Major comments:

1. The most significant improvement for this manuscript would be an increased clarify and structure, in my eyes. Often, sections are very long and one loses track of the purpose of a section. I would very much like to see the text restructured into more subsections and paragraphs. For example:
    - **a)** Introduction: separate subsections for modern challenges in large-scale implementation.
    - **b)** Introduction: too, much information on the dynamic FarmFlow part. I think you can remove lines 103-110: from "the dynamic simulation..." until "...in real-life measurements." without losing any valuable information in the introduction.
    - **c)** Section 2.3, page 8: why is the derivation of wind direction variability part of the "yaw model"? This, to me, should be part of the inflow/wind field model.
    - **d)** Table 1: this is a table related to validation of the model choices. This seems somewhat out of place to me, since you are still explaining the fundamentals of the model.
    - **e)** Section 2.4: why is this part of Section 2: "wind farm model"? Typically, the wake/wind farm controller is not considered to be part of the wind farm model, especially in this entire context. Perhaps instead this should become part of Section 3 and Section 3 should become "AWC design"
    - **f)** Section 3.1: find a way to clearly separate each factor of uncertainty/parameter. Perhaps a bullet point list or subsections/paragraphs.
    - **g)** Line 437: can start a new subsection *(see next comment)
    - **h)** Latter half of Section 4 vs. Section 5. One shows a basic case study for 3 turbines, and the other shows a more realistic case study with OWEZ. To me, it would make sense to put them both in Section 5 and separate them into two subsections: one for verification/simple study case for understanding, and then one for a more realistic evaluation.
    - **i)** I read that FarmFlow has been extended to include wake and yaw control dynamics. It also now accepts dynamic wind fields to drive the simulation, including temporal and spatial variations. These are great developments. I would very much appreciate any kind of validation of these new functions. However, with the paper already being as long as it is, perhaps it would be better to present the dynamic FarmFlow plus validation in a separate publication. This would also increase clarity in the current manuscript.

2. Please motivate certain statements with the right literature and avoid speculation.
    - **a)** In the introduction, I read that the potential AEP gain with AWC is several percents. This seems very high and currently not too realistic based on the recent expert elicitation and field experiments that exist in the literature. Actually, the papers cited with this statement are simulation studies and are better replaced with the Wingerden et al. expert elicitation and field experiments from Howland, Fleming, Simley, Duc and Doekemeijer. This relates to the minor comment on citing literature.
    - **b)** Line 33, where the author assumes that the accumulated loads over the whole lifetime of a wind turbine decreases with wake steering, rather than increases, because Siemens-Gamesa is selling a wake steering solution. This reasoning seems flawed to me. We do not fully know under what conditions Siemens-Gamesa is doing wake

steering, if they require additional equipment, whether and which loads increase and decrease, when they do, and whether this relates to fatigue or ultimate loads. There is too little information to make any conclusions based on the fact that Siemens-Gamesa is selling wake steering, besides perhaps that it has caught the interest of this OEM.

c) Line 134-135, it is stated that wind farm simulations require time scales of tens of seconds. How about wake meandering or finer flow effects? How about large-eddy simulations? Add citations or at least defend this statement. Similarly, motivate choices of spatial resolution and sample time of the inflow.

d) Line 302: motivate natural frequency of meandering

e) Would be nice to clearly define the novel contributions in this article vs. what was done in previous work. FarmFlow already existed, but has been made dynamic: that is new, no? Uncertainty quantification is novel, at least for that exhaustive of a parameter set. Robust AWC and hysteresis already existed in literature, right?

Minor comments:

1. The abstract contains the general outline of the paper but misses the actual contributions and results. It currently does not suffice as a standalone summary of the paper. Please include the core findings, qualitatively but also quantitatively. For example, depict the parameters that were found to be the most important from the sensivity analysis, depict the potential AEP gain in percent, and so on.

2. Generally, and especially when citing literature, you should clarify the test environment used in that publication. The differences between a FLORIS-based simulation study, a SOWFA-based simulation study, a field experiment or a wind tunnel experiment are very significant.

3. For literature review: similar work is from M. Sinner et al., 2021, but this only appeared in April 2021. I can understand that the authors had already finished this publication mostly by then. You could consider including it in a revision.

