Title: On the power and control of a misaligned rotor - Beyond the cosine law
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The submitted paper studies the power of wind turbines in yaw misalignment. An induction model is coupled with a simple blade element model, and the resulting model outputs are compared to large eddy simulations (LES) of actuator line modeled (ALM) turbines and wind tunnel experiments. Overall, the paper could be useful for the community. Modelling the power of yawed turbines is important. The contribution of this paper is coupling an induction model with a blade element model. The LES and experimental validation campaign is thorough.

However, it appears that several components of the proposed model have been already developed in the following two papers:

1. Shapiro, Gayme & Meneveau *Journal of Fluid Mechanics* (2018) [1]
   This paper develops a lifting line model for the transverse velocity (downwash) associated with a yawed turbine. The present paper appears to follow the same analysis, resulting in the same final answer (compare: Eq. 2.7 in Shapiro to Eq. (13) here), but reference to Shapiro *et al.* (2018) [1] is missing.

2. Heck, Johlas & Howland *Journal of Fluid Mechanics* (2023) [2]
   This paper develops a model for the induction, thrust, power, and wake velocities for a yawed actuator disk, using the lifting line model of Shapiro, Gayme & Meneveau (2018) [1], but also accounting for how the induction affects the transverse velocity. The induction and wake velocity model developed in Heck, Johlas & Howland (2023) [2] is the same as the induction and wake velocity model in this present paper (compare: combining Eqs. (2.15) and E1 in Heck to Eq. (14) here), but this is not stated in the current manuscript.

To summarize, Section 2.5 in the present paper can be replaced with references to [1] and [2]. Explaining the progress of yaw modelling based on existing literature and how the present paper has contributed will be helpful for the readers. Overall, the main contribution of this submitted paper is to build on Shapiro, Gayme & Meneveau (2018) [1] and Heck, Johlas & Howland (2023) [2], by coupling their induction model with a simple blade element model, and the detailed comparisons to LES and experimental data. These are useful contributions to the literature, but the framing, comparisons to baseline methods, and other comments below should be re-considered in the authors' revision.

General comments

1. The authors have not compared their modified model (coupling with a simple blade element model) version to baseline approaches, including the Glauert induction model (model for rotor averaged induction in yaw) and the actuator disk induction model from Heck, Johlas & Howland

(2023) [2] (same as present model without the blade element coupling).

2. It appears that tunable parameters in the model are calibrated based on the same data that they are tested against, which is not ideal practice. Can this be considered model validation? How should this be done in general?

3. Many figures are quite small, making it challenging to discern the accuracy of the model.

4. The paper states that a major contribution of the modelling is to capture asymmetry from wind speed shear, but the quantitative effect of wind speed shear presented in the results seems to be (visually) very small. I suggest quantifying its impact to help see its effect more clearly.

Line comments

1. Line 28: The formatting of the power equation looks as though all the variables are in the denominator.

2. Line 73: *"Additionally, the new model clarifies the behavior of power capture with respect to some rotor design parameters and – even more importantly – with respect to the way a rotor is governed when it is misaligned. This is an effect that has been neglected in all analyses conducted so far, and that most probably explains the large scatter observed by various author"*

   This statement is not correct. Howland *et al.* (2020) [3] developed a blade element model for the power-yaw relationship that incorporates rotor design parameters and the way a rotor is governed in misalignment. However, Howland *et al.* (2020) [3] did not have a model for induction in yaw.

3. Section 2.2: Why have the authors assumed inflow with linear vertical shear? This seems to be a limiting decision in the context of a paper which focuses on building a model for yawed turbines in general. This should be justified in more detail. For example, Liew *et al.* (2020) [4] identified that waked inflow modifies the power-yaw relationship, but this inherently cannot be captured in the present model that only considers linear vertical shear. Also, wind shear in the stratified boundary layer is very rarely linear. In the modelling and field experiment study of Howland *et al.* (2020) [3], the joint effects of low-level jets and wind veer were found to be important. They can be modeled using blade element modelling [3].

