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Interactive comment on "Optimal closed-loop wake steering, Part 1: Conventionally neutral atmospheric boundary layer conditions" *by* Michael F. Howland et al.

Anonymous Referee #2

Received and published: 21 May 2020

Thank you very much for this paper. On the whole I found it to be both well written and thoroughly researched. I also commend the author's on excellently situating this work within the public research and providing a thorough review of the relevant background.

Additionally, I agree with a main suggestion of the paper, that wake steering has reached a point where it is important to not only demonstrate that it can work, but to struggle with the real-time implementation and controls issues. I believe the method proposed in this paper makes a lot of sense, using state estimation to calibrate the wake model. I believe the paper can be accepted with minor revision.

General Comments:

One overall comment I had was I wasn't quite positive of the main, conclusion of the paper. Is that the proposed method is at least successful in the provided tests, or that it is necessary to use a method like this? The paper is a little bit long, I wondered if parts might be condensed also to make more clear what are the bigger, more important, findings? The authors however can freely disregard this suggestion, but I hope this impression is useful.

What is the model of yaw control within the turbine used? Is the turbine free to yaw at any moment? Most turbines have a built-in yaw control strategy which includes some dead-band about vane angle, and an intentionally delayed response to changes in wind direction. If this was not used, how would it change results if it were?

One general comment I had on the sections related to the cos pP parameter, is that the discussions and conclusions are proposed in absolute terms, where relative would be more appropriate. As i understand, pP=3 is "correct" in this LES simulation, and so 2 is 2/3 of correct, and 4 is 4/3 of correct. Most of the numbers are baselined to a correct value of 3, but should be scaled in other simulations or on physical turbines. My main point is to avoid stating that 2,3 or 4 is better/worse and more under-predicting pP by x% leads to, while over-prediciting pP by x% leads to ... In a similar way this would change the statement "a conservative estimate of pP=4" should be used in cases where it's not yet known could be the more reasonable 125% of the value of the most similar published value (in terms of rated power or rotor size).

Specific comments:

The last sentences of the abstract are somewhat confusing before you read the paper

p 13 "likely enhanced in yaw misalignment" see yaw-added recovery in https://www.wind-energ-sci-discuss.net/wes-2020-3/

P23 "wind speed and direction bins of arbitary size" this seems pretty possible using interpolating functions

P23: "using a neural network for example" these ideas are theoretically possible but practical observation suggests that any method who's parameters are not human intelligible will have obstacles because it will be difficult to make in-field adjustments

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