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Title: FarmConners Market Showcases Results: Wind farm flow control considering electricity prices and revenue

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Overall comments

The submitted article is a part of a series of papers related to the FarmConners project. Five different wind farm control methods (one individual turbine control, four wind farm flow control) are used to estimate the potential revenue increases in single 2020 and 2030 electricity price scenarios.

Overall, the motivation and contributions of the study could be more clearly explained. Results from different groups are analyzed but are not compared in detail to one another. If the focus isn't to compare the different strategies in detail, why are five distinct participants included? The stated focus of the article appears to be on evaluating methods for maximizing revenue, but the four flow control methods seem to maximize energy and the results are translated into energy after, using averaged electricity prices per wind direction sector. Also, since the authors focus on arbitrarily defined “high price” instances, it is hard to get a sense of the actual potential to increase yearly/farm lifetime revenue. Uncertainties (of which there are many) are discussed, which is great, but the discussion is limited to only one paragraph in the conclusions.

Overall, I believe this paper could have potential to provide insight into the market potential for wind farm control but I encourage the authors to consider the comments below, in the hope that they can help improve the paper. In particular, some decisions in the methods and discussions in the results seem qualitative or ad hoc, and could use more justification.

General comments

1. I am not clear on why the authors have parsed the price timeseries to only consider “high prices” (arbitrarily set at the highest 25% of prices). The electricity prices don't have any impact on the power gain. But since the paper filters the wind conditions by the electricity prices for the different years, the wind conditions analyzed are different, since the electricity price timeseries are different between the two years considered. I would have thought that the “showcase” should focus on annually averaged metrics, or even better, farm lifetime averaged metrics (with uncertainty). I also think that looking at only one 2030 scenario of prices is pretty limiting, since there is substantial uncertainty around future energy prices.

2. The study doesn't appear to actually do any revenue maximization, which I believe would have been much more interesting. The results demonstrated in this study take energy gain and multiply by wind direction sector averaged energy prices. But maximizing revenue (balancing short-term revenue

gains from power increases and long-term (potential) revenue gained/lost due to decreased/increase fatigue loads (causing changes in O&M, different lifetime, etc.)) could potentially lead to different control strategies, which would be interesting for the community.

Point comments

1. Lines 2-4: *"For this, offshore wind will play a major role, significantly contributing to a paradigm shift in the power generation and greater volatility of electricity prices. The operating strategy of wind farms should therefore move from a power maximization to revenue maximisation design."*

I don't quite follow the logic in these two sentences and I would suggest rephrasing. Why would a greater volatility of electricity prices necessitate revenue maximization rather than power maximization? My initial thought after reading the first sentence was that farms should focus on system benefits (e.g. regulation services) rather and power maximization or revenue maximization.

2. Line 12: "[…] and a favourable control strategy for dominant wind directions can pay off."

This statement is a bit vague. Do the authors mean that a revenue maximizing strategy differs from a power maximizing strategy? It would also help to include quantitative statements in the abstract.

3. Section 1.2: I did not quite follow the strategy to determine energy price scenarios. It appears that 2020 and 2030 price scenario timeseries are generated. Then the price timeseries in parsed into low and high prices, corresponding to the lowest and highest 25% of the price data respectively? Please add a few more sentences explaining and justifying these selections. Why was no full year scenario run with all of the price data (rather than just 25% of the highest prices)?

4. Line 83: Define power-boosting for individual turbine control.

5. Line 94: Can the authors include more details and references for the P1 aeroelastic and surrogate modeling methodology?

6. Line 98: What is meant by "desired trajectory"?

7. Line 100: *"The whole process is automated so that given a desired trajectory, the relevant design variables can be estimated, i.e., the torque constant, rated values, cut in, and rated wind speed."*

I did not follow this sentence. What do the authors mean by estimating the relevant design variables? Are the authors considering revenue maximizing wind turbine design in this study? My understanding was that this study focused on wind farm flow control (WFFC), and there is an existing reference case with fixed turbine properties. Perhaps these variables need to be dynamically estimated because of the modified control? It's not clear to me why you would need to estimate these design variables if you have "power coefficient (Cp), tip speed ratio, and blade pitch angle" already.

8. Figure 2: This is a helpful figure. The authors could emphasize that only turbine pitch is being modified to achieve the controller mode.

9. Section 2: To ensure a self-contained article, the authors should briefly describe each controller mode shown in Figure 2.

10. Line 118: How long does it take to achieve steady-state behavior in the FAST simulations? Is this hysteresis neglected in the timeseries evaluation of the controller?

11. Figure 3:
    a. Perhaps make it clear in the caption that some figures show a subset of the 2030 timeseries while others show the full timeseries.
    b. Bottom left subfigure: Should the y-axis be “instantaneous power” instead of “rated power”?
    c. Bottom right subfigure: This plot could use more explanation. Why is there a large initial transient in each case? I also find the terminology “Accumulated DEL” confusing when it is being computed using Eq. (1).

12. Line 166: Can the authors provide more details of the tuning process to give insight into challenges and research gaps? What input variables are the authors setting user-defined thresholds on? Just the PI pitch controller?

