Review of manuscript: WES-2021-152
Title: Multifidelity multiobjective optimization for wake steering strategies
Authors: Quick, King, Barter & Hamlington

**Overall comments:**

The submitted manuscript proposes a methodology for multifidelity (using multiple models with different cost and accuracy) and multiobjective optimization (with multiple goals, such as high power and low fatigue) for wake steering. The paper is timely, interesting, and satisfies a need for research methods which include consideration of loads in wake steering optimization.

I have several comments and questions listed below that I would like the authors to consider in their revision.

**General comments and questions:**

1. Even a slight change in the downwind turbine location is likely to substantially change the Pareto set results. I am especially looking at the results in Section 4.2 Flow Physics Insights. It seems from Figure 6 that negative yaw misalignment underperforms positive yaw only because of a very slight overlap between the curled wake shape and the lower half of the downwind rotor. I expect small changes in the ABL shear, stability, etc. would also change the results. It would be helpful to more clearly highlight throughout the manuscript that your results are specific to ABL properties and the turbine layout considered in your test case.
2. How does this methodology scale to higher dimensional input spaces (i.e. more than two values of control inputs)? A discussion of the scaling and potential challenges that it brings would be useful to understand how this concept may perform in more realistic scenarios. Especially since this Pareto set would be unique to the wind farm layout, ABL conditions, etc (**Point #1)**. So I expect that the Pareto set would need to be uniquely computed over these independent variable input combinations (curse of dimensionality)?
3. I am wondering about two forms of uncertainty not discussed in the manuscript:
    a. Sampling uncertainty - all results are taken from CFD with finite-time averages. Does this impact your results? Relatedly, are all CFD cases started from identical initial conditions? Are your Pareto sets robust to sampling uncertainty?
    b. Meta-uncertainty - How does the meta-uncertainty over different random seeds for your initial sampling points and your initial conditions affect the output Pareto set?
4. The refined sampling points shown in Figure 5 are helpful, but further validation of the proposed methodology's ability to capture the Pareto front would be useful. Can the authors refine their grid search over the $\gamma_1$ and $\gamma_2$ space?

**Point comments:**

1. Line 5: What is meant by "unsteady LES." Is there a time-dependent boundary condition or just turbulent variations about a mean state?

2. Line 19: “A counter-rotating pair of vortices is generated by the rotating blades”
The counter-rotating pair is also shed by non-rotating turbine models [1,2], so perhaps it is unclear to say that the counter-rotating pair of vortices is generated by ‘the rotating blades’, but rather ‘the yawed rotor’. The rotating blades do also affect the dynamics of the counter-rotating vortex pair [1].
3. Line 28: “Damiani et al. (2018) performed a detailed analysis of a single wind turbine, noting that negative yaw offsets tended to increase fatigue loading more than positive yaw offsets.”
The primary conclusions of Damiani et al. (2018) are that the loading depends on the site conditions (e.g. shear) and turbine model. From the referenced paper conclusions: “On average, the blade-root bending moment DEL decreased for positive yaw offsets and increased for negative offsets. Fairly large variations can be attributed to different turbulence seeds and data records, making generalization more difficult.” It is worth providing that context in your statement, since the result of negative yaw leading to more fatigue than positive yaw will not always hold.
4. Line 36: “While engineering wake models are remarkably accurate in power prediction [...]”
I am not sure the subjective descriptor “remarkably accurate” is useful or true. What level of accuracy is remarkable? Wake models exhibit predictive error in many applications.
5. Line 63: Here you state that the objective is always minimization in this section (Section 2) but subsections 2.1 and 2.2 use maximization objectives.
6. Equation 1: What is the dimensionality of γ? Is it of size = number of turbines?
7. Line 66: Here $f_i$ is not defined, is that intentional? I was not sure if $g_i(\gamma)$ is emulating $f_i$ directly or is related to $f_i$ somehow through an objective function?
8. Line 73: Related to the note above, on this line, $f_i(\gamma)$ is referred to as an “output” instead of an “objective function.” This confusion comes up a few times later in the paper so it is worth clarifying explicitly here.
9. Equation 4: It would be useful to add a validation of the GP model used.
10. Equation 9: Define Pareto dominance
11. Equation 12: $r_1$ and $r_2$ are not defined, I assume they are the coordinates of the reference point in two dimensional space?
12. Section 2.2: The difference between $N$ and $l$ isn’t clear in this section
13. Line 155: “No matter which fidelity is to be sampled next, the ultimate goal is to minimize the highest-fidelity function, [...]”
This is confusing. Here you mean the goal is to minimize the highest fidelity function meaning $f$? But to do that you maximize the objective function J ($f$ also being a different objective function).
14. Equation 18: So there is a different objective J for each model fidelity? That is not clear in this section if so. Also, it is a little confusing to have both J and f as objective functions.
15. Line 180: Is the turbine nacelle or tower included? This has been noted to affect CVP dynamics [3] so what the authors are using should be stated.
16. Figure 1:
    a. The comparative step between the high and low fidelity objectives in the workflow is not explained in the text.
    b. LF and HR (presumably high and low fidelity) not defined.

