Dear Editor(s),

Please find enclosed the revised version of our previous submission entitled “Direct integration of non-axisymmetric Gaussian wind-turbine wake including yaw and wind-veer effects” with manuscript number WES-2024-107.

We would like to thank you and the reviewers for the valuable comments which have helped to improve the quality of our manuscript. Our response to the comments by Reviewer 1 are given below.

Sincerely,

Karim Ali, Tim Stallard and Pablo Ouro

Authors' Response to the Editor

Many thanks for progressing and accepting our manuscript.

We have modified the text and a couple of figures according to the recommendations of reviewer 1 (detailed below), and wherever relevant changed instances of e^x to $\exp(x)$.

Figures G1 and G2 in the manuscript are correctly named. What happened previously was that the Latex engine placed the figure before the title of Appendix G. We moved the figure now below the appendix title to avoid confusion.

We have also taken this opportunity to address an oversight in the data presented in the previous version. The values tabulated in table 3 were incorrectly missing a square root and division by the number of cases, which is the same for all cases. We have updated the values accordingly. This change has no impact on the drawn conclusions.

Below are our responses to the comments by Reviewer 1.

General Comments. I commend the authors for the extent of their revisions and the detail and rigor of this manuscript. It is clear that the authors conducted a significant amount of analysis (and all of the figures are very sharp and easy to read), and this work will be an excellent contribution to the analytical wake modeling literature. I recommend this manuscript for publication with some minor revisions, as noted below, concerning some clarifying remarks and the summary of the contributions in Sections 4 and 5.

Response:

We deeply thank you for your comments and efforts to improve the quality of our manuscript. We replied to your comments below.

Comment 1

Line 249: Is this ratio L/R = 0.9 similar to the other references (DiDonato and Jarnagin, Ali, Cheung) that use this circle-rectangle analogy? It would be helpful for the reader to know if the authors' reasoning is consistent with the other similar methods in the literature.

Response: Thank you for the comment.

- DiDonato and Jarnagin used the circle-rectangle analogy to quantify error bounds for their approximate solution of a circle, but did not use a constant value of L/R.
- Ali et al, used a ratio $L/R = 1$.
- Cheung chose $L/R = \sqrt{\pi}/2$ to maintain the same area between the circular and rectangular disks.

This question motivated us to look deeper into Eq. 33 (in previous version; 32 in current version), which is the source for the ratio 0.9. We found that the solution to this equation takes the form

$$\frac{L}{R} \approx \left(\frac{\sqrt{\pi}}{2}\right)^{\mathrm{erf}(2\sigma/R)}, \tag{1}$$

which can be further simplified by realising that for a typical inter-turbine spacing $2\sigma/R \gg 1$, leading to $\mathrm{erf}(2\sigma/R) \sim 1$. As such we used $L/R = \sqrt{\pi}/2$, which is approximately 0.886 i.e., very close to 0.9 which we previously used. Using $L/R = \sqrt{\pi}/2$ marginally improved the accuracy of the rectangular-disk solution.

Comment 2

Line 277: I believe the empirical expression for the wake expansion rate k* = 0.3837*TI +0.003678 comes from this Niayifar and Porté-Agel paper (https://www.mdpi.com/1996-1073/9/9/741) rather than the 2014 Bastankhah and Porté-Agel one referenced by the authors.

Response:

Thanks for noticing this. The reference has been amended accordingly.

Comment 3

Figure 2, Line 293: Is there any hypothesis for why the analytical predictions for the circular disk break down for rho=0, and why the rectangular integral performs better? Also, this trend is visible for x/D = 6-10, not just at 10D downstream.

Response: Thank you for the comment.

As discussed in lines 176–181 (previous version), to simplify the equations, the approximation $I_0\left(n\eta^2 R^2/(2\sigma_{\mathrm{ns}}^2)\right) \sim 1$ was employed. The implication of this is that the shearing and stretching of the wake contours at $\rho = 0$ were ignored, leading to an error that increases with x/D, as indicated in Fig. 2. We re-emphasised this in section 3.1 by including:

> The deviations between the circular-disk results and the numerical results at zero offset ($\rho = 0$ in Figs. 2c and 2d) is primarily related to the simplifying assumption in section 2.2 (Appendix C), where $I_0\left(n\eta^2 R^2/(2\sigma_{\mathrm{ns}}^2)\right) \sim 1$ was employed.

Comment 4

Figures 2-3: Would the authors consider combining Figures 2 and 3, as they have done with the data in Figure 4? I'm wondering if it would be easier to compare the rectangular and circular disks if they are plotted on the same figure. If the authors find that this presentation would be too cluttered, I think it is fine as is.

Response: Thank you for the comment.

We have updated the figure by combining Figs. 2 and 3 into one figure (Fig. 2 in updated manuscript). As such, we also combined sections 3.1 and 3.2 to be section 3.1 (updated manuscript), which considers verification for both circular and rectangular solutions.

