Response to comments of Anonymous Referee #2

- Interesting paper and concept. Contains every detail of the simulation but requires a solid background in fluid dynamics to understand.
  We would like to thank the referee for the constructive feedback in improving the quality of the paper.

- 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 understanding 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.
  We fully agree with the referee’s statement. Therefore, the following paragraph has been modified at P12-L307:

  “*The optimization model described in Section 2.2 is time and space dependent. Hence, the model is capable of finding a time-periodic optimal thrust-coefficient distribution over the wind-farm area in a fixed time interval $[0, T]$. However, all optimal thrust set-point distributions found for the different combinations of time horizons and time steps reported in Table 1, are constant in time. We have verified this using a range of steady and unsteady starting conditions for C_T in the algorithm, but did not find any unsteady optimum. We believe that this is due to two reasons. Firstly, we use steady-state inflow conditions, therefore neglecting meso-scale temporal variations in the velocity field (these could lead to time-dependent optimal control signals, but are not included in the current work). Secondly, the objective function is non-convex and there is no proof about the uniqueness of global minima. Hence, there is no guarantee that the optimal solution found by the optimizer corresponds to a global optimum. Nevertheless, since we do not observe any unsteady behaviour in our optimal solutions, we show only steady-state results in the remainder of the manuscript, and conclude for the time being that unsteady time-periodic excitation is less effective than a stationary spatially optimal distribution in this context.*
  We also note that our findings are in contrast with recent works of Goit and Meyers (2015), Munters and Meyers (2018) and Frederik et al. (2020), in which the authors illustrated the benefits of dynamic induction control over yaw and static induction control. However, the characteristic time scale of gravity-wave effects is estimated to be approximately 1 h (Gill 1982, Allaerts and Meyers 2019) which is an order of magnitude above the typical time scale of wake convection between turbines, and turbulent mixing in turbine wakes (this also justifies the larger sampling time used). Hence, while unsteadiness of the thrust coefficient (with a typical time scale of 50 seconds for large scale turbines) can lead to improved wake mixing (Goit and Meyers (2015), Munters and Meyers (2018), Frederik et al (2020)), it has no impact on phenomena that occur at larger time scales, such as wind-farm induced gravity waves.”

- 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.
  We agree with the referee that we did not talk about the model validation. The constraints of our optimization model (the state equations) correspond to the same model derived by

Allaerts and Meyers (2019). This model has been validated in Allaerts and Meyers (2019) (see Section 3, VAL2) against LES results. The validation showed that the three-layer model outperforms the Smith (2010) model and agrees well with LES results for low perturbation values. To include this in the article, the following paragraph has been added at the end of section 2.1 (P6-L155):

"*The three-layer model configuration described above has been validated against LES results by Allaerts and Meyers (2019) (see Section 3 VAL2) on a two dimensional (x-z) domain (i.e., all spanwise derivatives are set to zero). The model shows a mean absolute error (MAE) of 1.3% and 1.8% in terms of maximum displacement of the inversion layer and maximum pressure disturbance, respectively. Moreover, the model underestimates the velocity over the wind-farm area with a MAE of 5.6%. Note that the three-layer model is a linearized model, hence the discrepancies with LES results increase with increasing perturbation values. In fact, the model agrees very well with LES data when perturbations are small (i.e, when non-linear effects are negligible). For further details, we refer to Allaerts and Meyers (2019).*"

Before to apply the results obtained to a higher validity model (i.e, our in-house LES solver SP-Wind), we need to improve the LES setup, which is what we will do in the near future. However, we did not mention it in the article, therefore we have added the following sentence to the last paragraph of the conclusions (P22-L531):

"*In the future, we also plan to apply the results obtained in this article to a higher fidelity model (i.e, our in-house LES solver SP-Wind). However, this requires some work on the efficiency of non-reflecting boundary conditions in our LES solver (Allaerts and Meyers 2017, 2018).*"

Finally, to avoid mentioning only the highest energy gain found in the abstract , we have modified the last sentence to (P1-L16):

