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Interactive comment on "Wind farms providing secondary frequency regulation: Evaluating the performance of model-based receding horizon control" *by* Carl R. Shapiro et al.

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We thank the referee for their careful reading of the manuscript. We have included detailed responses to each of the referee remarks/questions below, where the referee comments are in bold face.

General comments:

This paper presents results of model predictive control of wind farms to provide secondary frequency regulation balancing services for the power grid. A time-varying one-dimensional wake model is presented to model wake advection and

wake interactions while trying to enable real-time implementation.

Simulations show that the time-varying wake model leads to much better results than the static wake model that is presented and evaluated as a comparison. To fully put the simulation example in context, it would be useful to know the rated power of each of the 84 wind turbines in the example wind farm. Further, what is the rated wind speed for these turbines, what is the mean wind speed of the incoming flow onto the wind farm, and what is the distribution of the incoming wind speeds onto the wind farm in the simulations? What is the turbulence intensity? Or perhaps it is more useful to characterize the turbulence in terms of IEC turbulence characteristics.

The actuator disk model used to represent the wind turbines in LES assumes an idealized wind turbine operating in region 2 (always operating below the rated wind speed and power). Based on the maximum observed wind speed in the simulations, we will report the effective rated wind speed and power corresponding to this assumption in section 5. The mean effective wind speeds are shown in Table 2, but we agree that it would be helpful to provide some data on the turbulence intensity, including IEC turbulence class, of the incoming wind, which we will also provide in section 5. We will also add rotor diameter, hub height, and representative mean and maximum wind speeds to the caption of Figure 3.

In discussing the results shown on the left side of Figure 5, the authors describe the change in behavior at approximately 5 minutes, though they don't explain why the change in behavior occurs. Can the authors determine a reason for this sudden shift in behavior? It would be useful for readers if the authors also explain the other changes in behavior that are evident, such as around 15 minutes in the upper left 2 plots and around 25 minutes in all of the left plots.

Similarly for the right hand plots in Figure 5. The changes in behavior are slower, but there still appear to be qualitative changes in behavior. For instance, in the

lower right plot, the behavior before about 29.5 minutes is different from after that time. Can the authors analyze their data further to explain why the change in behavior occurs? And of course similarly for the other right hand plots of Figure 5. And for the plots in Figure 6 as well. By understanding the reasons for the shifts in behavior, both the authors and readers will be able to gain better insight into the properties of the receding horizon control technique used in this paper.

Thank you for the interesting analysis of the results. In some cases, we have some idea of the physical mechanisms the controller is leveraging to provide power tracking. For example, between minutes 10–20 of the right panels of Figure 5, the controller increases the thrust coefficients of upstream rows. This would increase the magnitude of the wakes and reduce the power production of downstream rows to follow the reference signal. In the left panels of Figure 6, the controller moves to a region with thrust coefficients of 2 for the last row and less than 1 for upstream rows. This is most likely the optimal power point for the Jensen model with constant wake expansion coefficients, but not necessarily the LES where changes in C_T' will affect the turbulent mixing in the farm. In other cases, such as minute 5 of the top left panel of Figure 5, the controller was already tracking a relatively constant signal, so change in behavior would probably require an analysis of the kinetic energy available in the flow field. In this case, however, the controller is again moving toward the Jensen model optimum. We propose to incorporate these discussions into the text of Section 6.

Specific comments:

1. The second sentence of Section 3.2 does not make sense (Page 9, line 9). It reads: "The row-averaged power and row-averaged, rotor-averaged are defined from velocities u_{nm} measured at every turbine in the wind farm ... " Would the following be more accurate? "The row-averaged power and row-averaged, rotor-averaged downstream wind velocities are defined from velocities u_{nm} measured at every turbine in the wind farm ... "

Thank you for pointing out this issue. We will change the sentence to "The row-averaged power and row- and rotor-averaged wind velocities are defined as [Equations], where u_{nm} is the velocity measured at the turbine in the n-th row and m-th column of the wind farm. "

2. When discussing Figure 7, the authors repeatedly specifically indicate that these results are based on an LES simulation, while such comments are never mentioned when discussing the results in Figures 5 and 6. Presumably the results in Figures 5 and 6 are also based on LES simulations?

Yes, Figures 5 and 6 are also based on LES simulations. We will add clarification about the use of LES to test the controllers in the captions of Figures 5 and 6 and at the beginning of section 6.

Technical corrections:

1. In equation (7), should the last term in the denominator be divided by D rather than multiplied by D? That is, should the last factor in the denominator be $[1 + 2k_n(x - s_n)/D]^2$?

2. In the summation in equation (8), should it be δu_m^2? That is, should the subscript on δu be m rather than n?

3. In the caption for Figure 3, the actuator disk turbine models look to me to be indicated by black "dashes" rather than "lines".

4. Page 12, line 17: "form" should be "from"

5. Page 13, line 1: "RegA.D4.IC4.TS" should be "RegA.D4.IC3.TS"

6. Page 13, line 29: "compared to other PJM signal" should be "compared to other PJM signals"

7. Page 16 line 6: What are "2/14" and "8/17"? These have no meaning to me.

8. References: Please list out each of the authors and do not use “et al.” in any of the author lists.

Thank you for pointing out these typographical errors. These issues will be corrected in the final version. For #7, the reference to “2/14 and 8/17" will be replaced by “the first two historical signals", which is what we are referring to in the text.

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