Reviewer 1 response

The authors wish to thank the referee for the time and effort spent in reviewing the manuscript, and for providing feedback to help improve the study. Below please find our responses to the reviewer's remaining questions and comments.

9. **The authors have chosen to not add any additional cases representing different TI values and thrust coefficients. While this is okay, I think that the authors should at least mention in the conclusion and abstract, that the present results represent only two inflow cases that are similar in TI and correspond to below-rated thrust coefficients. More cases need to be investigated in future work in order to obtain general conclusions.**

   We agree with the reviewer that additional cases with different wind speeds and TI values should be included in future calibration studies. In the updated manuscript, we have mentioned this limitation and noted that more cases should be considered to obtain more generally calibrated parameters and more universally applicable conclusions.

11. **The authors have added the Coriolis force in the momentum equation. However, I think it should not be present in Eq. 1d (w momentum equation), as most microscale modellers use the f-plane assumption.**

    The Coriolis force has been removed from equation 1d.

    Furthermore, the authors write in the revised article:

    "For the cases considered in this study the Coriolis body forces are not expected to cause a large deflection of the wake over the distances of interest. In the offshore wind turbine cases of section 4, the calculated Rossby numbers based on turbine diameter and hub-height wind speed were relatively large (Ro 90 - 400) and the impact of the Coriolis forces were minor, which is consistent with the observations of Heck and Howland (2025)."

    Note that the addition of the Coriolis force leads to two main effects: Coriolis-induced wind veer and a spatial variation of the Coriolis force due the wind turbine wake velocity deficit. The authors correctly mention that the latter effect is small for single wakes and small wind farms, however, the effect of Coriolis-induced wind veer cannot be neglected. I think the authors are aware of this, but it should be more clearly written in the article when the effect of Coriolis force is discussed. More info about these two effects can be found in van der Laan and Sørensen [2017], Gadde and Stevens [2019]. The authors could also remind the reader in this discussion that the wind veer is added directly from the LES precursor.

    In the updated text of the manuscript, we have included additional language clarifying the two main impacts of the Coriolis force as mentioned by the reviewer. We have also included mentions to Gadde and Stevens [2] and van der Laan and Sørensen [3] for additional discussions on the topic. In addition, we have also added reminders in the text that the veer profiles are derived from the LES precursor simulations, and not calculated directly.

Regarding the use of C_k, I am still of the opinion that this is not an independent parameter, and it is effectively a different C_μ which will lead to an imbalance if none of the other turbulence model constants are readjusted. I am surprised to see that the present model seems to have a minor downstream development of the inflow over a distance of 20D, but it may be related to the use of a parabolic model framework. Based on my experience with elliptic RANS models, the imbalance would be much larger. The authors do not need to take any further action regarding C_k, as they have shown that it works for their application, but it is not something that I would recommend to use for elliptic RANS models.

Additional clarification on this topic has been added to the most recent version of the manuscript. Note that the use of C_k only appears in the inflow profiles in equation (13), and in the application of the lower TKE boundary condition, it is a function of C_μ only – this has now been clearly rewritten and stated in the text. We have also added clarifying statements that the use of the initial profile and the C_k profile would not apply to the more general problem with elliptic RANS models.

13. **The authors have updated the TKE plots by showing $\sqrt{2/3 \times TKE}/U$, which is nice to see. However, I did notice that the updated plots have TI values, outside the wake, that do not match the LES cases (see Fig. 7 and Tab. 3). For the freestream flow, the normal Reynolds-stresses of the RANS model are isotropic; $\overline{u'u'} = \overline{v'v'} = \overline{w'w'} = 2/3 \times TKE$, hence, $TI_{TKE} = \sqrt{2/3 \times TKE}/U = \sqrt{uu}/U = TI_u$. Reading the RANS TI_u results in Fig. 7b (Med WS case), I get $TI_u = 0.04$. However, the LES TI_u results of the inflow is 0.031 (Tab. 3).**

    Why is the RANS inflow model not calibrated to obtain the same inflow TI_u as LES? This can be done by tuning z_0 or by changing C_μ. Another issue with this mismatch in freestream TI, is that the obtained wake TI from RANS seems to match with the LES wake TI, but if one would subtract the inflow TI, then resulting wake-added TI would not match between the RANS and LES models (in the far wake).

