Paper Title:

Wind turbine wake detection and characterisation utilising blade loads and SCADA data: a generalised approach

General summary:

The manuscript presents a 3-stage methodology for turbine-based wind sensing, wake detection and wake characterization. Core of the methodology is a deliberate combination dimensionality reduction and machine learning methods, linking turbine data and wind field information. Both training and testing use aeroelastic simulations coupled with the dynamic wake meandering model and Mann turbulence. The wind sensing shows convincing results, both qualitative and quantitative. Wake detection and characterisation are mainly assessed qualitatively and show mostly good performance. The shortcomings of the methodology and possible improvements are discussed.

General comments:

1. The abstract is good and expressive!
2. Regarding the background chapter: The paper should be concise and focus on its main topic. A generic literature review of >6 pages is not appropriate in this context, especially since these topics are not picked up in the discussion section. The target audience can be expected to have a wind energy background. The interested reader will not choose this paper to learn about ABL, momentum theory or wake physics.

   Bottom line: It's suggested to remove section 2 "Background" and include the literature review with relevance to the paper topic (mainly contained in subsections 2.4 - 2.6) in the introduction section. (Accordingly, no specific comments were done for section 2 at this point).
3. The manuscript mixes past and present tense (e.g. in sections 3.2, 3.4, 3.5.2, 5.2). Please formulate in present tense where possible.
4. The language could be more concise. There are many instances of "As explained earlier, …", "First of all, …", "With that in mind, …", etc.
5. At many instances in the paper, simulation parameters and numbers of runs are mentioned (training/sensing tests/performance tests). Gathering all that information in one concise table would be helpful.
6. The manuscript has individual "Results" and "Discussion" sections, which is good. Yet, the results section already contains aspects that would belong in the discussion section (e.g. Line 488-496, 520-521, 541-542, 569-570). On the other hand, the discussion section is very brief and further lacks a comparison to existing methods. Please revise and make sure to have a clear distinction, possibly ending up with a shorter but more concise results section and a more in-depth discussion section.
7. In section 4.2 and 4.3 the wake detection is mainly assessed qualitatively and visually. The detection ratio (in Fig. 14&15) is the proportion of detected wakes out of all sample slices, but not with respect to a reference. Using the DWM model, the wake positions of the simulations should be available. It is suggested to use this information and show a quantitative performance metric of wake detection. Furthermore, it's suggested to use the RMSE of the estimated wake position as a metric of the characterization. Additionally, this information could help to unravel the unexpected behaviour of the detection at 5 m/s in Fig. 14.

Specific Comments:

Abstract

1. Line 9: “virtual wind farm” – Please state the test environment here. It should be clear from the beginning how the method is tested. Especially since the title does not tell whether it’s in field/simulations/wind tunnel.
2. Line 15: Partial wake conditions are not necessarily harder to track. The high load-imbalance along the rotor can even make it easier to estimate in comparison to a full wake. Your results, e.g. the findings of Fig. 11, do not seem to support this statement. It’s suggested to leave out that sentence.

Introduction

3. Line 19: “at the dawn of 2023” – please consider replace by “by end of 2023”
4. Line 35/36: Cyclic pitch control is just one type of “dynamic induction control”, which should be mentioned here as the general discipline. It splits up into Pulse and Helix. Frederik et al. focus on Helix. Please adjust and add a more general source.
5. Wind farm flow control techniques are mentioned in the introduction, but the connection to the tracking task within the framework of closed-loop control is lacking. The outlook and final role of the wake tracking and characterization should be mentioned.
6. Line 49-53: This is not entirely true. The approach of (Bottasso et al., 2018) is used for impingement detection and EKF-based approach in (Onnen et al., 2022) further links wake-presence to the observability. Yet, the 3-stage approach of this manuscript is a novelty and the justification for initial unbiased wind field reconstruction, as mentioned in line 57, is there. Please elaborate in the paragraph and differentiate between the approaches.
7. Line 55-56: “For this reason, the vast majority of methods developed so far are not yet applicable to real world operations.” This is a too strong statement, considering that there are field validations for these other methods, see (Schreiber et al., 2020), (Lio et al., 2021). As said in the previous comment: The research gap exists, but it is not accurately described in this introduction.
8. Line 65-67: “It is highlighted that focus of the current work is that of developing a solution which is able to confidently assert when a turbine is impinged by a wake from a nearby turbine, as this information is critical to farm level wake steering control.” – Please rephrase this sentence or make it two sentences.
9. Line 64-65: “A full end-to-end methodology is presented, which aims to provide both a demonstrator and performance benchmark for generalised wake detection and characterisation methods of this type.” – This would be a good place to mention the test environment (aeroelastic simulations, turbine type, DWM model, …).

