Response to comments of Alan Wai Hou Lio

- The paper is interesting. The authors investigated the problems of the mesoscale interaction between a wind farm and atmospheric boundary-layer. A three-layer model is proposed for modelling the wind-farm induced gravity wave. Based on the simplified model, optimisation is then proposed to find the optimal thrust coefficient distributions that maximise the wind farm power output. The concept is appealing and the topic is definitely relevant to the wind energy community.
  We would like to thank the referee for the constructive feedback in improving the quality of the paper.

- Comments: pg1: Some claims by the authors were not clear. For example, the optimal thrust coefficient distributions are spatially stationary rather than time-periodic. Did the authors consider turbulent wind inflow and turbine-to-turbine interactions? Does the claim imply that stationary spatial distributions of thrust coefficients are better than dynamically changing the thrust set-points for maximising the wind farm power? This claim disagreed with some of the other works (e.g. [1]). In [1], the benefit of periodic dynamic induction control was shown, where the thrust coefficient of the upstream turbine was periodically adjusted to improve the downstream wind flow. How is this work related to [1]?
  The referee is right in saying that our claims disagree with some previous works. Although our study deals with much larger scales than the ones usually considered in dynamic or static induction control, we believe that it is useful to relate it with other recent findings. Hence, 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."

- pg2 l59: “asses” -> asks.
  Thank you, we have corrected this erratum.

- pg4 l116: Ct is a function of Ct(x,y,t)? What is x and y in B(x,y)?
  To avoid the influence of the wind-farm layout on the results presented, the wind profile is always oriented along the x-axis. Hence, the x- and y-axis denote the streamwise and spanwise direction, respectively. The function $B(x, y)$ is a box function equal to one for the (x, y) coordinates within the wind-farm area and zero elsewhere. Similarly, the thrust-coefficient distribution $C_{\mathrm{T}}(x, y, t)$ is function of the spatial coordinate x and y and of the time t. The thrust- and power-coefficient distribution are always multiplied by the box function $B(x, y)$ in the text, since they are defined only within the wind-farm area.The following sentence has been added at P5-L122:

  “*the x- and y-axis denote the streamwise and spanwise direction, respectively.*”

- pg5 l123: What is the dimension of Ct? Is Ct a vector where the number of elements in that vector is equal to the number of turbines? How is Ct of each turbine related the aggregate wind farm drag f?
  $C_T = C_T(x, y, t)$ denotes the thrust set-point distribution and can be represented mathematically as $C_T : \mathbb{R}^2 \times [0, T] \to \mathbb{R}$. In the text, the thrust-coefficient distribution is always multiplied by the box function $B(x, y)$ (see previous comment) so that it assumes non-zero values only within the wind-farm area. If we denote with N_x^{wf} and N_y^{wf} the number of grid points within the farm along the x- and y-direction, $N_x^{\mathrm{wf}} N_y^{\mathrm{wf}}$ represents the number of grid cells in the farm area. We assume a constant C_T value in every cell, and each of these values represent a control parameter of our optimization problem. Hence, the number of control parameters is given by $N_x^{\mathrm{wf}} N_y^{\mathrm{wf}} N_t$ (this is mentioned in the article at P7-L190).

  In regards to the last question, note that we select the turbine spacings s_x and s_y to have a density of turbines in the farm similar to the one of Allaerts and Meyers (2019) (i.e. leading to $\beta = 0.01$ in Eq. 8, using $\eta_w = 0.9$ and $\gamma = 0.9$ similar to Allaerts and Meyers (2018)). Hence, we fix the density of turbine in the farm but we do not specifically define a layout or a number of turbines. In fact, the force model uniformly spreads the force over the simulation cells in the wind-farm area and the number of grid cells within the farm define the DOF of our optimization problem (as mentioned above). The turbine spacings (together with other parameters) only define the wind-farm drag-force magnitude. In order to compute the thrust-coefficient $\tilde{C}_{T,k}(t)$ of a turbine at location (x_k, y_k), it is possible to evaluate the thrust coefficient distribution $C_T(x_k, y_k, t)$. A more accurate connection between $\tilde{C}_{T,k}(t)$ and the drag force $\boldsymbol{f}$ would require the use of an analytical wind-farm model, but this is out of the scope of the current work. To include these information in the text, we have added the following sentence at P5-L127:

  “*Finally, $C_T(x, y, t)$ represents the thrust-coefficient distribution. To compute the thrust coefficient $\tilde{C}_{T,k}(t)$ of a turbine at location (x_k, y_k), it is possible to evaluate the thrust set-point distribution $C_T(x_k, y_k, t)$. A more accurate connection between $\tilde{C}_{T,k}(t)$ and the drag force $\boldsymbol{f}$ would require the use of an analytical wind-farm model, but this is out of the scope of the current work.*”

