Thank you for taking the time to review our manuscript and for your valuable comments, which have helped us enhance the quality of the paper. Below, we include your comments in **black**, followed by our responses in **blue**.

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

General Comments

This article presents a model for the detection and characterisation of wakes via estimating wind fields from blade load data. The model is well-explained, and the paper reads very clearly, with informative visualisations of the results. The approach developed here would be of interest to readers of this journal. There are a few areas that could be expanded on, including a deeper look into the accuracy of the wake impingement classification and wake characterisation. Additionally, a detailed flow-chart of the full process would guide others looking to reproduce these results.

Thank you, we appreciate the positive feedback. We agree that this methodology will hopefully be of interest to the readers and will contribute to the wake estimation field.

Specific Comments

1. Metrics / Accuracy:
    a) Is there a metric by which it is decided whether a wake is impinging on a rotor, e.g. a velocity deficit threshold? In line 39, "significant" impingement in terms of both magnitude and time is mentioned, but there is no further detail on the training data labelling for wake detection and classification. Could the details of how flow was classified as containing a wake be included in e.g. Section 3.4?
    For the purpose of this work, we did not specify such a metric, as there was no need to explicitly do so. It is largely due to the fact, that our wake detection model is based on classification by identifying patterns in the labelled data. The convolutional neural network learns the pattern of the wake deficit by being trained with an extensive sample library showing the four wake impingement conditions under varied wind speed and turbulence intensity values. Although such a 'black-box' approach is not based on physics of a wake deficit, it does offer high flexibility; with appropriate training data, it can easily capture various wake impingement conditions (e.g. multi-wake). Currently, the training data takes the 'wake impingement' samples from a single turbine at a fixed distance. Possibly, this will be expanded in the future. All in all, we agree that the original explanation of classes' definition could be improved, which is the section "Framework implementation: wake

detection" (2.4 in new numbering) was modified appropriately in the revised manuscript.

b) For the reported wake detection accuracy of 91% in Section 3.4, did this vary by "class" of impingement, e.g. was the model more or less accurate at predicting partial impingements? This may be relevant for future work using this model in wake steering controllers.

   No, this is an overall metric for all 4 classes. We agree that this section could use a mention of how accurate prediction of each class is. Consequently, we added a confusion matrix (Fig. 9 in revised manuscript) that describes the results of integrated CNN testing (done automatically during the training procedure) in more detail. We believe this will allow the reader to immediately realise what are the strong and weak points of trained neural network, making their interpretation of results more informed.

c) It would be informative to include accuracy metrics of related wind flow estimators or wake classifiers, to provide context for the model(s) developed in this paper.

   Although we do agree that this additional metric would be informative, we believe that adding a discussion subsection that would satisfy your comment is outside of the paper scope. The main novelty of this work is the introduction of a modular approach to the wake estimation problem – a generalised framework being a combination of several models. The models that we implemented can be easily swapped for something more accurate, and we want to encourage the community to use their methodologies this way.

   Furthermore, a direct comparison with other detection/ characterisation studies would be difficult due to fundamentally different assumptions. Other works that consider the wake detection aspect, such as (Onnen et al., 2022) or (Bottasso et al., 2018), do not test their solutions under a full range of wind conditions as we did. Existing simulation-based wake characterisation studies such as (Onnen et al., 2022) use different approaches for establishing the reference.

Results:

a) When the trained model was tested on a new receiver turbine in Section 4, were metrics for the accuracy of the wake detection or classification models calculated? In particular for the wake impingement predictions under 9% turbulence intensity, were the results in e.g. Figure 15(d) confirmed to be sensible given the increase in turbulence compared to training data?