"*Overall, energy gains above 4% were observed for 77% of the cases with peaks up to 14% for weakly stratified atmospheres in critical flow regimes.*"

- 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?
  We agree with the referee that the sentence "the wakes between the turbines are not explicitly modelled" is misleading. To model the farm drag force, we use a box-function wind-farm force model (also used in Smith (2010) and Allaerts and Meyers (2019)) which uniformly spreads the force over the simulation cells in the wind-farm area and does not represent the disturbances caused by each turbine in detail. The force magnitude depends on the wind-farm layout (see parameter β), the wind speed, and the thrust-coefficient distribution (i.e., the C_T value in every grid cell within the farm). To avoid confusion, we have modified the sentence to (P4-L111):

  "*We use a box-function wind-farm force model similar to Smith (2010) in our study. This al-*

lows us to avoid the complexity of wake models while gaining in computational time. In fact, this model uniformly spreads the force over the simulation cells in the wind-farm area and does not represent the disturbances caused by each turbine in detail. The force magnitude depends on the wind-farm layout, the wind speed and the thrust set-point distribution (i.e., the C_T value in every grid cell within the farm)."

To understand how this simple wind-farm model affects the conclusion drawn in the article, it is useful to compare the three-layer model predictions using both the model previously discussed and the Gaussian wake model, which are shown in Allaerts and Meyers (2019) (VAL2 and VAL3, respectively). Although the magnitude of the predictions are slightly different, the trends are unchanged. Hence, we expect that trends may remain the same if a more accurate force model would be used. Nevertheless, the accuracy of the results could benefit from an improved force model (this also answer to the question regarding the ALM). Future work needs to focus on further improving the model, as well as on validation.

Regarding the question on the sampling time, gravity waves have a different time scale than wake convection, which justifies the larger sampling time used. We have added this consideration in Section 4.1 (see second comment). We have also tried to use smaller sampling time (i.e, down to a couple of seconds) but the optimizer has never found an unsteady optimum.

- 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 and Meyers 2019? If the latter, consider removing it from this article.
  The model described in section 2.1 is similar to the one discussed in Allaerts and Meyers (2019), but nevertheless, we have added the time dependency to the equations, a different equations' form is used, and the wind-farm force model is different. Therefore, we believe that a brief explanation of the model equations makes the article more understandable (the model description occupies approximately a page and a half, excluding the wind-farm force model). This is the reason why we decided to include this section in the article. However, to reduce the length of the section (and of the article in general), we have deleted some unnecessary sentences and explanations from the text.

- 5) Figure 1: It seems as if you have very few iterations before convergence. Can you comment on this?
  Figure 1 shows that the cost function decreases rapidly in the firsts two to three iterations, reaching convergence after approximately 5 algorithm iterations. The use of a quasi-Newton method in combination with the limited complexity of our optimization model (for instance, the constraints are linearized equations) allow us to reach such a fast convergence (e.g., note that a Newton method reaches convergence in one step for a classical convex QP, i.e. convex quadratic cost function with linear constraints). Moreover, the continuous adjoint method limits the number of function evaluations, since it is not necessary to evaluate $\tilde{\mathfrak{J}}(C_T + \alpha\delta C_T)$ for all directions δC_T in the control space (at the expenses of solving an auxiliary set of equations). Based on this, we were not surprised in reaching convergence after 6 L-BFGS iterations with only 20 function evaluations. To include these considerations in the article, we have added the following sentence in section 3.1 (P9-L246):

  "*Fig. 1 shows that the cost function decreases rapidly in the firsts two to three algorithm iterations, reaching a plateau afterwards. The use of a quasi-Newton method in combination with the limited complexity of our optimization model (for instance, the constraints are* *linearized equations) allow us to reach such a fast convergence. Moreover, the continuous adjoint method limits the number of function evaluations, since it is not necessary to evaluate* $\tilde{\mathfrak{J}}(C_T + \alpha\delta C_T)$ *for all directions* δC_T *in the control space (at the expenses of solving an auxiliary set of equations).*"