  Moreover, we have also mentioned at P10-L267 that:

  “*The wind turbine relative spacings along the x- and y-direction are $s_x = s_y = 5.61$ (both non-dimensionalized with respect to the turbine rotor diameter D), so that the density of* *turbines in the farm is similar to the one of Allaerts and Meyers (2019) (i.e. leading to $\beta = 0.01$ in Eq. 8, setting both the wake efficiency η_w and γ to 0.9 as in Allaerts and Meyers (2018)). Note that we do not define a specific layout or a number of turbines but we only fix the density of turbines in the farm.*"

- p5 l124: "the thrust-coefficient distribution Ct has to be interpreted as a perturbation." Is Ct the thrust coefficient or the perturbation to the thrust coefficient?
  The equations are linearized with respect to the background state variables, that is around a state for which the wind-farm is not operating. Hence, there is no distinction between the thrust coefficient C_{T} and its perturbation C'_{T} . In fact, $C_{\mathrm{T}} = C^{\mathrm{b}}_{\mathrm{T}} + C'_{\mathrm{T}}$ but we are linearizing around a non-operating wind farm, therefore $C^{\mathrm{b}}_{\mathrm{T}} = 0$ and $C_{\mathrm{T}} = C'_{\mathrm{T}}$. However, in an attempt to reduce the length of the article (suggested by the Anonymous Referee #2), we have decided to remove this sentence from the text (a similar explanation is already written in Allaerts and Meyers (2019)).

- "The goal of the optimization framework is to find a time-periodic optimal thrust-coefficient distribution". Why did the authors assume that the optimal thrust distribution would be time-periodic in the beginning?
  We are using steady environmental conditions. Hence, if we use a control signal for a finite time window $[0, T]$, since nothing changes in the atmospheric conditions, the only option is that the signal repeats itself (at least if we want to arrive at a control that is on average steady) in the time window $[T, 2T]$, and so on. To include this information in the article, we have modified the text as follows (P6-L163):

  "*The goal of the optimization framework is to find a time-periodic optimal thrust-coefficient distribution $C^{\mathrm{O}}_{T}(x, y, t)$ that minimizes the gravity-wave induced blockage effects, maximizing the flow wind speed and consequently the wind-farm energy extraction over a selected time period T. The background atmospheric state is presumed to be steady, which is the reason why we use a time-periodic control (i.e. leading to a moving time average of the optimal control that is steady, and does not lead to end-of-time effects).*"

- p6: Equation (13), what is ψ and J in (13) is not a function of Ct. I suggest the authors swap equation (13) and (14) for clarity.
  ψ is the vector containing the state variables and is defined in P7-L189.
  Correction (P6-L169):

  "*We have swapped equation (13) with equation (14) as suggested by the referee. Hopefully this will help in clarifying the dependence of J on C_{T}.*"

- p7 l183: $\mathcal{N}(\psi(C_t), C_t) = 0$. Is this only valid around the neighbourhood of the solution? What is $\mathcal{N}$?
  The vector ψ contains the state variables, hence it depends upon the thrust coefficient distribution. We denote with $\psi(C_T)$ the solution of the state equation. $\mathcal{N}\big(\psi, C_T\big)$ is an operator which represents the state equations. If $\psi(C_T)$ is a solution, then $\mathcal{N}\big(\psi(C_T), C_T\big)$=0. This does not hold in the neighbourhood of the solution but only for $\psi(C_T)$ which satisfies the state equations. The sentence has been changed to (P7-L195):

  "*To avoid exploring the entire feasibility region, we require $\psi(C_T)$ to be the solution of the state equations throughout the optimization process. In other words, defining an operator $\mathcal{N}\big(\psi, C_T\big)$ that denotes the state equations, we are enforcing $\mathcal{N}\big(\psi(C_T), C_T\big)= 0$ during optimization iterations.*"

- p11 l268: what is P_N?
  The non-dimensional number P_N is defined as $P_N = U_B^2/NH\|\boldsymbol{U}_g\|$ where N is the Brunt-Väisälä frequency. This number is an indicator of the effects of internal waves in the troposphere. For instance, low P_N values correspond to strongly stratified atmosphere which in turn implies strong excitation of internal waves. The following sentence has been added (P12-L298):

  "*Further, P_N expresses the impact of internal waves in the troposphere which increases when P_N decreases. The background state defined in Table 1 leads to $P_N = 1.92$.*"