

Wind Energ. Sci. Discuss., author comment AC1  
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## Reply on RC1

Rad Haghi and Curran Crawford

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Author comment on "Surrogate models for the blade element momentum aerodynamic model using non-intrusive polynomial chaos expansions" by Rad Haghi and Curran Crawford, Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-110-AC1>, 2022

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Thanks for your constructive and concise comments. Here is the reply to your comments:

### Comment 1

However, there are issues with two of those three key aspects that would need to be addressed by the authors before releasing this work. First, the curse of dimensionality is not adequately alleviated for the future purposes of this work. Selecting a simpler wind velocity model with a reduced number of inputs does not tackle the problem, just avoids it. When a more complex model is needed, the same technique presented here is not enough to tackle the challenge of dimensionality in PCEs and the work should be revisited entirely. The literature is very rich with mathematical ways of dealing with this issue (low-rank approximations, sparse PCEs etc) and these have not been explored which would be more important for the impact of this work in the field of wind engineering. Furthermore, there are no account of errors for each PCE approximation, for instance for Figure 4, computations of the L2 error norm, not just a qualitative indication.

### Reply

The authors agree with the comment about the reduced Veers model. We are aware of the challenge and will tackle this in future work. This comment is thoroughly addressed in the new revision. The reduced model showed enough accuracy in Fluck's (2016b) work in covering the variation in the unsteady wind. This reduced Veers model is not a substitute to the high fidelity Turbsim model but a necessity to study the surrogate model at this stage. The sparse PCEs were tested, and the result was not promising in terms of the required data for the Quadrature method. The full gaussian quadrature methods for 10 random variables and polynomial order 4 require 9765625 data points, which means 9765625 simulations. For the sparse setup, 10626 simulations are required. The L2 error for the point-to-point comparison is added to the revised version.

### Comment 2:

The statistical convergence is assessed using only one metric without giving enough motivation as to why that metric and what errors are committed when using it (how good of an assessment does the Hellinger distance give you? Could it give you a good

assessment even in cases that are clearly not good?). It would be interesting to round up that part of the work with another widely used metric, the Kullback-Leibler divergence, used in surrogate modeling when approximating models for Bayesian analysis, for example. Furthermore, when using more complex models, the argument of statistical convergence may not hold due to new underlying physics, so how should we take this?

Reply

The KL divergence and Hellinger distance provide similar results, as they are mathematically similar methods. The reasons to use Hellinger distance in this study are its properties and ease of implementation. The Hellinger distance metric has lower (zero) and upper (one) limits. This property makes it a proper metric for this study. This comment is reflected in the new revision of the work. The authors believe the model we implemented here is complex enough to cover the basics for statistical convergence.

Comment 3

An interesting way of enriching this work and properly tackling those three key aspects would be by adopting both the reduced Veers and the full Veers model and make a comparative study with different techniques to build the PCEs and show how you could efficiently use these techniques with a baseline model (reduced Veers) and a more complex one without having to trade some of the physical aspects.

Reply

This is indeed an interesting way and can be another paper by itself. However, the full Veers model has, if not hundreds, but thousands of random variables as the input for the unsteady wind generation. This means, considering the curse of dimensionality, the PCEs to build the surrogate models will not be helpful, and it needs other methods to build a surrogate model. We will tackle these ideas in future work. Also, I would like to refer you to the reply to your first comment.

Comment 4

Another concern to be raised is the structure of the paper. It is not well-organized and some sections are a paragraph long. Maybe a better way would be to devote section 2 entirely to the models the authors aim at approximating, with more in-depth discussion. Section 3 could be devoted to the methodology and all the different aspects. Section 4 could then discuss the results.

Reply

Thanks for bringing this to our attention. The model in this study is treated as a black box, therefore we do not see any need to go further into the details of the model. We did revise the manuscript to better organize the section and subsections and address your comment.

Comment 5

Technical corrections

Find a list of typos found (non-exhaustive):

Page 4, line 84: dimensionality

Page 4, line 97: properties are extracted

Page 6, line 123: orthogonal polynomials

Page 7, line 173: collocation  
Page 9, line 214: Hellinger  
Page 12, line 273: we use the Hellinger distance

There are also English inconsistencies throughout the text, with sentences missing verbs etc. The authors should re-read the manuscript carefully.

Reply

Thanks a lot for the technical correction. We implemented them and did correct them in the revised manuscript.