    The reviewer raises an excellent point regarding the comparison of the RANS and LES TKE fields. We agree that there is a small mismatch in the free-stream hub-height TKE values shown in figure 7. However, some caution must be exercised when directly comparing the TKE variable from the LES calculation with the counterpart TKE from any RANS calculation. The reviewer correctly notes that the normal Reynolds stresses calculated in the RANS are isotropic. However, in the LES calculation and in the measured lidar data, the free-stream turbulence is notably non-isotropic for stable ABL conditions (see Cheung et al [1] for additional details), and the $\overline{u'u'}$ components are significantly stronger than the $\overline{w'w'}$. Thus an exact match between the RANS and LES TI_{TKE}, as directly computed from each simulation, would not imply the turbulent fluctuations are resolved correctly. While various weighting schemes can be applied to the RANS TI_{TKE} in an attempt to match the anisotropic TKE from LES, fully addressing this question is beyond the scope of the current work. In the revised manuscript, we have clearly indicated this limitation of the TKE comparisons: the RANS and LES TKE figures are meant to provide a qualitative indication of the wake turbulence distribution and magnitude, and should be further refined with additional exploration of the closure model and appropriate calibrations.

References

[1] Lawrence Cheung, Michael J. Brazell, Alan Hsieh, Shreyas Ananthan, Ganesh Vijayakumar, and Nathaniel deVelder. *Computation and comparison of the stable Northeastern US marine boundary layer*. 2021.

[2] Srinidhi Nagarada Gadde and Richard J.A.M. Stevens. Effect of coriolis force on a wind farm wake. *Journal of Physics: Conference Series*, 1256(1):012026, jul 2019.

[3] M. P. van der Laan and N. N. Sørensen. Why the coriolis force turns a wind farm wake clockwise in the northern hemisphere. *Wind Energy Science*, 2(1):285–294, 2017.

Reviewer 2 response

The authors appreciate the reviewer's time and efforts in evaluating the manuscript and for providing feedback to help improve the quality of the study. Our responses to the feedback are included below.

1. **The authors claim that adding more recent wake models for baseline comparison beyond the axisymmetric Gaussian wake model is out of scope because those models need to be calibrated to have a fair comparison, and that this is "not possible within the scope of the current study." I strongly disagree with the authors.**
   The authors should include more recent models for comparison as their results largely focuses on the new model's ability to capture 'veer' which has been comprehensively addressed in recent studies (e.g., `https://www.mdpi.com/1996-1073/11/7/1838` **and** `https://pubs.aip.org/aip/jrse/article/17/3/033302/3347781`**). The authors could perform a very simple calibration, or they could present results from the more recent models with a few variants of the parameters as a simple sensitivity check. More clear and comprehensive comparison to existing models is important to actually understand the contribution of this work.**

   In section 4.5 of the revised manuscript, a comparison of the SANDWake3D model against the semi-analytical model of Abkar et al [1] is now included. As discussed in greater detail in question 2 below, the comparison contrasts the near wake behavior using the Joukowski disk model in SANDWake3D against the semi-analytic, Gaussian-based model and better highlights the differences between the two approaches.

2. **In the introduction, the authors frame their contributions relative to previously developed and published parabolised RANS models: WakeBlaster (2020) and the "combined curl model" (2021, note that I think the authors have the name wrong and they mean the curled wake model?). As an aside, more recent improvements to these parabolised RANS formulations are not mentioned. But more generally, the authors claim that their model is an improvement compared to WakeBlaster and the "combined curl model."**

   In the revised manuscript, the terminology has been updated to refer to the model of [4] as the curled wake model.

   The innovation claimed over WakeBlaster is because this new model is 3D and therefore captures veer. I don't fully agree with the authors here, because they use surface layer equations and turbulence modeling, they seem to be only capturing the advection by veer. Again, this is a very simple mechanism and was done by Abkar (2018) https://www.mdpi.com/1996-1073/11/7/1838.