17. Figure 2: The 'low-fidelity model' looks like it has unphysical grid-to-grid oscillations in the output. How do these unphysical CFD errors impact your results?
18. Line 226: I am puzzled by the authors' choice of normalization to have the objectives be in the same order of magnitude. The choice seems *ad hoc*. Why not use a more precise transformation to ensure they are more directly comparable (e.g. standardization transform). A multiobjective objective function composed of two different units (MW and Nm) seems strange.
19. Line 241: Why will random sampling 'drastically affect the optimization.' What do you mean by 'drastically'? The authors could (perhaps should) account for meta-uncertainty by testing the results over several initialization realizations.
20. Line 243: Missing degree symbol
21. Section 3.3.2 should be mentioned earlier, perhaps in an outline introduction to Section 3. It was confusing as written. Several questions came to mind:
    a. How did the authors specify that 0.89 correlation is sufficiently high while 0.74 (correlation between HF DEL and LF DEL) is not?
    b. Does this correlation depend on the yaw misalignment? In the introduction, the authors stated that the bending moments depend on yaw.
    c. I anticipate that this will depend on the inflow conditions as well, so I am wondering how this method could be used in practice.
22. Section 3.3.2: I am wondering what these results suggest about the approach of 'low-fidelity loads modeling.' It would be helpful to more clearly discuss why the low-fidelity model fails to capture the fatigue. Is the turbulence in the low-fidelity model insufficiently resolved such that it misses the effect of turbulence on the loading?
23. Equation 28: Is the DEL function missing here? In Equation 27, L = DEL(M), not just M.
24. Figure 7:
    a. This figure is very small, please increase the size
    b. I found it to be confusing that the wake deficit increase from x/D=6 to x/D=8, but that is because the downwind turbine is at x/D=7. That should be made more clear in the figure. I am not sure what I am supposed to learn from the x/D=8 contours.
25. Figure 8: Likewise, this figure is small and has many lines. Hard to see.
26. Line 334: "A positive front turbine yaw offset is more effective at reducing loading and increasing power than a negative yaw offset because the counter-rotating vortices produce a greater velocity deficit in the downstream wake."
    I believe this sentence needs to be re-phrased. The authors meant to say that positive yaw leads to <u>less</u> velocity deficit in the wake region (at least the wake region where the downwind turbine is located).

**References**

[1] Howland, Michael F., Juliaan Bossuyt, Luis A. Martínez-Tossas, Johan Meyers, and Charles Meneveau. "Wake structure in actuator disk models of wind turbines in yaw under uniform inflow conditions." *Journal of Renewable and Sustainable Energy* 8, no. 4 (2016): 043301.

[2] Shapiro, Carl R., Dennice F. Gayme, and Charles Meneveau. "Modelling yawed wind turbine wakes: a lifting line approach." *Journal of Fluid Mechanics* 841 (2018).

[3] Zong, Haohua, and Fernando Porté-Agel. "A point vortex transportation model for yawed wind turbine wakes." *Journal of Fluid Mechanics* 890 (2020).