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Comment on wes-2021-56

Anonymous Referee #1

Referee comment on "Effectively using multifidelity optimization for wind turbine design" by John Jasa et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-56-RC1, 2021

My initial impressions is that this is a good article and that it should be accepted to this journal with some revisions.

The authors have looked at a multi-fidelity algorithm that uses additive surrogates based correction models. Furthermore, the approach does not require gradients from the high-fidelity model. The author made these choices so that the method is general purpose. The authors then applied this method to a blade design problem, controller tuning problem and finally a wind farm layout optimization problem. In all cases the authors were able to show that the multi-fidelity method was able to obtain a better solution (according to the high-fidelity analysis) than single discipline low fidelity optimization. While at the same time solving these problems faster than the single fidelity high fidelity optimization. The article is interesting and relevant work to the community and has been well written. However, there are some areas for improvement and I feel that some revisions can help make a stronger manuscript. Those are discussed as follows:

First, I have concerns about the multi-fidelity algorithm that I think the authors should comment on in greater detail. The first point of concern is that none of the multi-fidelity optimization solutions matched the high-fidelity solution. This suggests that at the limit the corrected low fidelity problem is not equivalent to the high-fidelity solution. Some corners of the multi-fidelity literature try to show that the corrected multi-fidelity algorithm is equivalent or not. It would be good for the authors to comment on whether their algorithms tries to achieve this or not. This lack of equivalence is obvious in the design solution for the blade optimization. For readers and engineers that are more concerned with the final design configuration than the objective, it would be helpful if the author could comment on that and explain to what extent the surrogate correction played a role in this discrepancy.

Also I think that this shortcoming is related to convergence of the surrogate to the high-fidelity model. The authors show some plots that qualitatively show convergence. However, it is small differences in the gradients at the limit that ultimately define an

optimal solution. I think that it would be good for the authors to show quantitatively the convergence rate of the surrogate model at the optimal high fidelity optimization point for both the solution and the gradients.

Furthermore, surrogate based methods suffer greatly from the curse of dimensionality. Despite this, I was quite pleased that the authors were able to obtain good results despite this typical weakness. To overcome this weakness the authors explain how they used a specific type of surrogate that internally performs model reduction to overcome this dimensionality. I personally would like to know more details about this. For example, do many dimensions cause the surrogate longer to start converging? How many high-fidelity evaulations were needed before good convergence was achieved? Maybe more details on the internal model reduction methods used in their surrogate would be nice.

At the end of the article in figure 10, the authors summarize the different cases purely in terms of computational savings. However, I think different readers are more interesteed in a range of metrics. Since none of the multi-fidelity solution achieved the same performance of the pure high-fidelity, I think that you also need to include this in the summary. Furthermore, sometimes it's not the objective value that matters, but the final design configuration. Thus, I think that it's also important that you discuss limitation. Finally many readers would consider the distance in the design space from the high fidelity solution an important metric. So I would also show this in the summary. Most would see the objective and final design configuration more important...

Concerning the cited literature, you have many citations of the work within wind energy. However, I think you could give a better summary of the different contributions in surrogate based optimization. This is an old topic in the aerospace community. There are a mix of different methods that have a range of speed improvements and accuracy in terms of reproducing the high fidelity solution. It would be good if you could present these different methods and explain how your work fits with these.

Another comment is the choice of problems. The IEA Task 37 has created various optimization test cases for this purpose. It would be better for the larger community if your multi-fidelty was applied to these test cases so that the community can see how your results compare with the larger community results. This is of course a big task ... but maybe this is something to consider for future articles.

In your future work discussion, I would elaborate a bit more on how this method could be improved. The multi-fidelity literature in aerospace shows faster optimization with better correspondence with the pure high fidelity solution. You made choices to improve generality and applicability, so this not a critism, but I think in light of other methods it would be good to comment on what would be needed to get closer the high fidelity solution faster.