Comment 5

Section 3: There are a few points throughout this section where the authors refer to the accuracy of the analytical predictions relative to the numerical predictions (for example, "high accuracy" at line 322), and I think these claims could be supported by some quantitative information to clarify to the reader what the authors consider to be "accurate" versus "inaccurate": perhaps the maximum relative error of the rotor-averaged deficit predictions? I know that the authors do look at RMSE in Section 3.7 but it is specifically for different resolutions and distributions of averaging points.

Response: Thank you for the comment.

Following this suggestion, we included the mean error and maximum error between the analytical and numerical solutions for the verification cases in section 3.

For the verification of the circular-disk solution:

> The mean absolute error (difference between analytical and numerical solutions) for the circular-disk solution is approximately 7.2×10^{-3}, with a maximum error of 22.5×10^{-3} occurring in the case of zero offset ($\rho = 0$).

For the verification of the rectangular-disk solution:

> Specifically, the mean error for the rectangular-disk solution is approximately 2.7×10^{-3}, which is approximately a third of that of the circular-disk solution, with a maximum error of 7.2×10^{-3}.

For the section on veer effects, we added:

... both the circular- and rectangular-disk solutions match the numerical solutions with high accuracy with a maximum error of 8.1×10^{-3} for the circular disk and 5.2×10^{-3} for the rectangular disk.

Specifically, the circular disk has mean and maximum errors of 4.5×10^{-2} and 10^{-1}, respectively, which is 1–2 orders magnitude higher than the errors in the case of $\Delta\alpha_o = 5°$. Conversely, the rectangular-disk solution maintains higher accuracy with mean and maximum errors of 3.9×10^{-3} and 10^{-2}, respectively.

Of slightly less accuracy than the lower veer cases, the mean and maximum errors for the rectangular disk are 9×10^{-3} and 1.7×10^{-2}, respectively, which are more accurate than the circular disk results at $\Delta\alpha_o = 15°$. The larger error for this case ($\Delta\alpha_o = 45°$) compared to the previous two cases is primarily due to the empirical expression for the size of the rectangular disk (Eq. 33). Higher accuracy could be achieved if the size of the rectangular disk is optimised, even though current accuracy is acceptable.

Comment 6

Section 4: Overall, I find this section to be a bit repetitive considering the amount of detail and discussion throughout Section 3. There are some new ideas presented here: the modified definition of the coordinate frame in Eq. 37, the physical meaning of different averaging orders in line 453, the superposition of multiple wakes discussed around line 493, the partial waking discussion at line 516 (also mentioned in my next comment), and the nacelle wind speed deficit at line 531. However, much of the text that I haven't highlighted here is a summary of the results presented immediately before in Section 3. My suggestion is to expand Section 5 to include much of this summarization, which would streamline the ideas presented in Section 4 and make the paper feel less dense towards the end.

Response: Thank you for the comment.

Following your suggestion, we shortened section 4 significantly by relocating the repetitive parts to the summary in section 5.

Comment 7

Line 514: I think two ideas are being combined here. First, there is the definition of the wake velocity deficit and the wind speed used for normalization: zero velocity deficit means that the local wind speed is assumed to be equal to the wind speed at the location of the upstream turbine. The authors point out that this assumption—which applies to both averaging techniques—could be invalid for wind farms with heterogenous flow fields. Second, there is the "partial waking" effect, which traditionally refers to how the wake region only partially overlaps with the rotor swept area (https://wes.copernicus.org/articles/7/433/2022/), leading to a nonuniform distribution of velocity across the rotor. The averaging process across the rotor swept area is very important in these cases of partial wake overlap. I don't fully understand the connection the authors are trying to make in this paragraph—how exactly does the wake deficit normalization factor relate to the partial waking issue?

Response: Thank you for the comment.

We agree our phrasing in this paragraph was confusing. Since the considered wake is assumed Gaussian, which is a continuous field rather than the discontinuous top-hat wake, the integration is taken across the part of the wake field occupied by the whole rotor, and is thus directly applicable to partial waking. As such, the limitation is not partial waking itself but flow heterogeneity within the farm. If the flow is highly heterogenous, the wind speed of a point of zero deficit on the turbine rotor is not necessarily the same as the wind speed of the upstream turbine. Nonetheless, all engineering wake models of this type have limitations in predicting the wake interactions with the heterogenous background flow. We omitted the part referring to partial waking, and this paragraph now is:

> Some limitations should, however, be considered. The rotor-averaging process inherently assumes that a zero-deficit point on the rotor disk has a wind speed that is equal to that of the upstream turbine (wake source), rather than the free-stream wind speed or another background wind speed. This can have profound impacts on the rotor-averaged wind speed in the case of highly heterogeneous flow within a wind farm, such as in the case of hurricanes or extremely large wind farms. In such a scenario, all numerical and analytical approaches based on engineering wake models have shortcomings as the underlying assumptions of the wake-deficit model cannot predict the interactions between the wakes and the heterogeneous background flow.