   Excellent point, we agree that quantitative analysis would make this study better. We have spent significant time looking into the version of the DWM model that we're using, attempting to find the reference for wake detection as per the other reviewer's suggestion. When computing the wind field at the receiving turbine, multiple meandering wake deficit profiles are in general imposed on an otherwise clean "Mann box", including additional added turbulence determined by the shape of the deficit profiles. To combine multiple overlapping wake deficit profiles, some rule is applied pointwise, that is, independency at each relevant grid position in the wind field. The rule we used was "pick the maximum deficit at the point", but other rules are possible. As a result, the wake deficit profile that is applied to the Mann box does not necessarily have a simple shape with a well-defined location of maximal deficit. For majority of wind directions where the flow is coming from the inside of the wind farm, there are always some influencing turbines registered by the code – even if they are too far away to have a clear effect on the 'receiver' device. As a result of the above, a reference such as 'waked/not-waked for a given wind direction' is impossible to establish directly from the simulations. This is partially dictated by the nature of our study - we have tested the detection performance for all wind directions, where the wake shedding turbines are at various upstream distances, hence the rate of wake breakdown/lateral displacement is also varied. Furthermore, to the best of author's knowledge, there isn't a widely-recognised definition of 'wake impingement' (e.g. by means of reduced power output) we could use here. As a work-around to this issue, we have provided a reference for the quantitative analysis in a 'synthetic' way. We've trained a new classifier analogically to the process described in Methodology, with the only difference being that the training dataset is derived from simulated wind fields, not the estimated ones. Without the bias from the wind field reconstruction, this classifier achieves approx. 99% accuracy under integrated testing, and its classifications are thus assumed to be precise enough to become 'ground truth' reference. Such a reference allows to consider the effects of varied wake dispersion under different ambient conditions, and arguably fits this analysis better than a general impingement definition based on inflow angle. All in all, the goal of our wake detector is to identify "clear wake impingement from a nearby device" as we define it in the article. The reference as seen in Fig. 12 (revised manuscript) is sensible, as the impingement ratio decreases with increasing wind speed – just what would happen in real life, as operating in the above rated region makes wake less pronounced. We consider the limitations and bias from this approach in the Discussion section.

b) Is there an explanation for the "fake" wakes seen in Figure 17(c) / 13(a), or a proposed method to alleviate this? These simulated areas were mentioned as potentially resulting in mis-classification, is there any way to quantify how often this might occur?

Thank you for this comment, we agree that this aspect should be explained better. We believe that these 'fake' wakes originate from the training dataset selection. The wind sensing model was trained with simulations with a following distribution: 25% full impingement, 50% partial impingement, and 25% strictly ambient turbulence. By having one wind sensing estimator for all these cases, a bias is introduced – it is most likely a result of training the model with data where most cases show a wake. In the revised manuscript, we have introduced an additional discussion on that hypothesis (Discussion section, Framework performance/Wind sensing subsection). Moreover, we have added a confusion chart (Fig. 9 in the revised manuscript) that quantifies how many 'false positives' or 'false negatives' occur during the integrated testing of the classifier. This chart clearly shows that misclassification between 'full impingement' and 'no detectable impingement' is the main source of error, and gives a metric to how often this happens.

c) Did the superposition of wakes or the position of the turbine deep within the farm have any effect on model's accuracy in Section 4?

Firstly, we did not see a particular effect of wake superposition. As seen in the polar wake detection plots, the wakes originating from the eastern turbines (a single machine upstream) did not yield substantially different wake detection results than the wakes originating from the direct NW or SW neighbour. It appears that the primary aspect that decides on the wake detection performance is the distance between the 'receiver' and 'emitter' machines. In the revised manuscript, we have introduced an additional discussion on that aspect (Discussion, Framework's performance subsection).

2. Flow Chart: It would be very useful to have a more detailed flow chart (i.e. more indepth version of Figure 3) that includes the steps take for e.g. pre-processing to extract wind field from turbine blade loads, fitting of DCT factors, constructing wind fields, sampling frequency and fitting wake parameters.

Although we do agree that it's generally good to provide the reader with a comprehensive diagram describing the methodology, we believe that there is a better alternative here. A diagram like this could potentially overwhelm the reader – the purpose of this section is to introduce a modular way of thinking to wake estimation problem, not introduce the specific implementation details. We aim for this framework to be reused by the research community, and they can swap the models that we used for other methods that are leaner/better for their application. However, we do agree that a diagram that shows the rather complicated turbine response preprocessing would be very informative; we will discuss this further in your next comment.