   The innovation claimed over the "combined curl model" is based on this point: "However, one of the more unexpected findings of the current study was the fact that an actuator disk model is compatible with the parabolized

RANS formulation itself, and that previous attempts to initialize the turbine wake by explicitly subtracting a wake deficit profile are unnecessary." I again don't quite agree with the authors here, and I don't believe the results they have shown supports their claims. The authors argue that they can directly include actuator disk forcing in the parabolised RANS formulation. While that is practically true, it is not clear that the physics are being solved for correctly. The equations being solved in the near vicinity of the actuator disk in this parabolised RANS model are not correct because the elliptic properties of the pressure are not handled correctly. This is the purpose of the "wake deficit profile" formulation in other studies, which circumvents the challenge of pressure in the near turbine vicinity which requires an elliptic pressure solve. The authors have not demonstrated that their actuator disk forcing approach based on an incomplete pressure solution is actually more accurate nor more reliable than the "wake deficit profile" solution. It's possible it could be, but no evidence of that has been shown in this paper.

The capabilities the SANDWake3D model, relative to the other wake models, are now explicitly discussed in the revised manuscript. In section 4.5, the results of a recent implementation of the Joukowski actuator disk model in the SANDWake3D code are compared to the semi-analytical model by Abkar et al [1], and some differences are immediately apparent. Following the work of Sorensen et al [5], this actuator disk model determines the rotor loading by assuming constant circulation on the rotor disk, with a root and tip loading correction applied. This allows for a more realistic loading profile compared to the uniformly loaded actuator disk model. Both axial and azimuthal forces are calculated on the disk, so the resulting wake deficit and swirl velocities are captured through the model.

The differences between the wake calculated using this approach and the model of Abkar et al are clearly shown in figures 1 and 2 for the Low WS, single turbine case. In the near wake region, the lower loading near the blade root leads to the formation of a small high velocity region near the centerline. This is present in both the LES and the RANS calculations, but absent in the Abkar et al model, which uses a Gaussian wake deficit velocity profile that is not applicable in the near wake region. The presence of the tangential forces in the LES and the RANS models also leads to the presence of an azimuthal swirl velocity in the near wake fields. This swirl/veer interaction leads to an lateral asymmetry in the wake velocity deficit profile (shown in figure 2(a) and 2(b)). These effects are not included in the Abkar et al model as it only calculates the axial wake velocity deficit.

For these calculations, note that the RANS calibration parameters C_μ, $C_{\epsilon 1}$, $C_{\epsilon 2}$, $C_{\epsilon 3}$, and C_k were unchanged, and an estimated value of $r_\delta = 28$m was used in the Joukowski model. In the Abkar et al model, an estimated value of $k_y = k_z = 0.035$ was used to provide generally consistent velocity profiles in the far wake region.

Several important comments may also help clarify the applicability of the current formulation. First, a critical distinction between elliptic effects and the convective processes that appear in a wake flow should be made. The presence of a turbine or other obstruction in a flow field creates both a induced pressure field, and establishes a velocity difference between the freestream and the downstream centerline. The induced pressure (or *blockage*) field travels both upstream and downstream from the turbine, but rapidly decays with distance and its impact is minimal a few diameters upstream of the turbine. Generally speaking, this pressure field can be neglected for the purposes of determining the overall behavior wakes, otherwise, any parabolic approach, including WakeBlaster would be invalid. The latter effect, establishing a velocity difference, usually leads to a convective wake which predominantly travels in the downstream directly. Under specific circumstances, e.g., when there is strong reverse flow or when specific regions of the wake become absolutely unstable, the flow development can also travel upstream. A rigorous characteristics analysis of the parabolic flow equations is possible (see [3] for example), but the importance of the latter over the former phenomena allows the force term in parabolic RANS equations to capture the general wake development. An analogous situation can be seen in previous studies of compressible jets and mixing layers [2]. There, the nonlinear forcing term allowed the parabolized stability equations to faithfully replicate the development of ostensibly elliptic phenomena such as vortex roll-up and vortex pairing, but failed to capture upstream acoustic propagation due to similar limitations.

