Discussion of:

Performance of non-intrusive uncertainty quantification in the aeroservoelastic simulation of wind turbines

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The authors present the application of two non-intrusive uncertainty propagation techniques: Universal Kriging and Polynomial Chaos Expansion, as means of propagating the effect of uncertainty in wind conditions and blade aerodynamics on wind turbine loads. The manuscript describes the process of setting up the uncertainty propagation models and demonstrates an application on a 10MW research turbine. In the results section, the authors show how the uncertainty in two variables – the airfoil unevenness, and the extent of degradation along the blade span, affect the distribution of various wind turbine load components. The article is well structured and clearly written, and deals with a relevant scientific problem. In my opinion, the manuscript will benefit scientifically if the authors go in further depth in some aspects of their analysis. These recommendations are given in the comments below.

General comments

1) In several places in the paper (e.g. page 5, line 3) the authors state that there are some potentially significant sources of uncertainty, which are not considered in order to allow more focus on other relevant uncertainty sources. This is reasonable; however in such a situation it is important to understand what is the effect of not considering these uncertainties. For example, would the ignored uncertainties have the same effect over the entire variable space considered, meaning that they will not mask the relative effects of other uncertainties? Or will their effect mix with that of other uncertainties meaning a larger model error in general?
2) The uncertainty propagation models are trained based on variable spaces with beta-distributed marginal variables. Then the probability density functions for the response surfaces are plotted based on a Monte Carlo simulation which apparently uses the abovementioned marginal distributions. However, these sampling distributions do not fully correspond to the real-world distributions of the uncertainty variables. It is therefore difficult to judge on whether a given load event is critical as it may have a high probability of occurrence in the sampling space used to train the uncertainty propagation model, but low probability in the real world, and vice versa. I suggest that the authors redo the MC analysis (Figure 6) using realistic joint distributions of the uncertainty variables. This is also a key distinguishing point between uncertainty propagation and uncertainty quantification: the response surface only propagates the uncertainty, so in order to quantify the uncertainty of the dependent variable we need to feed the propagation model with the right input uncertainties.
3) To me, the authors are considering a manifold of four random quantities: two uncertainty variables (k_{AF} and ESD) combined with two environmental conditions – wind speed, and turbulence intensity (and wind shear as fully dependent on the latter two). I think it will make the paper clearer if the presentation is made along this logic. In this way one can also distinguish between point-to-point uncertainty between individual realizations, and the effect of the two uncertainty

factors integrated over the joint distribution of the environmental conditions (which is what I believe is the purpose of Figure 6 in the current manuscript).

4) It is not clear whether the results reported in Figure 6 are averaged over the wind speed or not. If we were considering integrated quantities such as e.g. fatigue loads, it would be relevant to show the average values. However, when talking about extremes it would be more appropriate to not do any averaging, and instead include the wind speed as one of the factors in computing the pdf of the extreme loads. This also relates to the comments above.

Specific comments:

5) Page 3, line 20 (first paragraph of Section 2): This is a classification of the uncertainties according to the physical mechanism that causes them. Another maybe even more relevant classification could be according to their type, e.g., statistical, measurement, model, human-caused… This should make it easier to categorize the uncertainties.
6) Page 3, lines 23-25: "Not only the nominal values of all such parameters are uncertain, but additional sources of uncertainty are introduced by manufacturing processes and the status of wear and tear of each individual machine or component". Another uncertainty source which the authors should consider here is the measurement uncertainty: the observed value of a given variable is different from its true value due to imperfect observation. This also means that we don't necessarily know the true reference.
7) Page 4, line 8: The authors describe that turbulence boxes include random realizations of a turbulence field. It would be useful to describe in more details what are the statistical properties of these randomly generated fields – e.g. are they Gaussian, what are the spectral parameters.
8) Page 4, line 12: "…These effects may alter in a significant way the statistics of the wind at a given site. All such effects are difficult to measure and quantify with precision…" What the authors refer to may be considered as a kind of measurement (epistemic) uncertainty due to not being able to quantify the variables with sufficient precision. A specific reference to this type of uncertainty can be found in Tarp-Johansen at al. [1] where this is referred to as "Exposure uncertainty".
9) Page 4, eq. 1: Please note that in Dimitrov et al. (2015) the reference turbulence intensity TI_{ref} is a function of the turbulence quantile, i.e., the wind shear distribution changes with respect to the turbulence quantile. What kind of turbulence quantile have the authors considered as TI_{ref}? Is that taken into account by the uncertainty factor k_{TI}? I think the authors have to explain the relationship between the turbulence quantile and k_{TI}.
10) Page 5, line 14: "either uniform or a beta probability distribution" – why either distributions and not one specific?
11) Page 6, line 22: Is the severity of surface degradation k_{AF} assumed to be uniform over the full extent of spanwise degradation (ESD)? I would suggest that a more realistic approach would be to have 1) k_{AF} as a random, spatially-correlated variable over the blade span, and 2) the expected value of k_{AF} to gradually increase towards the blade tip. This could still amount to some integrated degradation measure.
12) Page 6, line 9: What was the trend function used in the Universal Kriging approach? One could consider e.g. a polynomial chaos expansion as a trend function – one could even make use of the NIPCE already trained as a standalone model.

13) Page 6, line 29 (and Figure 3): what turbulence quantile does TI_{ref} refer to? Why is the turbulence uncertainty factor k_{TI} beta-distributed, normally one could use the standard assumption that the turbulence (standard deviation of wind speed) is log-normally distributed? Again, in continuation to a previous comment, we need an explanation of the relationship between the turbulence probability distribution and the uncertainty factor k_{TI} and what are the implications of replacing the turbulence distribution with k_{TI}.
14) Page 9, Table 4: are these statistics based on the full data set over all wind speeds? Have the results been Weibull-weighted according to a certain wind speed probability, or is the wind speed probability considered uniform? Is the "standard deviation" the sample standard deviation, or the uncertainty in the mean estimate?
15) Page 10, line 7: give a definition of the collocation ratio
16) Page 10, line 8: what is the sampling distribution of the MC? Is that the same as the MC sample used to train the models?
17) Page 10, line 11: the UK converges faster than the NIPCE. Could that be because Kriging is in essence an interpolation scheme, and the response is linear enough (as the authors point out themselves) so that a few points are sufficient to establish a reasonable extrapolation?
18) Figure6: there are some "wrinkles" in the contour plots. Could these be caused by having few data points (40 function evaluations only)? What if we added more data – maybe the contours would resemble more straight lines (= closer to linear dependencies)?
19) Page 13, line 24: "…the deterministic conditions prescribed by international design standards generate maximum values of loads and power production, which however are typically associated with a very low probability of occurrence". This is guaranteed only if the sampling distribution used to propagate the uncertainty is the same as the real-world distribution of the random input variables. As discussed in the general comments, this is not necessarily the case with the present data sets.

Technical comments:

20) Page 4, line 14: "give turbine" -> "given turbine"
21) Page 8, line 10: "converge" -> "convergence"

References:

[1] Tarp-Johansen, N., Madsen, H. & Frandsen, S. T. (2002) Partial safety factors for extreme load effects. *Technical report Risø-R-1319(EN)*, Risoe National Laboratory, Denmark