Review of wes-2021-56
Michael Muskulus (Referee)

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-RC2, 2022

This manuscript talks about multifidelity optimization in wind energy. Now "multifidelity optimization" is a large umbrella under which many different approaches fall - things that people have been doing for years in order to speed up their optimizations, but also more recent ideas how to do this in a more structured way. The manuscripts starts with an informative review of some of the literature and recent domain-specific works that explicitly considers the multifidelity optimization idea, and this contains useful references and is valuable. The authors then quickly describe their method of choice, which is the use of trust-region optimization with a "Kriging partial least squares" surrogate model. Unfortunately no details at all are given about their particular algorithm used and what parameters or other choices have been made. This is a major weakness of this paper, especially so since trust-region optimization is not something that most people will be familiar with (even though it is extensively used with highly nonlinear problems). The used surrogate model is a modern variant of the Gaussian process model, which has received much attention recently (especially in wind energy applications), due to its intrinsic ability to estimate the uncertainty in its predictions. I would assume that this feature will be used when performing trust-region optimization with it, so this should be explained and discussed. There are many interesting options and ideas here that would interest readers, and generally these methodological details are more interesting than the shown example studies and their results. In fact, as long as the method is not fully clear, the results are not that interesting - to a scientifically-minded reader. One might be inclined to forgive the authors this lack of details here given the typical space requirements of journals (and the cost of publication), but without this information the article reads more like an advertisement for the authors' method than what was probably intended. At the very least a reference to supplementary material (e.g. in the form of a non-peer reviewed technical report - this is a prime example of why we still need and should value this type of publication!) should be given where readers can find all implementation details.
The manuscript continues to discuss three example studies where this approach - correcting a low fidelity simulation with a meta-model based on few evaluations of a high-fidelity simulation - seems to perform somewhat better than either optimization based on the low-fidelity model alone (being more accurate than this) or when using the high-fidelity model alone (being faster than this). Again, it is unfortunately completely unclear how the optimization with these models (only the low- or high-fidelity ones) is performed!

Somewhat surprisingly, the high fidelity models used in the case studies have only slightly higher complexity than the low fidelity models. For example, in the blade design study, both models are based on blade element momentum (BEM) theory. The low fidelity model is using the original steady state BEM approach, whereas the high fidelity model is using its unsteady generalization. For this pedagogic demonstration this is sufficient, but in real applications one would expect to use some kind of CFD simulations as the high fidelity model, I assume?

All in all, the topic is relevant, and the authors seem to have some success with their approach. If now they could only describe the approach with enough details that readers could reproduce it (or learn from this paper how to do something similar with their own wind energy optimization problems), then this would be a very nice contribution to the literature.

Major comments

- The authors need to explain the details of the trust-region optimization and KPLS surrogate model used, in sufficient detail that readers can (potentially) reproduce the results in the paper.

  page 4: "This approximation is devised such that it is equal to the high-fidelity model at the points where we have high-fidelity data." - So it is what we would call an interpolating approximation. Note that this property is not strictly necessary (although it usually makes sense).

- Fig. 2: It is unclear how the trust region is changing between iterations.

- Fig. 3: There seem to be samples missing in the figure (e.g. in Fig. 3a, the function depicted is not piecewise-linear between the sample on the far left and the cluster of samples in the right third). Also, are all three figures based on the same number and location of samples from the high-fidelity model?
In the blade design study, the low-fidelity optimized blade must be cheaper (less materials used) than the high-fidelity or multi-fidelity optimized blade? By how much? The comparison (Table 2) would be more informative and convincing if the optimization was using the same route (e.g. by enforcing only increases in twist and chord?) instead of converging on completely different paths (toward completely different local optima).

How the method works is intuitively clear in 1D (second case study), but what happens in higher-dimensional cases? The first case study is 7 dimensional, the third one is 14 dimensional. How does the approach work with such high-dimensional data, what are its limits or challenges with it? For example, how are the surrogate models initialized, and does this effort not become too computationally expensive in higher dimensions?

Fig. 6: Abbreviations of what simulation outputs are shown should be explained. Not every reader is so familiar with OpenFAST software that they will immediately understand what they see here.

page 6: "By not using simple additive or multiplicative factors ..." - This seems to suggest that only the last example uses a surrogate model as correction function, and the first two case studies use something simpler? Or is it just this statement that is somewhat misleading?

Where is the data availability statement (required by the journal)? Where can readers find the code and the data used for the results in this paper?