

Interactive comment on “Set-point optimization in wind farms to mitigate effects of flow blockage induced by gravity waves” by Luca Lanzilao and Johan Meyers

Anonymous Referee #2

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Interesting paper and concept. Contains every detail of the simulation but requires a solid background in fluid dynamics to understand. The latter is directly a disclaimer since I don't have a solid background in this field. While reading the paper I have the following comments:

1) The authors state on pg. 11 line 280: “However, all optimal are constant in time. . . . And conclude that unsteady time-periodic excitation is less effective” I believe that this claim is too strong. There is no guarantee that the optimizer will find the optimal solution. The optimizer finds a solution under the specified constraints, and that is what the authors present. This only has value if this gives rise to a better under-

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standing of the physics. Now it is one of many possible solutions (local minima). The authors should consider rephrasing these claims to the assumptions made throughout the derivation.

2) The significance of the paper is also a bit unclear. In the conclusion the authors state that an optimization model was applied for set-point optimization. Many approximations have been made in the modelling step and there is no quantification of the potential error. The energy gains mentioned in the abstract are incredible high. I would like to see a validation of the model or the results applied to a high(er) validity model.

3) Generally, assumptions should be stated clearly. For example, the wakes between the turbines are not explicitly modelled. This is a large assumption to make, and is only briefly mentioned in the text. What is the expected impact on the results? How does it affect the conclusions drawn in the article? Also, the sampling time seems rather large for typical wind farm control algorithms. How does this impact your results? Would you be able to find a periodic optimal signal if you had a shorter sampling time? How about the fidelity of your rotor model – would things change with an ALM model?

4) The article is long, making it cumbersome to read. Perhaps certain parts can be omitted. For example, is the model from section 2.1 a novel contribution or is it identical to the one described in Allaerts & Meyers 2019? If the latter, consider removing it from this article.

5) Figure 1: It seems as if you have very few iterations before convergence. Can you comment on this?

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