A second clarification should also be stated regarding the use of a body force to represent the turbine actuator compared to the direct imposition of a velocity deficit profile. The direct enforcement of a velocity deficit profile at a downstream location avoids some additional computation, but is tantamount to including a body force or pressure gradient term in order for momentum to remain balanced. For the streamwise velocity given at two streamwise stations, u_n upstream of the turbine and u_{n+1} downstream, an notional conservation of equation in the x direction can be written as

$$\frac{u_{n+1} - u_n}{\Delta x} = \frac{1}{U}\left[-\frac{1}{\rho}\frac{\partial p}{\partial x} - V\frac{\partial u}{\partial y} - W\frac{\partial u}{\partial z} + \frac{\partial}{\partial y}\left(\nu\frac{\partial u}{\partial y}\right) + \frac{\partial}{\partial z}\left(\nu\frac{\partial u}{\partial z}\right) + f_{eff}\right] \quad (1)$$

Because u_n represents the given inflow, and u_{n+1} is also directly enforced and not solved for, balancing the right hand side of equation (1) requires the introduction of an effective force f_{eff}. This effective force can be viewed as roughly analogous to the forces applied in an immersed boundary method in order to achieve a desired velocity at a boundary. However, without careful consideration of the wake velocity profiles u_{n+1}, the effective forces f_{eff} can become uncorrelated with the forces actually imposed by turbines in the flow field. This situation is further complicated when dealing with multiple turbines with multiple wakes superimposed on one another.

The approach of SANDWake3D avoids this problem by explicitly formulating the body forces to correspond to the turbine forces on the rotor disk. As seen in the results in the current study, this leads to a consistent velocity profiles in the near and far wake, turbulence fields, as well as the actuator disk forces.

3. **Finally, I personally find that the model results compared to LES are not very clear nor compelling because so much error comes from differences**

between disk and line modeled turbines between the codes. Meanwhile, results compared to FLORIS are not compelling because the authors selected a model that explicitly does not capture the veer they set in their model. I think the paper would be much stronger if this issue of turbine representation in LES was resolved rather than being "future work." This last point is just my opinion, but naturally the authors may proceed as they wish.

As mentioned in the response above, additional results were included in the revised manuscript which demonstrated the accuracy of the Joukowski disk model compared to LES and other semi-analytic models [1]. This removes one item from the future work stated in the conclusions; however, additional models with increasing sophistication can still be explored.

References

[1] Mahdi Abkar, Jens Norkaer Sorensen, and Fernando Porte-Agel. An analytical model for the effect of vertical wind veer on wind turbine wakes. *Energies*, 11(7), 2018.

[2] Lawrence C Cheung and Sanjiva K Lele. Linear and nonlinear processes in two-dimensional mixing layer dynamics and sound radiation. *Journal of Fluid Mechanics*, 625:321–351, 2009.

[3] Hossein Haj-Hariri. Characteristics analysis of the parabolized stability equations. *Studies in Applied Mathematics*, 92(1):41–53, 1994.

[4] Luis A Martínez-Tossas, Jennifer King, Eliot Quon, Christopher J Bay, Rafael Mudafort, Nicholas Hamilton, Michael F Howland, and Paul A Fleming. The curled wake model: a three-dimensional and extremely fast steady-state wake solver for wind plant flows. *Wind Energy Science*, 6(2):555–570, 2021.

[5] Jens Nørkær Sørensen, Karl Nilsson, Stefan Ivanell, Henrik Asmuth, and Robert Flemming Mikkelsen. Analytical body forces in numerical actuator disc model of wind turbines. *Renewable energy*, 147:2259–2271, 2020.