4. Section 2.2: Why have the authors elected to neglect wind veer, which has been shown to be important in wake steering [5] and in power-yaw modelling [3] in published papers?

5. Equations 5a and 5b: Tangential induction has been neglected. This should be mentioned and justified.

6. Section 2.4: The structure of presentation in this section is a little odd. It starts by claiming that the non-uniform induction does not affect the results, then shows the equations, then neglects it for the remainder of the study. I suggest moving this section to the Appendix, and also including the quantitative evidence (referenced but not shown) that it is negligible in your cases.

7. Page 9 footnote: *"This interpretation also reveals that the so-called curled shape of laterally deflected wakes (see e.g. Martínez-Tossas et al. (2021) and references therein) is nothing else than the effect of the horseshoe vortex structure generated behind a lifting wing, albeit with the addition of the swirl caused by the rotor rotation."*
   This explanation is exactly the one provided by Shapiro *et al.* (2018) [1] which the authors have not referenced in their study. This statement should be removed and references to Shapiro *et al.* (2018) [1] must be added.

8. Equation (13): This is the same lateral velocity equation derived by Shapiro *et al.* (2018) [1] derived using Prandtl's lifting line theory, except that Shapiro *et al.* (2018) [1] assumed that thrust varies with cos(yaw)^2. Heck *et al.* (2023) [2] extended the lifting line model of Shapiro *et al.* (2018) [1] to no longer assume thrust follows cos(yaw)^2, and the final answer in Heck *et al.* (2023) [2] (Eq. (2.15)) is the same as Eq. (13) here.

9. Equation (14): Similarly, this is the same induction model derived by Heck *et al.* (2023) [2], although it is presented in a slightly different form in the previously published paper. Appendix E from Heck *et al.* (2023) [2] is pertinent (i.e. combine Eq. (2.15) with Eq. (E1) to arrive at the induction model form below that can be compared to Eq. (14) in the present paper).
   From Heck *et al.* (2023) [2], the induction model equation is:
   $$a_n = \frac{2C_T - 4 + \sqrt{16 - 16C_T - C_T^2 \sin^2(\gamma)}}{-4 + \sqrt{16 - 16C_T - C_T^2 \sin^2(\gamma)}}$$
   which appears to yield identical predictions to Equation (14) in this study.
   In summary, Section 2.5 is a repeat of existing literature and can be removed, with appropriate references added.

10. Line 206: Please justify the assumption of the small inflow angle, especially in the context of yaw and tilt misalignment and shear.

11. Equations (18a) and (18b): Have the authors assumed that the lift and drag coefficients are constant along the wind turbine blade? Please explain.

12. Line 239: *"The power model reveals that vertical shear is the culprit for the observed lack of symmetry with respect to yaw misalignment."*

I don’t quite understand this sentence. When the authors state “ observed lack of symmetry,” are they referring to existing published literature or to their own data (which to this point has not been presented). Previous studies have already explained and modeled that wind speed shear and wind direction veer cause the asymmetric power with respect to yaw misalignment. This current study neglects veer, which also seems limiting.

13. Figure 5: The effect of wind speed shear is very small. I expect the effect of veer is much larger, and especially when there is both shear and veer.

14. Paragraph beginning on line 274: It’s good the authors state assumptions and simplifications here, but they should also all be stated and explained within the derivation. Otherwise, it seems as if the authors are making ad hoc choices about what is important and what approximations are made.

15. Equation (28) and associated discussion: I do not understand the motivation for simplifications to be applied to the model and then the tuning of more unknown parameters. How does this affect the result? How can this be done in general? Do the authors expect these parameters to be universal, and if not, how can this model be applied to a new wind turbine model? Do we need power data for turbines in yaw misalignment to tune this model? If so, that is not necessarily useful as a predictive model.

