Overall, this revision is a much stronger paper, accepting most of the suggestions presented in my previous review. The results and discussion sections of the paper contextualize the findings in a much more practical way. Additionally, the flow of the paper works well as it stands. The length is quite long, however, so the authors could consider moving parts of the analysis to the Appendix. For example, only a high level overview of the LiDAR processing chain is necessary for the reader to understand the results, and the same applies to the system identification process. This would allow the reader to focus better on analyzing the more relevant information, such as the helix identification and transformation, as well as the $\mathcal{H}_\infty$ controller synthesis. A few additional suggestions are presented below:

- *Perspective of wind stakeholders:* Line 378+ addresses why the closed loop helical wake generation is beneficial to stakeholders. However, the argument that they "value the consistency in wind farm performance to ensure predictability of power production and reduce operational risk" is vague. The results demonstrate that the vortex placement is more consistent in shear conditions, but not in turbulent conditions due to the variance from the nominal trajectory due to high frequency turbulence components. If such an argument with wind farm stakeholders is not able to be adequately justified, then it may be better to stick with the reasoning that closed loop helix generation has not been attempted before, and that additional work can discern whether this method results in the consistency that stakeholders value. Looking at the current results from the single run case, it is difficult to see where this consistency would manifest itself.
- *Single run results:* Although mentioned in the discussion, it should be made abundantly clear in the Simulation Setup section (4.1) that only one simulation is run per wind condition. A natural place would be around line 375, where the rest of the simulation parameters are defined. This would yield clearer interpretation of Figure 14, which could be interpreted as a multi-run statistical analysis rather than a single run analysis.
- *Figure 13 S&T (Shear and Turbulent) analysis:* The paper clearly states why the CL helix controller cannot eliminate fluctuations in the vortex trajectory due to its low cutoff frequency. However, it seems as if the trajectory is experiencing a higher variance from the nominal uniform trajectory in CL3 vs OL3 (i.e., comparing the rightmost graphs in Figs 11 and 13). Since the paper states a single wind seed was used, it is surprising that this behavior exists, and the paper does not mention that it increases – rather it states that the extra oscillations are not able to be mitigated as expected. It could be worth quantifying the extent of these oscillations with respect to the uniform case.

Perhaps an additional part of the figure could enumerate the variance of the turbulent helix trajectory from the non-turbulent trajectory, which would free the viewer from having to make their own conclusions visually.

- *Figure 16:* The y-axis limits should be shrunk. It is difficult to discern the two lines. In addition, the analysis for this figure is a bit misleading. For one, it is visually difficult to discern the difference in time-averaged magnitude between the OL and CL pitch signals, so a quantitative result should be given. Additionally, the primary concern for pitch actuation is *changes* in pitch actuation, rather than magnitude, since changing blade pitch results in actuator degradation. Perhaps this a nuance of the MBC transform that I am not grasping, but regardless, some additional clarification could be beneficial.