1 Response to comments of Katherine Dykes

- Very interesing study but could be improved quite a bit through a stronger introduction that helps motivate and contextualize the issue further. A major weakness seems to be the overall set up and architecture of the optimization (the problem formulation). One issue is the ad hoc case study selection. That can be reasoned away to an extent and the authors have sought to do so- not as well as I would like but well enough. More importantly, though, if I understand correctly, inherent in the optimization of the thrust coefficients are flow effects both having to do with the atmospheric effects and the wake effects. Thus, there are multiple physical phenomena driving the results and these need to be disentangled at least in explanation if not in the analysis itself. I may be mistaken on this, in which case please clarify further in the paper on this front.
  We would like to thank the editor for the very constructive feedback. We will address the questions and requests below.

Introduction

- Consider a more fundamental definition and description of gravity waves for those who are not familiar with the concept. A more general description and then the concept particularly applied to wind energy induced phenomena
  To provide a better definition and description of gravity waves, we have added the following paragraph at P1-L24:

  "In a stable atmosphere, an air parcel which is vertically perturbed will have the tendency to fall back to its original position. In such case, an oscillation is initiated that is driven by gravity and inertia; this is called a gravity wave. Mountains are examples of orographic obstacles that trigger vertical flow displacement, and consequently gravity waves (Smith, 1980). The drag force exerted by the mountain is usually transported upward by these waves. At the point of breakdown, the drag force is released in the upper levels of the atmosphere, causing a slow down of the large-scale flow (Eliassen and Palm 1960; Durran, 1990). Moreover, when air is lifted in a stable atmosphere, a cold anomaly is created, which induces horizontal pressure gradients (Smith, 2010).
  In a wind farm, the upward displacement of the boundary layer, caused by diverging fluid streamlines due to flow deceleration by the turbines, can trigger gravity waves in the stable free atmosphere above the boundary layer as well (Smith, 2010; Allaerts and Meyers, 2017). As a result, an adverse pressure gradient develops in the induction region of the wind farm, which slows down the wind-farm inflow velocity (Allaerts and Meyers, 2018). The size of this region scales with the length of the farm. This phenomenon is one possible cause of flow blockage. Note that it differs from classical hydrodynamic blockage caused by the turbine induction, which typically scales with the turbine rotor diameter, and which has also been studied recently in much detail (Bleeg et al., 2018; Segalini and Dahlberg, 2019). "

- Thrust coefficient manipulation is an intermediate effect that is brought about by wind farm control. The distinction should be addressed
  To clarify our approach, we have added the following sentences at P3-L72:

  "Note that we do not use the tip-speed ratio and/or the pitch angle distribution as control parameters. Instead, we directly control the thrust set-point distribution. In fact, the former approach would not add further insight in the study performed in the current manuscript."

- Generally, the introduction seems to jump into details without enough context
  To add more context in the introduction, we have included a more fundamental description of gravity waves and a more consistent literature review about wind-farm control (see also below).

- Lines 44-48 – the way it is written, the concepts of blockage and gravity waves are being confounded
  Gravity waves are one possible cause of flow blockage in the induction region of a wind farm. In order to clarify this, we have added the following sentences at P2-L33:

  "As a result, an adverse pressure gradient develops in the induction region of the wind farm, which slows down the wind-farm inflow velocity (Allaerts and Meyers, 2018). The size of this region scales with the length of the farm. This phenomenon is one possible cause of flow blockage. Note that it differs from classical hydrodynamic blockage caused by the turbine induction, which typically scales with the turbine rotor diameter, and which has also been studied recently in much detail (Bleeg et al., 2018; Segalini and Dahlberg, 2019)."

- Literature review on wind farm control is weak. There is a lot more work in the space including the comprehensive review article from 2019
  We agree with the statement made by the editor. Hence, we have written differently this paragraph. You can find the revised version here below or at P3-L61:

  "In the last decades, a considerable amount of research has focused on wind-farm control strategies that allow to maximize the farm power output. We refer to Kheirabadi and Nagamune (2019) for a recent comprehensive overview. However, earlier studies all focus on influencing wake dynamics and wake mixing, which occur at a much smaller scale than wind-farm induced gravity waves, to improve power extraction in waked turbines. Important control mechanisms include wake redirection (by yawing and tilting of the turbine), and turbine de-rating strategies. Control actions that influence wind-farm physics on a much large scale, such as self-induced gravity waves, are not explored to date."

