Author's response to Referee #2

June 23, 2021

In the following we will comment on the general comments made by referee #2. The referee's comments will be repeated in blue italic before the answer. We will carry over and enumerate the referee's comments from the supplement pdf-file.

General comment

The manuscript presents a novel analytical wake model that is compared to aerial measurement of a utility-scale wind turbine wake. The reviewer is firmly convinced that such work must be rejected due to the poor scientific content, negligible relevance and confusing presentation. The analytical treatment and the supporting arguments are so rife with flaws that even major reviews would unlikely bring the quality of this study up to the standard of the Journal. Since this paper is already a re-submission of a work done a year before and rejected, a further re-submission is strongly discouraged.

This comment hurts to read. 'Negligible relevance'... Has the introduction and motivation even been considered? We give good reasons for easy to compute top-down analytical models in wind-energy science and their application for single wind turbines or even wind parks, where computation of each single wind turbine wake is usually not very feasible, e.g. in weather forecast modelling. It seems that the reviewer is stuck in his or her bubble.

Firstly, the introduction focuses mainly on Bastankhah and Frandsen model and claims that no efforts have been made to develop expression to relate the atmospheric turbulence to wake characteristics, which neglects all the scientific efforts in this sense done by several researchers in the past 5 years (see references).

Yes, we just used some of the most relevant easy-to-compute models. Also those models are semi-analytical which makes them nice to compare to the NS derivation.

As for the mentioned references:

- Carbajo Fuertes, F., Markfort, C.D. and Porté-Agel, F., 2018. Wind turbine wake characterization with nacelle-mounted wind lidars for analytical wake model validation. Remote sensing, 10(5), p.668.
  This is a semi-analytical model, as the authors even state. And it does implement longitudinal turbulence TI_x which is turbulence created by the wind turbine.

- Cheng, Y., Zhang, M., Zhang, Z. and Xu, J., 2019. A new analytical model for wind turbine wakes based on Monin-Obukhov similarity theory. Applied Energy, 239, pp.96-106.
  Sadly, I do not have access to this publication.

- Iungo, G.V., Santhanagopalan, V., Ciri, U., Viola, F., Zhan, L., Rotea, M.A. and Leonardi, S., 2018. Parabolic RANS solver for low-computational-cost simulations of wind turbine wakes. Wind Energy, 21(3), pp.184-197.
  How is this work countering the manuscript? This is nice to see. This looks like a more detailed bottom-up approach. But only using incoming TI.

- Niayifar, A. and Porté-Agel, F., 2016. Analytical modeling of wind farms: A new approach for power prediction. Energies, 9(9), p.741.
  This is again their semi-empirical model. And also each converter is modelled. Why is it so hard to understand that the presented manuscript would simply add a top-down analytical model, e.g. for wind park wakes where the approach is also applicable.

- Zhan, L., Letizia, S. and Valerio Iungo, G., 2020. LiDAR measurements for an onshore wind farm: Wake variability for different incoming wind speeds and atmospheric stability regimes. Wind Energy, 23(3), pp.501-527.
  This is a nice set of measurements. We will gladly add this to our list of current work done in this field. But we do not see any relevance to the presented work. They are presenting ensemble lidar measurements. No relevant modelling there.

The derivation of the simplified momentum equation (Equation (16)), which is simply the stream-wise momentum equation at the wake centre and for an axisymmetric flow, is necessarily convoluted. It switches between conservative and non-conservative form of the Navier-Stokes and is not rigorous. The turbulence closure (Equation (17)) is resemblant to a constant eddy viscosity model, but this is not mentioned. The 'approximate' solution in page 7 is evidently wrong, as it treats the residual velocity sometimes as a constant (a behaviour wrongfully ascribed to Taylor-frozen hypothesis) and sometimes as a variable when it is solved as a 2nd order polynomial. The experimental part is very concise and the characterization (especially stability) of the inflow not thorough. The several ad-hoc adjustments done to C and α make unclear the generality of the results. The calibration of Bastankhah model is also not explained sufficiently.

'The 'approximate' solution in page 7 is evidently wrong, as it treats the residual velocity sometimes as a constant' [...]

Yes, constant over dx to enable a first analytical approximation. Not constant over du_r, thus the characteristics of $u_r(x)$ is captured in the integral of u_r over du_r. If this simplification is not done the equation becomes very hard to solve. Check it out for yourself. And think about it. Assuming a slowly changing function (u_r along x) and treating it constant instead of slightly - let's say, linear or quadratic even - introduces a small error. But this error is acceptable as the Euler series shows. Even more so, once you consider wind park wakes, where u_r changes over several thousands of meters (30-60 km).

Of course this is an error that we choose to make, and we state in a whole paragraph how we deal with this. But you have chosen to ignore this!

The atmospheric stability in the inflow is treated and supported with all data that are available from the field campaign. Actual measurement campaigns can not provide grid-based data similar to an LES or any simulation. We have to deal with what we are able to record. And as for the stability we have a vertical profile of the virtual potential temperature and a wind profile affected by the near-by dyke. The thermal stability is further described by the friction velocity u_* from the measurements and from the logarithmic fit. There is nothing else to do or to characterise the thermal stability any further with the available data.

The following specific comments are given just for the records, as the reviewer is not willing to further review the present work.

As are the authors.