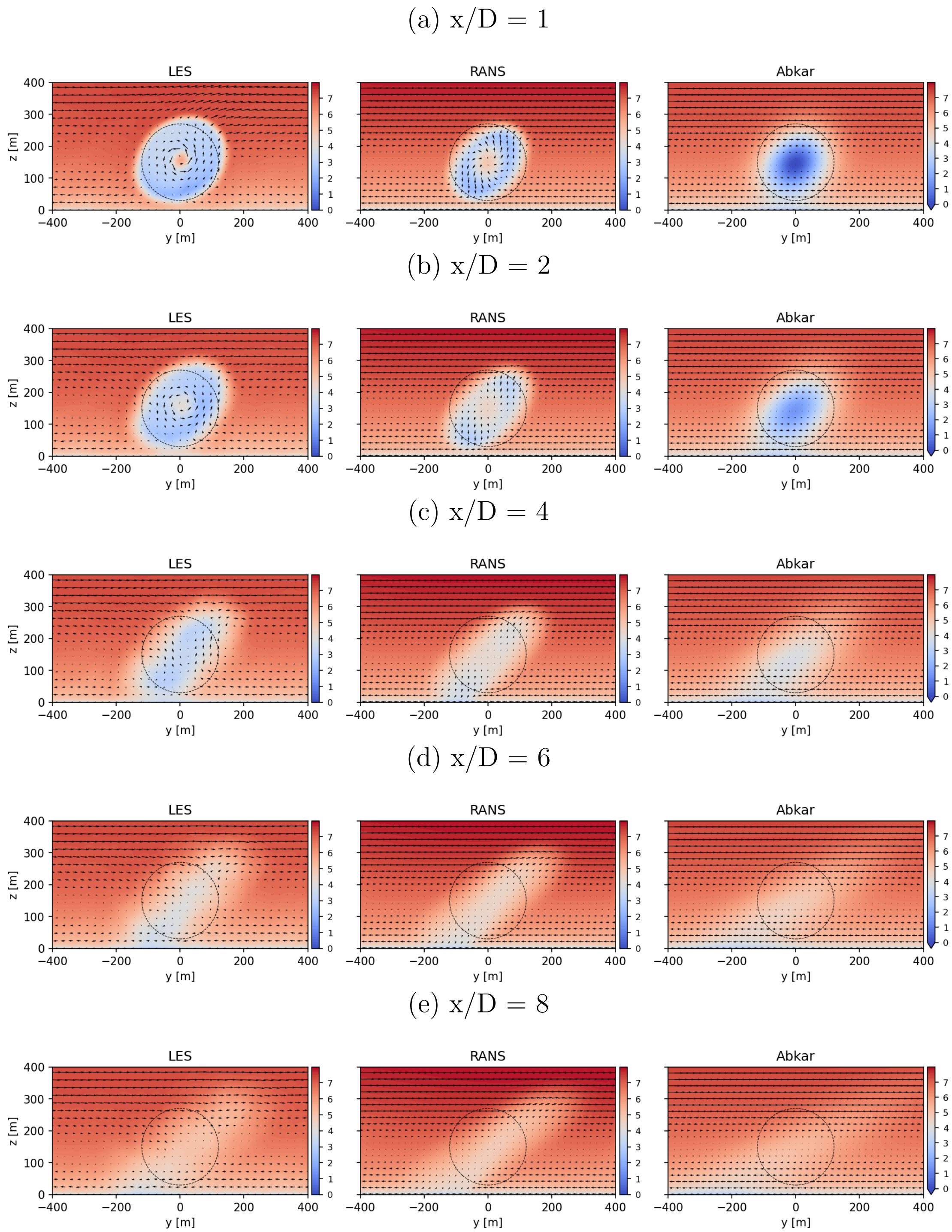
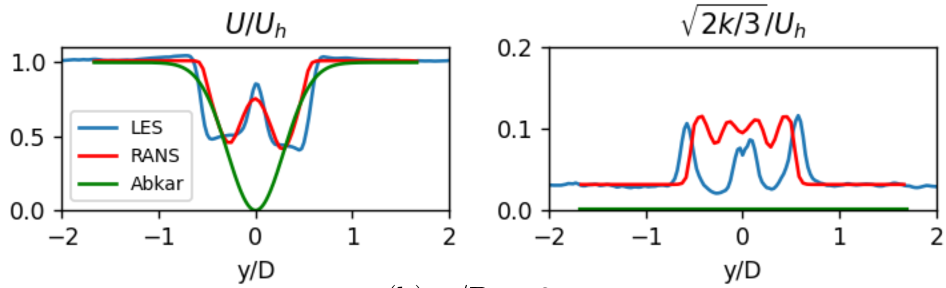
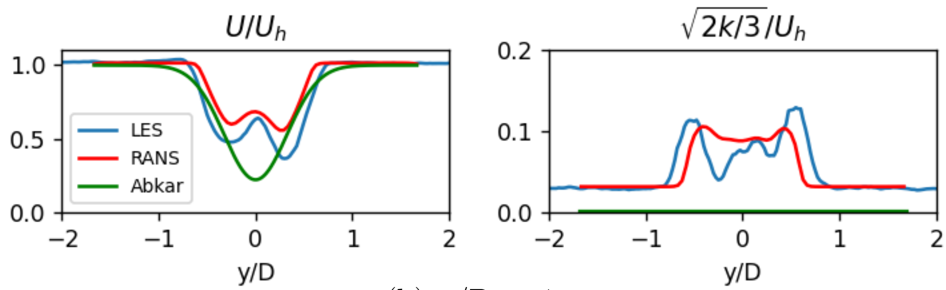