13. Line 170: This paragraph’s discussion and the results in Figure 5 are very interesting! It would be great if the authors could provide more discussion about challenges and opportunities for multi-objective optimization of revenue and loads based on your results (as also related to the previous comment). It appears from the discussion that the empirical results shown here depend strongly on the tuning.

14. Figure 5 is very small, consider increasing the figures and the fontsize.

15. Line 175: This statement makes it more clear why the details were very limited here. No reference is provided for this method, just an indication of a future study. This is not ideal, as articles should be self-contained, or at a minimum have adequate descriptions and references to peer-reviewed publications which are accessible to readers.

16. Table 2:
    a. I recognize ‘Gauss-legacy’ is the FLORIS terminology, and useful to include, but can the authors explain in the table caption why it is called ‘legacy’ to inform readers who aren’t as familiar with FLORIS.
    b. P3: Citation missing for linear superposition. Is this linear superposition of deficits with respect to freestream or the local velocity [1]?
    c. P2 and P4 are using identical model setups?

17. Table 3:

a. Add a definition of each parameter introduced in this table. It would be much easier for readers to make this paper more self-contained, rather than having to refer to FLORIS documentation (which is also not archival since GitHub repositories can evolve).
b. Why are different values of n used by P2 and P4? These values of n depend on the turbine model, the wind speed and direction shear [2], and the waked conditions [3]. Since the test case is the same between the participants (with DTU-10MW turbines), I would have expected n to be the same.

18. Section 3.3: How are the wind shear exponents found? Please include more details. To which data are the power-law curves fit? Please also explain how the wind shear exponent is used by the wake models in FLORIS.

19. Section 3.4: What is the power-yaw exponent for P5?

20. Figure 6: I suggest having the same colorbar axis limits for all of the subfigures.

21. Line 346: *"Adjacent wind speed bins with high and low power gains in the same sector are observed exclusively in P5 results."*

This also appears to happen with P3 (e.g. northerly flow), or am I misunderstanding this statement?

22. Line 348: *"The discussed Figures 6 and 7 show consistent power gains per participant for both years."*

What do the authors mean by "consistent"? Consistent between 2020 and 2030 or consistent between the different participants. If it is the latter, I would suggest the results have significant spread.

23. Line 373: *"Although both P3 and P4 simulated the full TC-RWP, the different control strategies applied are seen to be the main driver for that disparate behaviour."*

Can we definitively say this or can it also relate to the different wind farm model?

24. Line 384: *"The energy gain reported by P5 in 2020 is 2%, which is slightly higher than that in 2030 with 1.7%."*

Are these differences (between 1.7% and 2%) statistically significant?

25. Figure 8:

a. Is the high energy gain associated with P4 due to the discretization of the wind directions within a wind direction bin?
b. With this (and other bulk metric figures) it probably makes sense to plot the income gain per turbine so that P2 isn't falsely interpreted as an outlier.

26. Line 395: How is income gain computed? Is the power increase for each time step multiplied by the cost of electricity within that timestep? In the introduction, the study is motivated by the time-varying nature of the energy prices, so it would seem natural to do a timeseries analysis rather than using a mean energy price per wind direction sector.

27. Line 403: Sentence starting “Accordingly […]” is a bit confusing. I didn’t understand the point being made. Consider rephrasing.

28. Line 469: *“The benefit of revenue maximisation and structural load reduction as control objectives depending on the electricity prices is demonstrated at a single wind turbine”*

As in previous comments, I don’t think the authors have actually done revenue maximization. They have done power maximization and then translated the results into economic terms through a price for energy. I expect that a revenue maximization approach would focus on balancing short-term revenue gains from power increases and long-term (potential) revenue gained/lost due to decreased/increase fatigue loads (causing changes in O&M, different lifetime, etc.).

29. Line 482: *“The normalised gains of the four analysed WFFC implementations is consistent across the simulated cases.”*

Not sure I agree with this qualitative statement that the different participants have “consistent” results but since it is qualitative it is subjective. It appears that the results differ in some cases by a factor of two or more, which seems significant.

30. Line 488: *“Benefit of maximising income instead of power gain”*

Which results in this study demonstrate this conclusion?

References

[1] Niayifar, Amin, and Fernando Porté-Agel. "Analytical modeling of wind farms: A new approach for power prediction." *Energies* 9, no. 9 (2016): 741.

[2] Howland, Michael F., Carlos Moral González, Juan José Pena Martínez, Jesús Bas Quesada, Felipe Palou Larranaga, Neeraj K. Yadav, Jasvipul S. Chawla, and John O. Dabiri. "Influence of atmospheric conditions on the power production of utility-scale wind turbines in yaw misalignment." *Journal of Renewable and Sustainable Energy* 12, no. 6 (2020): 063307.

[3] Liew, Jaime, Albert M. Urbán, and Søren Juhl Andersen. "Analytical model for the power–yaw sensitivity of wind turbines operating in full wake." *Wind Energy Science* 5, no. 1 (2020): 427-437.