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Interactive comment on "An analytical solution for wind deficit decay behind a wind energy converter using momentum flux conservation validated by UAS data" *by* Moritz Mauz et al.

Anonymous Referee #2

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Summary

In the manuscript, the derivation and validation of a model for the velocity deficit in the wake of a wind turbine is presented. The model derivation starts from the Reynolds decomposition of the differential momentum equilibrium in a fluid and models a momentum flux from the wind at greater heights, which finally compensates the wake velocity deficit at a certain stream-wise distance to the wind turbine. A differential equation is obtained from the derivation and is solved analytically as well as numerically, where the analytical solution could only be obtained by introducing a simplification. Measurements of the mean wind speed in the wake using an UAV were undertaken to provide validation data to the derived velocity deficit model. The UAV was equipped with a five-hole probe for the velocity measurement. A flight pattern with 8 horizontal lines parallel to the rotor plane in different distances up to 5D from the rotor was chosen and repeated 3 times. The analytical as well as the numerical solutions of the derived differential equation was compared to the (mean) wake velocities obtained from the measurements. Good agreement was stated up to a distance of 2-3D behind the rotor. After this, the authors claim that the helical tip vortex structure has collapsed and therefore a modification of the derived velocity deficit model is presented. This modification is based on the assumption that a stronger mixing of the wake and the surrounding wind field is apparent from this distance. The modification of the model yields results that better fit the experimental data at higher distances. A discussion on the influence of the shear velocity, which is used as an input parameter of the velocity deficit, is added. In the conclusion, it is stated that the modelled and measured velocity deficit in the wake fit well and a number of possible improvements and further applications of the model are listed.

Comments

Before starting with the detailed comments, one major issue needs to be addressed: The variable u_r is defined as “the reduced horizontal wind speed in the wake along the x direction”. This definition is not sufficient. I assume that u_r is the mean value of the wake velocity at hub height. All my comments are based on this assumption. Furthermore, it is not clear if the averaging length is one rotor diameter or if the wake expansion is considered (resulting in an increasing length of the averaging space with higher distances from the rotor). Applying the above assumed definition of u_r, the analytical model in Figure 7 shows a reduction of the wind speed in the wake of 70% at 1 D behind the rotor. This is within the scatter of the measurements. This seems to me a surprisingly low mean axial velocity in the wake for a normal operation of the rotor. In wind tunnel measurements of Bartl et al. we see a deficit of 40-50% at that point. Other wind tunnel measurements of Kim et al. show a similar picture at ∼1.5

D with a deficit of a bit more then 40%, while the derived model shows a deficit of more than 60%. PIV measurements performed during the MEXICO experiment also show a considerably lower velocity deficit at design TSR (see Parra et al.). Especially when considering that a higher velocity deficit would be expected due to the absence of atmospheric turbulence in the wind tunnel experiments, the observed and calculated velocity deficit in this work seem surprising to me. From my point of view, it needs to be clarified if this is really the case or if there is a misunderstanding on my side. If not, a discussion on this discrepancy is necessary.

The general idea of the manuscript and the measurements seem promising to me, but the implementation and description of the performed work lacks accuracy at some points, which makes it difficult to judge on the results.

The comments will be clustered in three groups, namely: Derivation of the analytical velocity deficit model, Measurements, General comments.

Derivation of the analytical velocity deficit model

The derivation starts promising with a description of the Reynolds decomposition of the differential momentum equilibrium in a fluid. However, the equation is dramatically reduced by a number of assumptions. After this, the remaining (u'w') term is shall be replaced by an empirical relation. Here, the derivation starts to become difficult to understand and seems to contain some mathematical mistakes or some steps of the derivation were skipped, which prevents the reader from understanding what exactly happened here.

The reduction of the momentum equation is based on several assumptions. The assumption of a 'one dimensional, horizontal steady state wind field' implies that the wind turbine wake is no longer seen as a three dimensional tube or something similar. The model therefore assumes that a momentum flux can only be added to the wake region from higher altitudes but not from the flow on the left and the right from the wake. This assumption is valid for the far wake of wind farms, where the velocity deficits of multiple wind turbines merge and a more or less homogeneous horizontal layer with a velocity deficit up to a small height (in the order of magnitude of the wind turbine height) in comparison to its lateral size (in the order of magnitude of the wind park width) can be assumed. Here, the influence of the added momentum from the sides is negligible. This is not the case for a single wind turbine and no explanation why this assumption should be valid was found. In addition to that, the authors apply this assumption to the near and mid wake region, which is a region, where the flow is strongly dominated by the geometry of the tip vortex structure. These vortex structures seem completely neglected in this approach.

