Wind Energy Sciences Paper Revisions

March 29, 2023

Dear Editor and Reviewers,

We thank the editor and the reviewers for their efforts and the time they spent reviewing our manuscript. The thorough reviews and the helpful detailed comments helped to improve our manuscript. We have made modifications according to the reviewers' comments/suggestions and colored the corresponding text in the revised manuscript. For corrections made in response to Reviewer 1 text are colored blue while corrections made in response to Reviewer 2 are colored in green. For convenience, we include below the original comments of the reviewers and the detailed responses to each reviewer's comments/suggestions. Thank you again for processing our manuscript. We hope that we responded to all comments and concerns of the reviewers and that you will consider favorably our manuscript for publication in Wind Energy Sciences.

We are looking forward to hearing from you soon.

Sincerely,

Nicholas Peters

Reviewer 1

Q 1.1 *The paper does not validate the optimal designs found via the ROM model. I suggest validating the designs using the CFD model, or providing a caveat explaining that these designs would need to be validated with CFD before being used in production.*

Reply: As the reviewer correctly highlights, the lack of validation for POD ROM identified optimal points was a significant shortcoming of the original manuscript. While the derived surrogate model in this manuscript may have compared well against training and testing points, optimal points are often found in new locations of the parameter space, especially when a large degree of complexity is present. The consideration of this increased local complexity is an important point the original manuscript failed to address. As such, for the revised manuscript five additional CFD simulations have been completed to validate the local optimal design point identified in Fig. 16 (f) of the original manuscript (currently Fig. 18 (f)). Description of these five new simulations is provided on line 151 and Table 3. Further discussion of these results can be found on page 27 starting on line 537. Additionally, the manuscript has been updated to clearly iterate the limitations of surrogate model predictions.

Q 1.2 *I strongly recommend discuss the pros and cons of how this framework could be extended via a multi-fidelity formulation (for example, on P3L60).*

Reply: In light of the reviewer's comments, the authors agree that the original manuscript lacked a sufficient warning of cation regarding the implementation of surrogate modeling in the design process. As such, the

paper has also been reworded to caution readers on the dangers of relying on surrogate models derived from limited datasets. The revised manuscript has been reworded in a way ensuring that readers realize that this is a presentation of a framework for both improving the utilization of limited sampling and informing regions of interest for future sampling. While edits have been made throughout the paper, a new paragraph has been added starting on line 121 of page 4 and is highlighted blue for the reviewer's convenience.

Q 1.3 *Throughout the paper, phrases like "highly accurate" are used in a way I perceived to be subjective. The errors between the ROM and CFD seem significant, and it seems subjective to call the ROM highly accurate, or even accurate. I would instead focus on quantitative differences between the models, and refer to errors you are happy with as "reasonably accurate".*

Reply: The authors agree with Reviewer 1 that the term 'highly accurate' is mislabeled terminology for the performance of the surrogate model, particularly in instances where surrogate model prediction error becomes 5%. Additionally, the authors agree that 'highly accurate' miscategorizes the objective of the derived surrogate model. Rather than aiming to derive a 'highly accurate' surrogate model, the objective is to derive a 'reasonably accurate' model capable of modeling key characteristics of the domain at a greatly reduced computational expense. In light of this suggestion, the wording of the manuscript has been updated to remove reference to 'highly accurate'.

Q 1.4 *Please include more detail in exactly how $\Phi_i(x)$ is constructed, since each set of blade parameters would be associated with a different set of basis functions, $\Phi_i(x,\lambda,\theta)$.*

Reply: The implementation of x in Φ_i(x) is intended to be referred to as a general state variable. As such, it was intended for the reader to understand that x refers to both spatial positions in the computational domain and parameters varied in the design sweep. However, this implementation clearly leads to ambiguity as to what parameters each mode is derived from. On line 220 page 9 of the updated manuscript, the discussion of the POD algorithm implementation has been updated to address this concern.

Q 1.5 *It would be useful to compare ROM and CFD predictions using relative error, instead of the absolute error presented in Figures 12, 13, 14, and 15.*

Reply: Figures 14 through 17 have been updated to show the relative error instead of the absolute error.

Reviewer 2

Q 2.1 *Please include additional studies on estimating wind turbine loads with LES beyond the single reference. While wind energy is not the focus of the manuscript, many recent works employ tools such as OpenFAST within a LES framework to compute rotor loads in highly turbulent flows which is of relevance.*

Reply: The authors agree with the Reviewer that improvements to the original manuscript's literature review could be made. As such, additional works have been included to extend the discussion of LES applied to computing rotor loads in highly turbulent flow fields. This extended discussion begin on page 2 starting on line 38 and has been colored green.

Q 2.2 *This section would benefit from including reduced order modeling studies specific to turbulent rotor wakes. A list to include is herein provided:...*

Reply: The authors have incorporated the majority of the requested citations into the manuscript. However, we have omitted some that are not directly relevant to the present work. The omitted citations were "Review of rotating wing dynamic stall: Experiments and flow control", "The role of surface vorticity during unsteady separation", "Assessing spacing impact on coherent features in a wind turbine array boundary layer", "Focused-based multifractal analysis of the wake in a wind turbine array utilizing proper orthogonal decomposition" which are nice works but not related to ours.

Q 2.3 *In general, there is no verification of the framework and shortcomings should be clearly highlighted. This is likely the most important point to address.*

Reply: Both reviewers have highlighted a significant shortcoming of the manuscript, the lack of validation of POD ROM optimal solutions. To reiterate from the respose to Reviewer 1, the lack of validation for POD ROM identified optimal points was a significant shortcoming of the original manuscript. While the derived surrogate model in this manuscript may have compared well against training and testing points, optimal points are often found in new locations of the parameter space, especially when a large degree of complexity is present. The consideration of this increased local complexity is an important point the original manuscript failed to address. As such, for the revised manuscript five additional CFD simulations have been completed to validate the local optimal design point identified in Fig. 16 (f) of the original manuscript (currently Fig. 18 (f)). Description of these five new simulations is provided on line 151 and Table 3. Further discussion of these results can be found on page 27 starting on line 537. Additionally, the manuscript has been updated to clearly iterate the limitations of surrogate model predictions.

Q 2.4 *It would be beneficial to include a visual of the CFD results for hover operation similar to the contours presented in Section 4.2.*

Reply: To help demonstrate how POD ROM performance predictions vary with rotor radial station, we've included line plots for local contribution to coefficient of thrust versus radial station in Figs. 7 and 9.

Q 2.5 *Figure 5, Line 305 - 325: Comment further on the relationship between the pressure snapshot and POD energy content here. The POD contains at least 99.5% energy with only two modes and achieves reconstruction errors below 1% with only eight modes which is surprising. This could stem from over-simplifying the pressure solution by only including spatial information but it is difficult to be certain without additional context.*

Reply: The Reviewer is correct, in the hovering case the solution is axially symmetric and as such we don't take into account time variations. An additional comment has been added in text to clarify this remark on line 343 page 14.

Q 2.6 *Line 404: How did the authors decide on 4.5 degrees for saving pressure distributions as opposed to saving at specified times? Does this approach introduce spatial artifacts?*

Reply: We picked 4.5 degrees as this appears to be standard for line actuator simulations. it is the author's stance that this sampling rate should provide sufficient resolution for most practical engineering design tasks. Nonetheless, the influence of varying resolution in Ψ when sampling was not addressed in this study and as such the manuscript has been updated to explicitly address this point. This added text can be found on line 451 page 22.