Review for "Evaluation of the global-blockage effect on power performance through simulations and measurements" by Alessandro Sebastiani et al.

Referee: James Bleeg

General comments

I am not aware of any other field observations in the literature directly demonstrating how global blockage effects (as defined in the manuscript) can influence the outcome of a turbine power performance measurement. The CFD simulations presented in the paper offer a useful complement to the observations, helping to provide a physical understanding of the trends. While the wind energy community seems vaguely aware that the undue influence of blockage can distort the outcome of a power performance test, very little as of yet is being done about it. Research such as this can help move the conversation forward to action.

Although I do not believe that the manuscript requires any major changes, I nevertheless have some change requests. They generally relate to adding detail and clarification in the interpretation of the results—and contextualizing them. Please see the next section for the change requests.

Main specific comments

Some key conclusions need to be softened or caveated. Even though the paper focuses on power performance, it also states multiple times that the turbines in a row produce on average 1% more power than they would operating in isolation. The finding that interrow interactions can increase power production is consistent with findings from other referenced researchers as pointed out in the Discussion section (your references are fine, but if you wanted to you could also add Nishino and Draper "Local blockage effect for wind turbines" as well as Strickland and Stevens "Investigating blockage effects in Large-Scale…"). Based on the results in the manuscript, and probably with added confidence derived from similar findings from others, the conclusions state "our work shows that wind turbine power output can be enhanced when wind turbines are aligned on a row."

All the evidence I have seen for this conclusion comes from simulations and experiments involving neutrally stratified flow (i.e. zero buoyancy). At the same time, there is strong evidence in the literature indicating that stable stratification, both within and above the boundary layer, has a first-order impact on blockage effects (e.g Schneemann et al and Allaerts and Meyers from the reference list). My own work in this area indicates that once atmospheric stability is accounted for in the simulation, the single-row production gains generally go away or are reversed (https://winddenmark.dk/node/2042, first download on this page).

Another factor that may materially affect the turbine interaction gains reported in the paper is the lack of shear and ground effect in the simulations. When actuator disks are simulated above a ground surface, the streamlines generally rise as they approach the wind farm such that the height of a streamline passing through the rotor is generally higher than the height of the streamline far upstream. The height differences are on average more positive than what would occur for a simulation of an isolated turbine. The significance of this is when the simulation is also run with vertical shear, the flow passing through the rotor within a wind farm originates from a lower height—with a lower energy flux—than flow passing through the turbine were it to be operating in isolation. The impact on power probably is not large, but it very well may be on the order of the ~1% trends noted in the paper.

For these reasons, I think the conclusion regarding the enhancement of power output for turbines aligned in the row should be scaled back or clearly caveated. At the least, the manuscript should indicate that this conclusion pertains to pure neutral conditions only. Different trends may be found when accounting for atmospheric stability within and/or above the boundary layer. All that said, I actually agree that there are probably combinations of turbine spacing and atmospheric conditions that do enhance power production. My concern is that as currently written the paper could leave the reader with the impression that this is generally the case in the field, and I do not believe the evidence as it currently stands is strong enough to support such a conclusion.

More could be done to explain the significance of the main conclusion. In my view, the most important finding is the evidence indicating that "power performance might be biased when performed on such an array of wind turbines with an inter-spacing below 3D." This conclusion largely derives from the results summarized in figures 9-11. At the least, the measurements demonstrate that global blockage effects, as defined in the paper, can materially influence the outcome of a power performance test. The CFD results further indicate the measured performance of a turbine in a row of turbines could be greater than what would be measured for a turbine operating in isolation—by around 2-4% for below-rated conditions in the simulated row. I think this what the authors are referring to when they say there is a bias, but I'm not sure.

I agree that the findings signify a bias, but I think more could be done to specify precisely what the bias is relative to. Is this bias limited to consideration of the outcome of a power performance test run on a turbine located in a row compared to a measurement with the turbine in isolation? Do the authors have a view as to whether there is also a bias relative to what is described on line 57 as the ideal definition of a power curve?

In addition, it might help the reader if these findings were put in context. One might argue that regardless of what would be measured for a turbine in isolation, the power curve measurement for the turbine in the wind farm is more relevant to consideration of how the turbine is performing in that wind farm. I would disagree with such an argument, but it would be good to hear the view of the authors.

My own view is as follows. There is no requirement to use it in the paper; I just want to provide an example of what I have in mind. When the gross energy is calculated during an energy yield analysis (EYA), the estimated freestream wind speed frequency distribution at hub height at each wind turbine location is fed directly into a power curve. Therefore, an accurate estimate of the gross energy of each turbine requires a power curve that faithfully represent how much energy the turbine produces when operating in isolation for the given freestream wind resource. In my view, your results strongly suggests that global blockage effects can cause measured power curves to depart from the power curve that is needed in an EYA. Further, since measured power curves influence the curves used in EYAs (there is plenty of evidence indicate that measured curves are similar to and no more energetic than those used in EYAs), this signifies a bias in EYAs.

Definition of power performance. When the term "power performance" is used in this paper, I believe it always refers to measured power performance or simulated 'measured' power performance. In other words, when on line 15 you write, "we also show that the power performance is impacted by the neighboring turbines," you are referring to the measured power performance. Further, you are not saying that the actual power performance of the turbine is impacted by neighboring turbines. Is my understanding correct? If my assumptions are correct, I think it would be a good idea to explicitly clarify this in the manuscript.

If my assumption is not correct, it would be to get a clearer understanding of your view on this.

Minor specific comments

- Line 85: I think parentheses should be around the two Meyer Forsting references.
- Line 135: Maybe this is a UK vs US thing, but I would replace ", which consists of" with "consisting of" (no comma)
- Line 223: How did you come up with a C_T level for the simulated turbines? What is that level?