After reducing the momentum equations, the term (u'w') in EQ 4 is replaced by an empirical correlation, which is inspired by the work of Emeis. (u'w') is set to a term stated by Emeis that models the momentum flux from the above air layers into the wake. In Emeis work, this term is used to compute the integral (from free-stream to hub height) momentum flux. However, EQ 4 is derived from the momentum equilibrium in its differential form, meaning that no integration over the height took place. It is not clear, why this should be valid. This problem is also visible, when differentiating (u'w') by the z coordinate in EQ 7. From my understanding of the derivation, this is simply done by dividing the equation by delta z. Delta z is is defined as vertical the distance of the hub to a flow layer, where no velocity deficit is present. I could not figure out, how the differentiation of the expression in EQ 5 representing the integral momentum flux over the height can lead to this expression. Furthermore, it seems that EQ 7 shows a difference quotient instead of a derivative, which requires a solid explanation. In addition to that, the function shown in EQ 7 seems to be independent from the height, as delta z is a constant as described in line 5, page 4., while EQ 4 is not defined for a certain height. It therefore needs to be clarified if EQ 7 should be an evaluation of EQ 4 at a certain height (including an explanation why this is done).

In EQ 8.1 an integration is performed after rearranging the delta x to the right side. Here, it is still not clear if (delta u_r / delta x) is a derivative or a difference quotient. It is stated that both sides of the equation will be integrated, but the integration variable is not known. Assuming that x is the variable to integrate over, the dependence of u_r in the denominator of the first term in the braces seems to be ignored.

At that point, so many questions raise on my side, that a further review of the mathematical derivation does not seem to to be possible any more. In the end, we have a one dimensional function in EQ 10, which is dependent on the constant parameters delta z, c, v*, which is extended with a variable delta z function for distances of more than 2D from the rotor. This function in EQ 14 should describe the radius of the core wake, which is untouched by the free-stream turbulence. However, no explanation how this function was derived is given.

While c may be computed more or less accurately from simulations and the sensitivity of v* on the result may be small as stated in lines 1-3, page 15, the parameter delta z should have a major influence on the modelled velocity deficit. Delta z is assumed to be the rotor radius, but no explanation is given for this. As delta z is defined as the height (measured from hub height), where the free-stream velocity is reached again, the rotor radius seems to be a choice, that does not comply with the reality.

Summarizing this part, considerable doubts on the physical assumptions, derivation and choice of parameters of the model must be raised. Dismantling these doubts would require a large effort and it is not entirely clear if this is possible. Therefore, I recommend to see the developed model as an empirical relation, rather then an analytical model. In this case, the derivation could be removed from the paper and the result could be stated without the claim of physical correctness.

Measurements

The description of the measurement setup and site as well as data acquisition seems a bit short to me. This means in particular:

- It is not clear what exactly represents u_r (see above). - It is not clear how u_r is calculated from the measurements. The methodology how the velocity in the wake is calculated from the measurement signals should be explained at least briefly. In addition to that, the use of filters or similar of any kind should be mentioned. - It is not clear how u_0 is measured. Is there a met mast? Where is it? How long is the averaging time? What is the standard deviation? - Are there changes in u_0 during the experiment? - If u_0 is measured by a met mast (maybe at a larger distance), wouldn't is make sense to determine u_0 from the UAV measurements on the flight path in a certain distance to the wake? In this way, u_r/u_0 could always be computed with a continuously updated value. - The results of a comprehensive measurement campaign are reduced to some mean values. In order to judge on the quality of the measurements, the lateral velocity profiles should be included into the manuscript. This would also underline the scientific value of the measurements. - The operational state of the wind turbine is not mentioned. Is the turbine in below rated conditions? Were pitch angle and rotational speed constant for all measurement runs? - A discussion on the uncertainty of the measurements related to the actual measured velocities is missing. In a work by Subramanian the absolute uncertainty of the UAV wake velocity measurement is stated with 0.7m/s. Applying this to the measured wind speed at 1D in Figure 7, which is 0.3*u_0=3.15m/s, would yield an uncertainty of 22%. I recommend to insert a discussion on this. - From my understanding, the height of the flight paths should be more or less constant. What is the tolerance here? - It is explained, that the flight path during flight 1 is not suitable at some points, which leads to the exclusion of some measurement lines. However, there are also points missing, where the trajectory of path 1 seems to be very similar to the others (x=D and x =2D). Also other measurement points are from flight 2 and 3 are missing. It should be explained and at least exemplarily demonstrated why those measurement points were excluded.

General comments

- It is not clearly stated, what is the advantage of the developed analytical model in comparison to other models. However, it criticised that previously developed wake deficit

models do not take into account the atmospheric conditions. From my understanding, the present model includes this influence with the parameter u*. In the discussion, it is stated that the model is quite insensitive to this parameter. Does'nt this mean, that the present model is also not really including the influence of the ABL characteristics?
- The literature review does not contain other measurement campaigns with UAVs. It is therefore quite difficult for a reader, who is not familiar with such kinds of measurements, to set the presented measurements into a context.

Conclusion

Concluding this review, a lot of minor and some major issues were identified. Some of the issues may be caused by misunderstandings, which in turn means that further explanations should be given. This is especially true for the derivation of the analytical model. From my point of view, the manuscript needs considerable reworking in order to gain a positive recommendation. However, if it is not possible to dismantle the doubts on the analytical derivation, the main original part of this work would be missing and another focus needs to be found.

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