"Power increases using wind direction spatial filtering for wind farm control: Evaluation using FLORIS, modified for dynamic settings", Sinner et al., 2021, JRSE

4. Line 96: "This analysis ... the wind velocity." This is a conclusion and should not be part of the introduction. Rather, the introduction should be limited to what topics will be addressed in the article. The same goes for the sentence starting at

5. line 99: "A stationary analysis ... of power gain." Nice, but should go to conclusion.
Line 182: "wake generated by a wind turbine is propagated downstream based on the local wind direction variations in its way", I do not understand this.

6. Line 183: "because the traveling time ... current time window." I do not understand this.

7. Line 202: "written in an output file", this seems inefficient. Can this not directly be exchanged through memory or over a network protocol?

8. Line 260: On what signal does the LP filter work?

9. Line 314: You mention that the PDF for turbulence intensity is based on historical data. Does your definition of TI (i.e., being the standard deviation in streamwise direction, match up with the definition in the data? I can imagine that the historical data considers the TI to include both streamwise and cross-stream turbulence.

10. Line 349: What optimization algorithm is used? How confident are you that the solution has converged? What are the bounds, e.g., have you limited the minimum and maximum yaw angles?
11. Table 3: the wind speed range seems so high, while in reality you could feed in the wind speed measurements into the LUT, perhaps with an uncertainty bound but definitely smaller than an uncertainty of 8 m/s. How do you defend this decision? Also, how do these findings line up with your earlier work stating that wind speed can be ignored in yaw optimizations?
12. Line 381: I would have expected the yaw-induced power loss coefficient to have a larger effect on the optimal yaw angles, since it directly impacts the energy lost by yawing an upstream wind turbine. Can you reason why this is not the case in this study?
13. Figure 5: yaw angles of -40 deg and + 45 deg seem excessive. Can you explain your choice for allowing yaw angles to go all the way to these values? Since we would never optimize the yaw angles until those limits in practice, this may skew the sensitivity analysis somewhat, no? Perhaps certain parameters are important at high misalignment angles, but really are not that important in the range we expect to yaw the turbines to.
14. Figures 5 & 6: please add legends
15. Line 414: should it read 'arg max' instead of 'arg min'?
16. Line 434: "...only decent directions...", what are "decent directions"?
17. Figure 8: neither line is particularly smooth. Does this suggest that the optimization has not converged?
18. Figure 9: "robust AWC" and "nominal AWC (with uncertainty)" are not the same thing, yet it is hard to distinguish them in their definitions. Can you clarify?
19. Figure 11: 11% Energy gain is very substantial and not particularly realistic for AEP. Maybe repeat that this is for particular 3-turbine case. Also, these figures are hard to see. I would suggest turning them into top-view (2D) contour plots instead. Same goes for Figure 12.
20. Line 555: Just to clarify, so dynamic FarmFlow runs 1:1 (6 hours of simulation means 6 hours of computing in real time on a single core)? If so, it may be worth evaluating the potential for a full year of operation (~9,000 CPU hours).
21. Line 567: "Notice that the overall gains are lower than one might expect", what would be a reasonable number to expect? 0.5-2% energy gain is still significant if you ask me.

Technical comments:

1. Variables should be italic, units should not.
2. Line 4: "by up to a few percentage points." Why percentage points and not percent?
3. Line 19: "possible power gains of up to a few percent on annual basis". Can you motivate this further, maybe add citations? To me, it seems that it is more towards a single percent, especially when looking at the most recent field experiments.
4. Line 20: The second challenge is presented as being mainly due to wake models being of static nature.
5. Figure 1: it says "yaw systems". Should it be "yaw system" since its for a single turbine?
6. Figure 1: The Robust AWC LUT seems only a function of wind direction and wind speed. Does this mean local WS/WD?
7. Line 149: "frequencies above 10e-3 Hz", should this be "above 10e-3 Hz"?
8. Line 279: "In this work ... robust design setting." I understand what you mean, but perhaps reformulate it in a clearer way. For example, differentiate between variables included as

uncertainties in the optimization process and variables that are used for the real-time interpolation of setpoints.