- Wind farm layout role in production is weak – there is a vast literature on optimization around wind turbine spacing considering multidisciplinary concerns with AEP a very large subset of said literature. I'm not even sure why this topic is thrown in here unless layout optimization is a consideration in this paper
  Layout optimization is not considered in the current article. Therefore, we have removed from the text the following sentences:

  "~~The wind-farm layout also plays a crucial role in power extraction. Meyers and Meneveau (2012) and Stevens (2016) used optimization tools to find the optimal turbine spacing in fully-developed and finite-sized wind farm, respectively.~~"

- The discussion in the introduction of the methodology proposed doesn't address any validation – this is done in section 2 but could be done here as well.
  We presume that the editor is suggesting to add comments about validation in the introduction of section 2 (rather than at the end of the section). This is a good suggestion. Consequently, we have moved up the discussion about model validation (P4-L98). Also, as suggested, we have extended this discussion (see comment below).

- In general, the introduction feels a bit incomplete – insufficiently motivated, insufficient description of concepts and insufficient discussion of prior art and how this work uniquely extend from it
  To improve the readability of this section, and consequently of the overall manuscript, we have included a more fundamental description of gravity waves. Next, we have modified the text so that the concept of flow blockage is not confounded with gravity waves. Moreover, we have written differently the paragraph about wind-farm control. Finally, to explain how our work differ from others in literature, we have added the following sentences at P3-L65:

  "... Control actions that influence wind-farm physics on a much large scale, such as self-induced gravity waves, are not explored to date. In the current work, we concentrate on using wind-farm control to alter/improve the interaction between the wind farm and its self-induced gravity wave system. To this end, ..."

Methodology

- For the validation of the three-layer model, the discussion in lines 155-161 seems quite limited. Is there anything more that can be said about the reasons for the error and underestimation of velocity beyond generic model fidelity arguments? How will these errors be expected to affect the current optimization study?
  We understand the concern of the editor. In general, the underestimation in our model is a result from the linearization. The smaller the perturbation, the better the model can potentially fit reality, but this will require further experimental validation in the future. From this perspective, it may be expected that errors decrease slightly in optimized settings in which displacement magnitudes are typically lower. In the manuscript, we have modified the following paragraph at P4-L98:

  "The three-layer model 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). From this perspective, it may be expected that errors decrease slightly in optimized settings in which perturbation magnitudes are typically lower. For further details about model validation, we refer to Allaerts and Meyers (2019)."

- At the beginning of section 2.2, consider adding a general discussion of how the thrust affects induction and interaction with gravity-wave blockage... this could also be brought forth in the introduction
  Thank you for the suggestion. We have added the following discussion at the end of the first paragraph of section 2.2 (P6-L182):

  "Note that the relation between overall wind-farm drag and wind-farm blockage is non-trivial. On the one hand, increased wind-farm drag leads to increased wind-farm blockage induced by gravity waves. This results from mass conservation and the upward displacement of the free atmosphere. On the other hand, increased wind-farm blockage reduces wind-farm drag. Thus, the aim of the optimization is to find the optimal balance between these two opposing trends."

Numerical setup and case description

- The description of the computational costs is loose and could be stronger and tabularized in terms of function evaluations, etc
  In the article, we only give an idea of the time required to compute the optimum. This was our aim. Looking back, we could have run the optimization algorithm with different initial conditions and different input parameters and further tabularize the function evaluation cost and the total simulation time. However, this work was done a year ago and, in the meantime, we have speeded-up the equation solver of several order of magnitude (as mentioned in the text). Therefore, we have decided to not add further information on this aspect. In a future work, we will make sure to provide a more detailed computational cost analysis.

- Typo line 246 firsts
  Thank you. We have corrected the erratum.