3. Pre-Processing Diagram: A diagram of the turbine loads and how they are transformed would be informative in Section 3.3.

   As mentioned above, we agree that it would increase the readability, helping the reader understand the turbine response preprocessing. We have added an appropriate block diagram (Fig. 5 in the revised manuscript)

4. CNN Model: More detail on the CNN architecture is needed in Section 3.4.
   We have prepared an expanded description of a CNN (including examples, references, introducing its architecture components, and explaining what specific layers are responsible for) and put it in the Appendix A. In the beginning of the section, the readers are referred to it. The reason for this separation is brevity of the main article content. The scope of the paper is large, and the goal of our Methodology chapter is to provide the necessary implementation details and thus repeatability of this approach. The Appendix A serves as an introduction into the key concepts of CNN for readers unfamiliar with this technique. To address your comment, we have added a table (Table 3 in revised manuscript) describing the architecture in with the details necessary for implementation.
5. Conclusions: The conclusions are very brief, they should be expanded to include a summary of the accuracy of the models developed, as well as a short description of current limitations before future work.

   Excellent point, we have added a mention on the accuracy metrics of all models and addressed the current limitations.

Technical Corrections

1. General: Please ensure all acronyms are defined with capitalisation at the first use, and used consistently thereafter.

Excellent point, we have fixed the issue.

2. General: Please be consistent with using either double or single quote marks.

Sorted.

3. Line 25: Please clarify "the aforementioned task".

Sorted.

4. Line 27: “altered” does not give enough information about the features of waked flow that lead to higher loads, suggest re-wording to e.g. “experience a more turbulent wind field”.

Sorted, thanks for the suggestion!

5. Line 34: “yaw control” usually refers to control of a single turbine to follow the inflow, the standard term for farm-wide yaw optimisation is “wake steering”; suggest using this term instead.

Sorted.

6. Line 50: “to date”

Sorted.

7. Line 52: Typo: “turbine’s wake”

Sorted.

8. Line 65: Suggested re-word: “that the focus of the current work is to develop a solution”

Due to the feedback from the second review, we have already re-worded this sentence.

9. Line 87: Please include a reference for the microscale length scale.

For comments 9 – 22: General comment 2 from the second review suggested to transform the entire Background section into a shortened and more relevant literature review, and put it in the Introduction. We agree with this critique – in its original form, the Background mentioned a lot of concepts not being directly used in the rest of the work. As a result, this comment and several other below are referring to text which has been removed from the revised manuscript.

10. Figure 1: I think it should be $A1 = A0/(1 - a)$?

See comment 9.

11. Line 109: “as a wake”

See comment 9.

12. Line 110: Please include a reference for the “2-4 rotor diameters” statement.

See comment 9.

13. Line 119: Please include a reference for the Gaussian wake model relations.

See comment 9.

14. Line 125: I think “differs” is meant rather than “defers”?

See comment 9.

15. Line 135: The explanation around atmospheric shear and the location of maximum turbulence needs more detail.

See comment 9.

16. Line 145: For the infinite wind farm case, the power extraction from the turbines is balanced by entrainment of kinetic energy from the flow above; the explanation given here seems to reference increasing vertical height in the ABL?

See comment 9.

17. Lines 153 & 154: Unclear wording, is the data from the first 10 rows and first 8 rows of turbines per farm? And is the power loss between 45% and 70%?

See comment 9.

18. Line 181: Suggested re-word: "distinguishes the various impacts of turbulence"

See comment 9.

19. Line 187: Suggested re-word: "The widely-discussed method introduced by…"

See comment 9.

20. Line 196: Suggested re-word: "in incoming flow"

See comment 9.

21. Line 217: The phrase "accurate approximate estimation" does not make sense.

See comment 9.

22. Line 225: "wind farm flow control" for consistency.

See comment 9.

