

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

Anonymous Referee #1

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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

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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.

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.

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 ... "

2. When discussing Figure 7, the authors repeatedly specifically indicate that these

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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?

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 $[1 + 2 k_n (x - s_n) / D]^2$?
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.

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