- The choice of wind-farm layout / case is not well justified. Generally, it would be good to see a two-fold approach where a smaller illustrative case is used to explore the effects of the drivers on the optimization problem and then the application in a larger case study. The ad hoc nature of using a large case leaves in question the generalizability of the results (the handling of the atmospheric states seems more in line with an approach to explore drivers under different conditions)
  To better justify the choice of the wind-farm layout, we have added the following sentences at P10-L281:

  "Allaerts and Meyers (2019) conducted a sensitivity study on the effects of wind-farm layout on gravity-wave induced power losses. They show that these power losses monotonically increase with the size of the farm. Also, they mention that the losses are maximum when the wind-farm ratio L_y/L_x is close to $3/2$, while being negligible for very wide but short farm, and vice versa (i.e., negligible for $L_y/L_x \ll 1$ and $L_x/L_y \gg 1$). Since we are interested in optimal thrust coefficient distributions in presence of gravity waves, we have selected the "worst-case" wind-farm layout (i.e., a wind-farm width and length of $L_y = 30$ km and $L_x = 20$ km, respectively). We note that this was also the farm layout chosen by Allaerts and Meyers (2018, 2019), which resembles in size the Belgian-Dutch wind-farm offshore cluster located in the North Sea, but simplified to a rectangular shaped wind-farm. Also, Smith (2010), Fitch et al. (2012) and Wu and Porté-Agel (2017) have used a farm with similar dimensions in their studies."

Results and discussion

- Line 314 – uniqueness is wrong word. You are not guaranteed with your optimization approaches of finding a global optima for a nonconvex problem such as this. That is certainly true. However this seems to be an odd argument for rationalizing the fact that there is not an unsteady optimum... (the latter point about time-scales seems much more relevant. I recommend striking the entire local/global discussion at least in this context)
  We have tried to give three different reasoning to the fact that we do not observe any unsteady behaviour in our optimal solutions. However, as recommended, we have deleted the local/global discussion, i.e., the following sentences:

  "~~The objective function is non-convex and there is no proof about the uniqueness of global~~

Table 2: Relative change in percentage between optimal and reference maximum flow perturbation values. Power gains are also included.

	$\mathbf{F_r = 0.9}$	$\mathbf{F_r = 1.1}$
Maximum inversion-layer displacement	-14.5%	-16.8%
Maximum pressure perturbation	-14.3%	-16.2%
Maximum velocity perturbation	-13.4%	-15.5%
Power gain	5.3%	7.0%

~~minima. Hence, there is no guarantee that the optimal solution found by the optimizer corresponds to a global optimum.~~"

- Figure 3 seems a bit disconnected. Fig 2 was nice but it would be nice to show some sort of relative effect on the inversion-layer displacement after the optimal Ct setpoints are found.
  We have organized section 4.1 as follows. First, in Fig. 2, we show the three-layer model predictions using the reference thrust coefficient distribution (i.e. $C_{\mathrm{T}}^{\mathrm{R}}(x, y) = C_{\mathrm{T}}^{\mathrm{Betz}} = 8/9$). Then, the optimal thrust coefficient distributions for both sub- and super-critical flow are illustrated in Fig. 3. Finally, in Fig. 4 and 5, we compare the inversion-layer displacement, pressure and velocity perturbations computed with the three-layer model using the reference and optimal C_{T} distributions. These effectively contain the comparisons that the editor is asking for. In these figures, we show the results as line plots along the center line of the wind farm. We do not show planforms of the inversion-layer displacement etc, since we observed that this type of plot does not visualize the differences very well.

- Section 4.1 could be strengthened by a summary table of key statistics for each of the cases...
  We agree with the editor's statement. Hence, we have added a table in section 4.1, which summarizes some key statistics. For simplicity, we have reported the table here– see Table 2.