Methodology

10. Line 295: why wind farm simulations, when only two turbines are used? Relating to intro: “generalized approach”
11. Line 290-295: Please state the turbine type, diameter and the spacing between “emitting” and “receiving” turbine.
12. It is unclear, whether the simulation environment includes just these two turbines or the whole wind farm.
13. Line 309: “Figure 6 illustrates the process for training the wind sensing model and demonstrates its post-training performance in producing wind field estimations.” The application scheme of the model is shown here, but not post-training performance. Please adjust the sentence.
14. Eq(1) & Eq(2) (and possibly others): don’t use italic font for sine and cosine and subscript text (except variables).

15. Line 344ff: This is the first time that higher harmonics are mentioned. The Coleman transform was only described for the 0P / 1P harmonics. Also: Fig. 7 names an “original load”, which suggests a (non-transformed) blade load. Meanwhile, it’s said in line 344 that the rotor loads are decomposed into their frequency components. Is it correct, that you e.g. calculate the 3P share of a yaw moment? Or do you calculate the 3P of a blade load? A flow chart of the pre-processing steps would help.
16. Line 351ff: Please elaborate on the temporal dependency and time lag. Which temporal scales of the turbine dynamics are you addressing here?
17. Subsection 3.3.3: The DTC is a nice choice and the dimensionality reduction shown in Fig.8 looks appropriate. My only point regarding the wind field parametrization is: The here shown YZ-wind field slices are rectangular, while the rotor swept area is circular. The corners of the wind field thus include non-observable features, but could still influence the lower-order share of the DTC outputs. Were the plain rectangular slices used for the training? Or was any weighting or masking applied? Please comment on whether you expect an impact here.
18. Section 3.3.4: Please add some more details and a literature source to the used regression approach.
19. Line 392ff: Is the distinction of the four classes based on the constellation of the simulation run or based on the instantaneous wake position (which could differ due to the employed DWM model). Also: Please define the overlap margins, from which you categorize full/partial/no wake impingement.
20. Section 3.5.1: The fitting function does not fully reflect the fit that was probably implemented. To fit a wind field with wake deficit, it should be U = u_ambient – f_G, here considering that parameter *A* is negative.
21. Section 3.5.2: How does the low-pass filter deal with falsely identified no-impingement instances?
22. Line 456: Please rephrase this sentence.
23. Fig. 11: Please add more details to the figure caption.
24. Section 4.1: a diff-plot between estimated and simulated wind field would help to analyse, whether systematic or just random differences exist (e.g. the central deficit mentioned in line 500)
25. Line 501-503: “A slight disagreement would not be normally problematic; however, in this case where the flow has little overall turbulence, a subtle deficit like this can’t ’hide’ among other eddies, which could potentially result in classifying the mentioned samples as being wake impinged.” This sounds rather complicated and nested. Please rephrase the sentence.
26. Fig. 14 shows that simulations for all wind directions are on hand. Please report this more explicitly in section 3.6.
27. Fig. 14 @ 5 m/s: why is detection ratio different here? In partial load range, the non-dimensionalized wake should be similar, thus limited impact on the sensing is expected. Also: It would be good to know the turbine’s cut-in wind speed. At 5 m/s undisturbed ambient wind speed, a wake-exposed turbine might experience a rotor-effective wind speed below cut-in.
28. Fig. 17 b&c) especially here it would be helpful to see the wake position reference from the simulation environment (see general comment 8).

Discussion

29. Line 591ff: “. Implementing other approaches such as Long short-term memory (LSTM) networks could potentially allow for the forecast to be extended to predict wake locations and meandering behaviour a few minutes ahead. Further research needs to be conducted to investigate these leads.” Please provide a source here and a stronger supporting argument for the claim that the (stochastic) meandering wake location can be predicted by the receiving turbine. Otherwise, please consider softening this statement.

References

30. Line 700: incomplete reference (journal, DOI)

References

Bottasso, C. L., Cacciola, S., & Schreiber, J. (2018). Local wind speed estimation, with application to wake impingement detection. *Renewable Energy*, *116*, 155–168. https://doi.org/10.1016/j.renene.2017.09.044

Lio, W. H., Larsen, G. C., & Thorsen, G. R. (2021). Dynamic wake tracking using a cost-effective LiDAR and Kalman filtering: Design, simulation and full-scale validation. *Renewable Energy*, *172*, 1073–1086. https://doi.org/10.1016/j.renene.2021.03.081

Onnen, D., Larsen, G. C., Lio, W. H., Liew, J. Y., Kühn, M., & Petrović, V. (2022). Dynamic wake tracking based on wind turbine rotor loads and Kalman filtering. *Journal of Physics: Conference Series*, *2265*(2), 022024. https://doi.org/10.1088/1742-6596/2265/2/022024

Petrovic, V., Jelavic, M., & Baotic, M. (2014). Reduction of wind turbine structural loads caused by rotor asymmetries. *2014 European Control Conference, ECC 2014*, 1951–1956. https://doi.org/10.1109/ECC.2014.6862484

Schreiber, J., Bottasso, C. L., & Bertelè, M. (2020). Field testing of a local wind inflow estimator and wake detector. *Wind Energy Science*, *5*(3), 867–884. https://doi.org/10.5194/wes-5-867-2020