16. The authors need to include an Appendix that describes the tuning process in much more detail. What do the authors mean when they say *“a different random 50% subset of the available data”*? Is this the training-testing split? Presumably the authors are not performing model tuning with the same data that are used to test the model accuracy, as this is improper practice and can bias the results. In the added Appendix I am requesting, model results without any tuning must be shown.

17. β: It strikes me as a bit strange to have a tunable parameter in the model represent a known geometrical feature such as blade twist.

18. Figure 7: the authors show four results in this figure that are almost identical. It is very challenging to discern any notable differences among the subfigures, so it’s reasonable to ask whether the authors have really tested the limits and applicability of their modelling framework. For example, why has the tip-speed ratio been kept within such a small region?

19. Line 324: *“However, as thrust is decreased (and pitch increased), power capture at positive γ values is larger than for negative misalignments.”*
The asymmetry is almost not visible in Figure 7 to me. Perhaps quantify to make it more clear?

20. Line 327: “trust coefficient” -> “thrust coefficient”

21. Figure 8: Why have the authors chosen to only consider low thrust coefficients with a maximum of C_T=0.6? It is interesting to show higher thrust coefficients. For example, Heck *et al.* (2023) [2] found that the thrust should increase with yaw to reduce the power loss. However, it seems that their induction model (same as your Equation (14)) is less accurate at higher C_T.

22. Line 341: *"[...] and is capable of describing even relatively minor effects of the complex behavior of a misaligned wind turbine rotor in a sheared inflow."*
    I did not follow what the authors meant by this statement

23. Line 345: Since the rotor aerodynamic characteristics are necessary in your model, please include an Appendix which describes all relevant characteristics in this paper, so that the paper is self-contained.

24. What is the thrust coefficient of the experimental turbine?

25. Figure 11: It seems that a lot of tunable parameters are fit within this model. I am again unclear as to what is within sample of tuning and what should be considered as model validation (which requires out of sample data).

26. Figure 13: The results are summarized as having "very good" agreement with limited discussion, but there are several occasions where the model predictions are outside experimental uncertainty. It would be better to discuss these in detail.

27. Section 4.1: The results of this section align exactly with the published study of Heck *et al.* (2023) [2], who found that the thrust coefficient should be increased as the yaw is increased to reduce the power loss.

28. Section 4.2: How does the operation of the leading turbine affect the wake? The specific equations should be shown. It should affect the initial streamwise and spanwise wake deficits. This was done in Heck *et al.* (2023) [2] (Figure 9 and Appendix C).

29. Figure 15: It seems that there is almost no benefit from the modified model compared to baseline FLORIS, from the lines in Figure 15(f). Why is the additional benefit so negligible?

30. Line 444: *"The LES-ALM results confirm the findings based on the FLORIS engineering wake model: less power losses for the front turbine, and more gains for the downstream one."*
    Looking at Figure 15, this statement appears to be incorrect. It seems that the "Opt. (Model)" approach actually *increases* the turbine 1 power loss and increases turbine 2 power gain. Rather than decreasing power loss for the front turbine with more gains for the downstream one.

31. References: I am not sure why the authors have chosen to cite *arXiv* versions of papers that have been published before this present paper was submitted, but that must be corrected.

32. I recommend a title change, since other published papers have previously gone ‘beyond the cosine law.’ It is better to be specific about what contributions this paper contains.

References

[1] Shapiro, Carl R., Dennice F. Gayme, and Charles Meneveau. "Modelling yawed wind turbine wakes: a lifting line approach." *Journal of Fluid Mechanics* 841 (2018): R1.

[2] Heck, Kirby S., Hannah M. Johlas, and Michael F. Howland. "Modelling the induction, thrust and power of a yaw-misaligned actuator disk." *Journal of Fluid Mechanics* 959 (2023): A9.

[3] 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).

[4] 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.

[5] Archer, Cristina L., and Ahmad Vasel-Be-Hagh. "Wake steering via yaw control in multi-turbine wind farms: Recommendations based on large-eddy simulation." *Sustainable Energy Technologies and Assessments* 33 (2019): 34-43.