- The language around the resulting optima is strange. You discuss sinusoidal behavior of the setpoints which is an odd way of saying that there is periodic pattern in subsequent rows of turbines in terms of the optimal setpoints. Try to tie this back more to the reality of what is going on with the turbines. These aren't mathematical features in a CFD world, these are turbines in a farm. Each turbine is a unique entity with a vector of design variables for its Ct setpoint over time
  We understand the remarks of the editor. However, as mentioned in section 2.1, the farm drag model used does not represent turbines explicitly. Therefore, we cannot rephrase the sentences as suggested. To give this kind of interpretation to the results, we would, e.g., need to couple an analytical wind-farm wake model to the gravity wave model. Then, each turbine would be a unique identity with a vector of design variables (e.g., D, z_{hub}, C_{T}, etc..) associated. In the manuscript, we tried to better explain this at P5-L145:

  "A more accurate connection between $\widetilde{C}_{T,k}(t)$ and the drag force $\boldsymbol{f}$ would, e.g., require the use of an analytical wind-farm wake model. This is however not considered in the current work, so that wake effects are not explicitly incorporated in the optimization. Rather, we consider the optimization of the gravity-wave system, while presuming that the wake efficiency parameter η_{w} does not change as a result of the optimization."

Finally, note that representing turbines as a smoothed thrust distribution that is spread out throughout the farm is, e.g, quite common in regional climate models (in which multiple turbines can fall in a computational cell). A similar force model has also been used before in gravity-wave studies by Allaerts and Meyers (2018, 2019) and in Smith (2010)).

- Honestly, I don't get why you would have a spatially invariant Ct as a sensitivity study... that makes no sense to me at all. In practice you would never try to force uniformity of Ct. Make sure what you do makes sense in reality even if you have to abstract and simplify away from it.
  In the first manuscript version uploaded on WES, we did not consider to optimize for a uniform set-point distribution. However, in the first round of review, Dries Allaerts asked us the following questions:

  Did you consider optimizing for a uniform set-point distribution? What are the maximum gains to be expected there, and hence how much more is there to be gained by using a non-uniform set-point distribution? How would that uniform value compare to the average of the non-uniform distribution, and would the uniform value depend on the atmospheric condition as well?

  Hence, Appendix B has been added to address these questions. We understand that forcing uniformity in C_{T} is not something that you would do in practice. However, in this framework, it allows us to quantify the gains that come from an optimal non-uniform distribution. Moreover, under the assumption of uniform C_{T} distribution, the control space has unitary dimension. Therefore, the adjoint equations are not needed to solve the optimization problem and the model find the optimum in a much shorter time. Despite this, the power gains found are still considerable.
  Since this analysis was explicitly asked by a reviewer, we prefer to not remove appendix B from the text - in the end, it remains an appendix, and can be easily skipped when reading the paper.

- I find the explanation of the results in section 4.1 generally weak. Can you tie things more to the physics at play? Maximizing for energy will drive your optimal set points to a certain setting already to mitigate wake effects. The atmospheric effects are another layer. Is there any coupling? Did you do the optimization without the gravity waves and optimize the setpoints of the thrust first? This would be good to do in order to investigate the influence of each of the phenomena separately. Optimizing the thrust without disentangling the two means that you may be attributing too much of the effect to the counteraction of the influence of the atmospheric state
  Our model does not incorporate wake losses explicitly, except by means of the wake efficiency parameter η_{w} that we presume constant during optimization. Thus, our optimization is performed under the assumption that wake losses do not change, and only gravity waves are affected. In the future, it will be interesting to see whether including wake losses can increase or rather reduce the potential for overall power gains in the wind-farm operation.
  In the manuscript, we better discussed this in section 2.1 (see comment above), and further we also modified the following paragraph at P23-L550:

  "... before this can be translated to real wind-farm applications. In the current work, we did not include an explicit wake model in our model, and have presumed that wake losses remain unchanged during optimization (i.e., η_{w} is assumed to be constant). In the future, an analytical wind-farm wake model, such as, e.g., the one developed by Niayifar and Porté-Agel (2016) and used by Allaerts and Meyers (2019) could be adopted for optimization. This would however also require better representation in the wake model of changing background variables and pressure gradients. For instance, gravity-wave induced pressure gradient effects on turbine wake recovery could be included using the model proposed by Shamsoddin and Porté-Agel (2018) that incorporates effects of pressure gradients. Furthermore, the use of a wind-farm drag model which computes analytically the wake of each turbine would allow us to investigate separately the influence of wake effects and gravity waves on the optimal turbine set-points. This is work for future research."