23. Line 264: This sentence is quite convoluted, please re-word.

Ultimately, we've decided that this sentence is not only convoluted, but it also doesn't serve much purpose here. This information is being given at several other instances in the Section. For this reason, we have removed it completely.

24. Line 276: What kind of evaluation metrics were used to determine the model had reached sufficient accuracy?

In this work, we wanted to propose a wake characterisation model that would output not just the wake centre position (as is the usual approach in other wake tracking studies, see e.g. Onnen et al., 2022), but also a measure of the impinged area. The methodology that extracts these two time series can be proved very useful to closed loop wind farm flow control schemes. Our original statement of 'sufficient accuracy' of this approach was not accurate, which is why we have reworded this sentence.

25. Line 298: Please specify whether "left" and "right" are as seen looking at the front or the back of the turbine.

Good point, added that information to the revised manuscript. The sentence now is: "Four wind directions, representing four distinctive wake impingement scenarios as seen from the front of the rotor, are defined as follows: (...)"

26. Line 314: A brief description of conditional dependence would be useful here.

We have added an enhanced explanation of how we apply the conditional dependence (lines 145-151 in revised manuscript). Readers interested in finding out more details are referred to the reference.

27. Line 315: Typo: "the following"

Due to the previous comment, the sentence was modified, so the typo is automatically resolved.

28. Line 348: Typo: "the the"

Sorted.

29. Line 354: "with a few"

Sorted.

30. Line 358: Could a brief list of all the inputs be given, either in the text or as a table, for clarity on what the 96 variables are?

We have added a table summary of the extracted features (Table 2 in revised manuscript).

31. Line 361: Suggested re-word: "are being processed"

Not exactly sure what do you refer to – in the original manuscript, the wording is "are being processed". Changed it to "are processed" for sharpness.

32. Line 382: More description of the "simple models" is needed.

To address your comment, we have expanded the description of our linear regression implementation. Moreover, we have added a source describing the localised linear regression in more detail. These can be found at lines 215-226 of revised manuscript.

33. Lines 396 & 411: It would be easier to read the proportions than the actual numbers of simulations, e.g. 10% instead of 1,120 on line 411.

Sorted, we have replaced 1,120 with 10% (line 258 in revised manuscript)

34. Line 414: Suggested re-word: “case was approximately 91%”

Sorted.

35. Line 430: Definition of the “rotation angle” needed.

Reworded the sentence to “ρ can be used to calculate the lengths of semi-minor and semi-major axis of the wake ellipse, as well as their orientation with respect to the YZ axes.”

36. Line 433: “D” has already been used as dimension e.g. “2D”.

To avoid confusion, in the context of dimensionality, we have replaced all instances of D with “-dimensional”. So for example, the “2D Gaussian fit” is now referred as “two-dimensional Gaussian fit”.

37. Equation 6 (Line 446): Is the integral missing dt?

Thank you for spotting this, sorted.

38. Section 3.6: This would make more sense as the first part of Section 4.

Sorted, moved this to the first subsection of Results section in the revised manuscript.

39. Line 460: Suggested added wording: “centrally located within the wind farm”

Sorted.

40. Line 480: “have a low mean RMSE”

Sorted.

41. Line 503: Suggested re-word: “simulations showed that”

Sorted.

42. Line 505: The term “amount of turbulence” is ambiguous, since the turbulence intensity has not changed but the wind speed has increased. Please re-word for a clearer explanation.

Due to the other comments, the Results section has been modified significantly. The sentence is no longer present in the manuscript.

43. Line 527: Suggest using “South and East” rather than “S and E”.

Sorted.

44. Line 534: Typo: “inverse”

Sorted.

45. Line 598: Suggested re-word: “after the consideration”.

Sorted.

46. Line 599: Suggested re-word: “Firstly, the wind farm flow control brings the largest benefits for below rated operation”

Sorted.

47. Line 620: Suggested re-word: “(and solution to some of”

Sorted.

48. Line 643: “2D” and “1D” without hyphen for consistency.

Sorted.

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

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