17** This paragraph is too long; split.

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

**p.38, Figures 24–25** I think that plots of $\sigma/\mu$ vs. $\mu$ would provide more insight here.

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