- I understand you are reporting energy gains because you are time integrating power. But still, these are gains for a particular inflow condition set... so the energy gains reported (particularly in the abstract) could be easily misinterpreted... energy gain in the world of wind farm optimization (for control or other) typically looks from an annual perspective. Gains for particular inflow conditions are generally reported as power gains
  We agree with the editor's statement. Therefore, to avoid confusion with annual energy production studies, we have written all gains in terms of power instead of energy. Also, we have changed the definition of gain as follows:

  "We denote with $\mathcal{P}^{\mathrm{R}} = \widetilde{\mathcal{J}}^{\mathrm{R}}/T$ and $\mathcal{P}^{\mathrm{O}} = \widetilde{\mathcal{J}}^{\mathrm{O}}/T$ the power extracted using $C_T = C_T^{\mathrm{R}} = 8/9$ and $C_T = C_T^{\mathrm{O}}$, respectively. Further, we define

$$\mathcal{G} = \frac{\mathcal{P}^{\mathrm{O}} - \mathcal{P}^{\mathrm{R}}}{\mathcal{P}^{\mathrm{R}}}$$

  where $\mathcal{G}$ denotes the relative power gain obtained using an optimal thrust-coefficient distribution instead of the reference one. Note that the optimal distributions are steady-state, therefore the power gain definition is not dependent on the choice of the time horizon T."

Sensitivity study

- Again, mentioning the wind farm layout is out of scope is odd. I think it goes back to the architecture of the study where a case study was selected ad hoc rather than building up from a set of canonical cases. It would be nice to see a follow on conference article go back and do a more exhaustive exploration. It is not clear to me why the layout (at least the spacing of turbines) would not be a key sensitivity done in the current study... to me, that is indeed a key sensitivity
  We mention in the article that gravity-wave induced flow blockage is also related to the farm shape and size. Therefore, we agree with the editor in saying that the layout is an important parameter to be investigated. However, this really falls outside the scope of the current study.
  To better justify the layout choice, we have added a couple of sentences at P10-L281 (see also comment above). Moreover, as suggested, we have added in the conclusion the following statement at P23-L545:

  "We note that the gravity-wave induced power losses are also sensitive to the wind-farm layout. Optimization of layout (including, e.g., relevant techno-economical constraints) is however not considered here and can be an interesting topic for future research."

- The study is interesting, but it could be made more accessible through better context. How often do these different conditions happen in reality?

For the time being, there is no complete answer to this, and we don't want to speculate too much on this in the article. In ongoing work in our group, together with colleagues from climate modeling we are identifying parameter ranges and distributions of relevant parameter conditions by means of ERA5 reanalysis. Based on this, we find that the selected parameter ranges are relevant, but we do not yet have full mapping of frequencies in, e.g., the North-Sea region. We hope to publish work on this in the future.
The current sensitivity study is based on to the one carried out by Allaerts and Meyers (2019). However, the authors used a very wide range of h_*, $\overline{z_0}$, N/f_c and $g'H/Au_*^2$, exploring also atmospheric states that never manifest in the real word. In our study, we limit the non-dimensional groups range for two reasons. First, to ensure $U_1 < U_r$, where U_r is the turbine rated wind speed. Second, to have atmospheric states that occur in reality. Although these parameter values are representative of real case scenarios, we do not yet have access to realistic frequency maps.

Appendices

- Recommend deleting appendix B – see prior notes
  See comment above in results and discussion section.

2 Response to comments of Anonymous Referee #4

- In the title, instead of referring to just "gravity waves", perhaps refer to it as "atmospheric gravity waves" to distinguish them from cosmological gravity waves. Just a small point, feel free to leave as is.
  This is a good suggestion. We have changed the title accordingly.

