Dear authors,

First of all, thank you for your responses to my comments. You have taken my points seriously and answered my questions with care. The figures have significantly improved, the document is easier to read, and notes and explanations have been added contextually. Also, not all comments led to a change in the manuscript, and I fully understand that.

Though, one important concern of me remains with the current publication. The conclusion of this article invokes the idea that axial induction control provided an estimated gain in power production of 1.7% to 2.4%. I am not saying that I disagree with these numbers, but at the same time I am not yet fully convinced after reading the paper. As mentioned before and as you have discussed in your manuscript, there may be other factors at play. I believe you can investigate this more, which would bring more confidence to these numbers. When I look at figure 15, I see that for basically every bin, the mean turbulence intensity for ON is higher than OFF. A higher turbulence intensity may give a higher power production in these situations. This argument is supported by looking at bins with directions 185 to 205 degrees, where axial induction control is not really applied and yet gains are large. Similarly, when looking at the left subplots of Figure 15, the largest gain is at the bin with WS = 10.5 m/s, which also has the largest difference in TI between ON/OFF. At other bins with almost equal TI between ON/OFF, we also see barely any gain in power production.

Now, when we look at the right subplots of Figure 15, things get better and I think this does indicate a gain due to axial induction control, specifically for bins 235 deg and 255 deg, which have almost no difference in TI. I believe it is fair to say that these bins indicate a gain, but I think it is not fair to say that *every* bin indicates a gain due to axial induction control. I think one must investigate the effect of the turbulence intensity (among others) first, before being able to conclude this. Similarly, if you use all data to calculate mean values like on page 21, then you will be comparing ON data with an effectively higher mean TI with OFF data with an effectively lower mean TI. There may indeed be a 2.42% gain between ON and OFF, but perhaps this is 2% due to TI and only 0.42% due to axial induction control. I would suggest the authors to be very careful making such statements. I anticipate this to be a high-impact paper and these numbers are likely to be used as reference values for the potential of axial induction control.

Now, the additional figures, especially Figure 18, adds significant value to the manuscript. From this figure, I conclude that the mean TI between ON and OFF is not very large. This is a very interesting observation and supports the conclusion that the authors already make.

To strengthen the conclusions from the authors, I would suggest diving deeper into Figures 16 and 20. LongSim was used to (re)simulate the measurement points and also reported significant gains. Using LongSim, you could figure out where these large gains come from. If in the "ON" dataset you simulate them with the baseline (OFF) controller, do you still see such large gains? If so, then the gains are not due to axial induction control, but due to other effects. Similarly, what if you simulate the datapoints in LongSim with a fixed value for TI – that could provide insight into the effect of the turbulence intensity on the power gain. By doing these manual simulations, you will gain much insight into where the gains are really coming from, and perhaps why the values are so high. Since such large gains are also seen in LongSim, it must be more than statistical uncertainty.

A second idea to give more insight into the effect of TI is by redistributing the bins in Figure 15 so make sure their average TI is identical. For example, one could make duplicates of low-TI "ON" measurements or make duplicates of high-TI "OFF" measurements to bring the average TI to the same value. Now, with the datasets already being so sparse, one may introduce additional bias, so perhaps the former suggestion is better.

Figuring this out will also prevent vague statement such as on page 20 "it is possible that […] might account for [..] some […] power increase."

Smaller comments:

- Abstract: "show a positive increase in energy production resulting from induction control" is a strong statement. If anything, perhaps rephrase it to something like "the experimental data suggests that induction control leads to both gains and losses in power production, with the gains outnumbering the losses."
- Figure 14: for consistency, it would make sense to put the plot title on the ylabel instead. Same goes for Figure 21. Also, for Figure 21, perhaps change the xlabel to "Time [Hours]"
- I am not sure if Table 3 adds significant value
- Figure 23: the authors state that the agreement is very good. Though, I believe at this scale it's hard to draw this conclusion. The power production may be off by 30% at any point in time, especially at such low absolute power values.