Review of: A generalised Gaussian wake model based on extended actuator disc theory

Summary

The manuscript presents a model for estimating the location of the start of a wind turbine's far wake based on the assumption that this occurs when the streamwise gradient of the kinetic energy along the wake's centerline is at its maximum value. Additionally, the proposed model assumes that the wake's shape at the defined location is of a Gaussian shape, which is used alongside mass and momentum conservation to estimate flow evolution inside the wake and in the bypass flow using a marching scheme. The model is compared to RANS of a uniformly-loaded actuator disk placed in a rectangular channel for different values of blockage ratio, turbulence intensity, and turbine's thrust coefficient. The language is mostly clear and readable, with a few locations that would benefit from further clarifications as listed below.

I split my comments below into major and minor comments. The major comments are focused on both the underlying physics and mathematical consistency of the model. The minor comments are listed to highlight where further clarification is required to avoid readers' confusion.

Major Comments

1. **Definition of the near-far wake location**
   In the manuscript, the authors effectively used two approaches to define the location where the near wake ends and the far wake begins (denoted x_3). The first comes from assuming that at x_3, the wind-speed deficit has a Gaussian shape (Eq. 6), which is acceptable. The second approach assumes that at x_3 the right-hand side of the momentum equation: $-\partial_x p + \nu_t \nabla^2_{yz} u$ is maximum, where $\nabla^2_{ij} = \partial^2_i + \partial^2_j$. This is equivalent to assuming that at x_3, $\partial_x u^2$, the gradient of the kinetic energy is maximum. The authors suggest that this definition marks an inflection location where the wake starts to recover in a decelerating manner, and that this behavior marks the start of the far wake.

   However, there was no justification presented that these two definitions coincide. In fact, some of the RANS results presented in the manuscript indicate otherwise. For example, the low turbulence case in Fig. 12 shows that at the location x_3, obtained based on the second approach, the wake is not fully Gaussian yet and retains some of the flatness of the top-hat profile characteristic of the near-wake region. The authors need to present a justification that x_3 truly marks where the wake becomes sufficiently Gaussian.

   A minor note here is that, to call x_3 an inflection point of the wind-speed deficit, one has to show that $\partial^2_x u(x_3) = 0$. While this is partially visible in Fig. 4, a clear figure showing the second streamwise gradient of velocity would be affirming to the nomenclature.

   It is an interesting observation that the location x_3, where $\partial_x u^2$ is maximum, occurs when

   $$\mathcal{R}(x) = \int_{-\infty}^{x} \mathcal{R}(\xi)\ d\xi, \tag{1}$$

   where $\mathcal{R} = \nu_t \nabla^2_{yz} u$. It would be greatly beneficial if the authors can discuss the physical meaning of this observation more as it is the cornerstone of the proposed model. Particularly, it is curious to know if this observation is affected by the RANS definition of the eddy viscosity ν_t obtained from the k-ε model. If resources and tools are available, it would be interesting to test this using LES without modeling the Reynolds stresses as done in RANS. This can be done by analyzing $\mathcal{R}_{\text{LES}}$ such that

$$\mathcal{R}_{\text{LES}} = \partial_j \overline{u_1' u_j'}. \tag{2}$$

2. **Is the quantity $\mathcal{R}$ continuous across the turbine disk?**
In the derivation, the authors integrated the momentum equation $u\partial_x u = -\partial_x p + \mathcal{R}$ upstream and downstream of the rotor disk separately, which is the correct way to handle the discontinuity in pressure across the idealized actuator disk. However, in Eq. 15, it was assumed that $\mathcal{R}$ is continuous across the disk, and hence the separate integrals of $\mathcal{R}$ where combined into a single integral. It appears from the reported RANS results that $\mathcal{R}$ is changing rapidly across the disk, but one cannot tell if $\mathcal{R}$ is continuous or not just from looking at the RANS results because the turbine is simulated as a finite-thickness forcing and because of numerical dissipation. I encourage the authors to discuss this more and perhaps support the modeled behavior of $\mathcal{R}$ in RANS with resolved behavior from LES. To safely combine the two integrals of $\mathcal{R}$ into a single integral, $\mathcal{R}$ must be continuous and integrable across the rotor disk.