- In the paragraph beginning at line 111, the model for the wind-farm force is described. It seems quite simple which is fine for this study, but I am wondering what the downsides might be to using such a simple model. For future research, if a more detailed model is used, what gains might there be? Perhaps some insight in this can be included here or in the conclusion. It is mentioned that it could be improved in the conclusion, but a little more insight would be beneficial to the reader.
  The reason why we did not adopt an analytical wind-farm model is three-fold. Firstly, we wanted to avoid the computational burden which comes with these types of model. Secondly, the derivation of the adjoint of the wind-farm drag first-order term would have been much more challenging. Thirdly, wake models as they exist now are not well suited for coupling to our gravity wave model, as they do not yet allow to incorporate varying background fields and pressure gradients, and this may significantly distort optimization results. Hence, we opted for a box-function wind-farm force model, which is, as you pointed out, a very simplified representation of a farm. However, in section 3.2 and 3.3 of Allaerts and Meyers (2019), there is a comparison of three-layer model predictions obtained using a box-function and an analytical wind-farm model. If you look at Fig. 4 of Allaerts and Meyers (2019), you could see that the green and blue lines (which represent flow perturbations) have similar trend and order of magnitude. Therefore, we would expect the power gains computed with a more detailed model to be of the same order of magnitude of the ones presented in the current manuscript. To include these considerations in the text, we have added the following sentences at P5-L151:

  "We note that Allaerts and Meyers (2019) have shown that the flow perturbations computed with this simple drag force model have similar trends and orders of magnitude as the ones computed using a drag model that relies on the more detailed analytical wake model of Niayifar and Porté-Agel (2016). Therefore, we believe that the model adopted is a reasonable representation of reality."

  Moreover, we added on P23-L550:

  "... before this can be translated to real wind-farm applications. In the current work, we did not include an explicit wake model in our model, and have presumed that wake losses remain unchanged during optimization (i.e., η_{w} is assumed to be constant). In the future, an analytical wind-farm wake model, such as, e.g., developed by Niayifar and Porté-Agel (2016) and used by Allaerts and Meyers (2019) could be adopted for optimization. This would however also require better representation in the wake model of changing background variables and pressure gradients. For instance, gravity-wave induced pressure gradient effects on turbine wakes recovery could be included using the model proposed by Shamsoddin and Porté-Agel (2018) that incorporates effects of pressure gradients. Furthermore, the use of a wind-farm drag model which computes analytically the wake of each turbine would allow us to investigate separately the influence of wake effects and gravity waves on the optimal turbine set-points. This is work for future research."

- Line 224 "asses" should be "assess".

Thank you. We have corrected the erratum.

- Line 322, Figure 2: perhaps I missed it, but what height are the plots shown at? Perhaps include that in the caption.
  The three-layer model is a perturbation model which solves depth-averaged RANS equations. Therefore, we cannot visualize the flow field at different heights. Instead, we can show the perturbations values with respect to the background state. This is what Fig. 2 illustrates. We note that in the text we have used the words "top view", which could be misleading. Hence, we have modified the following sentence at P13-L350:

  "Figure 2 illustrates a planform view of the perturbation flow patterns obtained with $F_r = 0.9$ (top row) and $F_r = 1.1$ (bottom row) using the reference model setup."

- Overall, my thoughts are wondering how the results would change when you include changing wind direction into the problem. It seems that it would complicate things and maybe reduce the stability of the gravity waves in the atmosphere. I am also thinking that if I was a wind farm owner, why would I implement this? What are the implications on loads? How can the atmosphere be measured to provide input to the wind farm in order to modify individual turbine set points? etc. Of coarse, all of that is outside of the scope of this well written paper, but just thoughts to think of in taking this and making it into something practical.
  These are interesting questions. Indeed, the gravity wave system can become much more complex when multiple layers exist in the free atmosphere, introducing reflection of waves, as well as when baroclinic conditions appear. There is potentially a lot of research to be done to study all these effect. We are currently working on extending our gravity wave model to include such effects, and once available, they could also be included in optimization and control approaches.
  Determining the atmospheric state and wind direction (which is an input to the model) will be an additional challenge. This could be done using on-site lidars, and may be also based on weather forecasting, or weather radar. There is definitely still a lot of research to be done before our work can be applied in a real wind farm. In the manuscript, we have not further discussed on these issues, as they are still very speculative, and a first important step is to improve models and validation.