Figure 1: Comparison of the streamwise velocity for the Low WS case computed by the LES, RANS, and semi-analytic approach of Abkar et al [1]. Contours of $u(y, z)$ are plotted with units of meters/second at distances $x/D = 2, 4, 6, 8$ downstream of the turbine. The dashed circle corresponds to the location of the rotor disk of the IEA 15MW reference turbine.

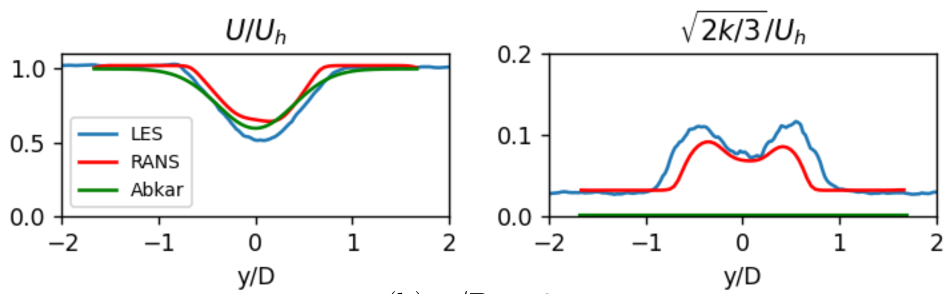
(a) x/D = 1



(b) x/D = 2



(b) x/D = 4



(b) x/D = 6

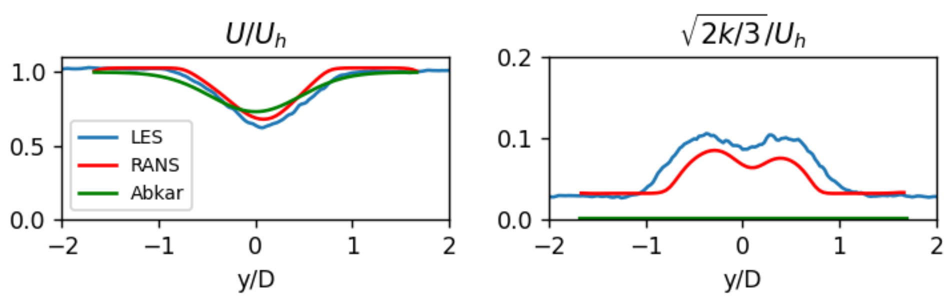


Figure 2: Hub-height profiles of the normalized velocity and normalized TKE $\sqrt{2k/3}/U_h$ for the single turbine wake in the Low WS ABL conditions, as computed by the LES, SANDWake3D RANS, and Abkar et al model.