General comments
- 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.

Additional detailed comments by section:

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
- Thrust coefficient manipulation is an intermediate effect that is brought about by wind farm control. The distinction should be addressed
- Generally, the introduction seems to jump into details without enough context
- Lines 44-48 – the way it is written, the concepts of blockage and gravity waves are being confounded
- Literature review on wind farm control is weak. There is a lot more work in the space including the comprehensive review article from 2019
- 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
- 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.
- 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

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?
- 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

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
- Typo line 246 firsts
- 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

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
- 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.
- Section 4.1 could be strengthened by a summary table of key statistics for each of the cases…
- 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
    - 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.
- 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
- 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 condtions are generally reported as power gains

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

Appendices
- Recommend deleting appendix B – see prior notes