Interactive comment on "Performance of non-intrusive uncertainty quantification in the aeroservoelastic simulation of wind turbines" by Pietro Bortolotti et al.

Anonymous Referee #1

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The authors present a well-written and well-motivated application of standard non-intrusive uncertainty calculations to the estimation of loads on a wind turbine.

There are, however, a few areas where additional clarity or corrections to the text and figures are required:

Abstract: In the first line, "uncertainties" should be replaced by "aleatory uncertainties". The last sentence should also be made more specific as to what the effects and shortcomings are.

Section 2, page 1, line 21: "uncertainties are [...] only indirectly accounted for" - the concept of "indirect" uncertainty calculation should be explained, preferably with a ci-

tation of an example. Section 2.1, page 4, line 14: "give" should read "given". Section 2.1, page 4, lines 16-20: The choice of a Beta distribution (actually strictly speaking a scaled Beta distribution, since the input values do not always lie between 0 and 1 - see also Section 2.2, page 5, line 21 for ESD) is not sufficiently motivated. This distribution has some specific purposes in the statistical literature, in particular for expressing an uncertainty distribution over a probability. The reason for the turbulence intensity to be modified by a factor which lies between 0.5 and 2 is not explained, since it implies that the turbulence intensity corresponding to $k\_TI=1$ will not actually be the mean or median of this distribution? A log-normal or truncated Gaussian distribution (with mean or median set to 1) would appear more appropriate. Section 2.1, page 4, lines 21-22: The Dimitrov paper does not appear to contain this equation, and the physical motivation behind asserting that $SE = SE\_ref + a/TI - 1/4$ (where a is a constant) is not obvious. The equation is in any case unclear, as TI(k) looks like a function, but appears to be a distribution, from the description on line 24. Section 2.1, page 4, lines 26-28: The method by which the k_TI values in table 4 have been derived should be explained, to aid reproducibility.

Section 3.1, page 8, line 3-4: what does it mean, to say that the mean is below 1%? Section 3.1, page 8, line 10: "converge" should read "convergence". Section 3.2, page 11, Figure 5: This figure is difficult to understand. Does the y-axis label "difference in" refer to a change between adjacent function evaluations? What is the definition of "potential inexactness" that the grey band is representing, and what information does it give the reader about the other lines on the graph? Finally, the legend says "1.1k MC" whereas the rest of the text indicates 1200 evaluations. Section 3.3, page 12, Figure 6: More explanation is required concerning the pdf values being shown - how should they be interpreted? They are different to the pdf values being shown in Fig 5. The pdf values are presumably also not conditional on $k\_TI=1$, since they do not appear to integrate to 1? Finally, the second graph on the top line has a typo in the title: "MDT" should read "MTD". Section 3.3, page 12, line 7: Isn't the low probability of occurrence of ESD=0 and $k\_AF=0$ an input assumption? Perhaps when the meaning of the pdf

plots is more fully explained, this will become clear. Section 3.3, page 13, line 5: The "largest probability" implies total probability greater than 50% of lying within +/- 1% of the mean?

Page 13: Mostly these conclusions are justified and well-written. However, some more discussion could be given to the relative influence on the qualitative or quantitative results (i.e. differences with a deterministic approach) of the method itself, versus the specific numerical assumptions made about input parameter values, distributions and covariances.

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