3. **Equation 23 is incorrect**
The condition in (1) is the 'chosen' definition of the location x_3. Therefore, at $x = x_3$,

$$\partial_x \mathcal{R}(x_3, \nu_t) = \mathcal{R}(x_3, \nu_t). \tag{3}$$

This condition can be written as the zero of a residual function

$$F(x_3, \nu_t) := \partial_x \mathcal{R}(x_3, \nu_t) - \mathcal{R}(x_3, \nu_t) = 0. \tag{4}$$

Hence, differentiating the identity $F = 0$ with respect to ν_t gives

$$\frac{\mathrm{d}F}{\mathrm{d}\nu_t} = 0 = \partial_{\nu_t} F + \partial_{x_3} F \times \partial_{\nu_t} x_3. \tag{5}$$

Assuming $\partial_{x_3} F \neq 0$, the implicit-function theorem gives

$$\partial_{\nu_t} x_3 = -\frac{\partial_{\nu_t} F}{\partial_{x_3} F}. \tag{6}$$

This expression has the same formal structure as Eq. 22 in the manuscript. However, Eq. 22 appears to use $\mathcal{R}(x_3, \nu_t)$ itself as the residual, rather than the residual $F = \partial_x \mathcal{R} - \mathcal{R}$. This is a critical distinction: Eq. (6) follows from the condition $F = 0$, whereas $\mathcal{R}(x_3, \nu_t) = 0$ is not the defining condition for x_3, which is correctly described by (3). Therefore, Eq. 22 in the manuscript applies the implicit-function theorem to the wrong function. The theorem requires a zero residual, and (3) defines this residual as $F = \partial_x \mathcal{R} - \mathcal{R}$. It does not define $\mathcal{R}(x_3, \nu_t)$ as a residual, nor does it imply $\mathcal{R}(x_3, \nu_t) = 0$. Hence Eq. 23 in the manuscript is not a consequence of the condition in (3).

4. **Equation 24 and the constant of integration**
An important consequence of the last point relates to Eq. 24 and the statements in lines 231–234. It is argued that the constant of integration can be obtained from analyzing the limiting case of $\nu_t = 0$. When $\nu_t = 0$, we have $\mathcal{R}(x) = 0$ by definition, which is a trivial solution of (3). Consequently, the condition used to define x_3 degenerates into the identity $0 = 0$ and no longer selects a unique value of x_3. Therefore, x_3 cannot be determined from (3) in the limit $\nu_t = 0$.

The authors appear to use the mathematical definition of x_3 and its proposed physical interpretation interchangeably. Physically, one may argue that, in the absence of turbulent diffusion, the wake does not recover and the start of the far wake is pushed to infinity. However, this does not follow from the mathematical condition defining x_3. The quantity x_3 is not independently defined as the location where the near wake ends; it is defined as the location where (3) holds. When $\nu_t = 0$ and $\mathcal{R} = 0$, this condition becomes vacuous. Thus, the limiting argument used to determine the integration constant is not mathematically well posed: in the limit $\nu_t = 0$, the defining equation loses its ability to select x_3.

The same analogy holds for the case when ν_t is large. The mathematical definition of x_3 in (3) does not on its own provide a boundary condition for x_3 at large ν_t. Additionally, it is not clear how, under the assumption that $x_3 = 0$, that this translates to

$$\mathcal{R}(0) = \int_{-\infty}^{\infty} \mathcal{R}(\xi)\ d\xi. \tag{7}$$

This is confusing because from (1), we have

$$\mathcal{R}(0) = \int_{-\infty}^{0} \mathcal{R}(\xi)\ d\xi. \tag{8}$$

Furthermore, alongside the assumption that in the case when ν_t is large we have $x_3 = 0$, how does it follow that this translates to a fully recovered wake at the centerline ($\alpha = 1$)? A minor point here is to mention that the location of the turbine was defined earlier in the manuscript as $x = x_2$ not $x = 0$.

5. **Momentum conservation condition**
Section 3.6 requires substantial clarification. The manuscript defines x_3 through (3) and then treats this location as the onset of the Gaussian-wake region, but the justification for this interpretation is not provided. The later modification of the momentum-conservation condition appears to be introduced in response to discrepancies with the RANS results, rather than derived from the preceding analysis. This is problematic because the argument that a Gaussian-wake profile overestimates the deficit near the wake boundaries does not, by itself, justify modifying a global momentum-conservation condition. The relationship between the discussion beginning at line 287 and the proposed modification beginning at line 295 should therefore be made explicit. In its current form, the reasoning is incomplete, and the proposed modification lacks a clear mathematical or physical basis.

Minor Comments

1. The introduction section lacks surveying wake models that adopt a marching approach starting from an initial wake stamp. This is highly relevant since the proposed model is also a marching scheme.

2. The yellow curve is not visible in Fig. 2.

3. The caption of Fig. 2 mentions velocity gradient, but it is not shown in the figure.

4. Line 178: Eq. 17 was derived from a momentum balance not form an energy budget.

5. Line 260: Please include a clear procedure supported by the relevant equations of how these are computed.

6. Line 288: What do you mean by the 'simplified momentum equation'?

7. Figure 5: Please indicate at what location x where these profiles plotted? Is it at x_3?

8. Line 425: This is not a finding, it is a modeling choice. The location x_3 was 'defined' to satisfy the described condition.