Author's response to Referee #2

September 8, 2020

Thank you for the effort that has been put into this detailed review of the manuscript. In the following I will comment on each point. The referee's comments will be repeated in blue italic before the answer. We will adopt the enumeration format from the original referee's comment list.

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. Measure- ments 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

1. *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.

Your assumption is correct. In addition, we have defined u_r more precisely in the next iteration of the manuscript.
Concerning the wind deficit measurements. The study by Bartl et al. (2012) states the residual wind speed in the wake (around 40%), as do our measurements; (we are aware of some confusing caption in Fig. 7 to Fig. 10 stating 'wind deficit measurements', as it is indeed the residual wind speed. We have addressed the issue). Also the wind tunnel experiments by Medici and Alfredsson (2006) show residual wind speeds between 20-30%. We think that Kim et al. (2018) average over the whole wake length behind the nacelle. This is also not representative of the wind deficit, since the undisturbed flow can stream around the nacelle and increases the residual wind speed behind it (nacelle effect). In our study, we want to consider the part of the flow (and its velocities) that represent the actual energy conversion. Wildmann et al. (2014) uses in-situ UAS measurements and shows a wind deficit of about 60% behind a Kenersys 2.4 MW converter.
Canadillas et al. (2020); Siedersleben et al. (2018) introduced a minimum average method for wind deficit calculation behind wind parks where the wake may meander. While this is not the case for our measurements, the same idea is implemented. We average around the wind speed minimum in the wake (neglecting blade-tip influence). Blade-tip vortices superimpose (positive and negative) with the horizontal wind speed at hub height. Thus they can have an influence on the measurement of the horizontal wind Mauz et al. (2019) at the borders of the wake to the undisturbed flow.
While this matter seems to be controversial, we want to argue that whatever averaging method might be applied, the model could adopt to the change of the wind deficit (being it 40% or 60%). We believe we have chosen an average method representing the energy conversion from the free flow, the most. The main argumentation in our manuscript, the change of atmospheric inflow along x, being caused by the collapse of the tip-vortex helix at around 2-3 D, remains unaffected. Regardless of the averaging technique.

2. *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*

   Thank you. While this comment is very vague, we will try to be more accurate in the next iteration of the manuscript. We have added some more paragraphs where we thought a reduction in the proceeding speed would be beneficial to the reader.

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

   We will adopt the commenting format of the referee in this response. To enhance the overview over all comments and to ease future reference, in addition we have enumerated the comments.

Derivation of the analytical velocity deficit model

1. *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 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.*

   We have restructured this part of the manuscript in the new iteration and added some more descriptions. From Eq. 7 to Eq. 8.3 we have a very detailed explanation of each step. We also mention the simplification we made, and how we justify to do so. Beyond the justification we also show, by using the numerical solution (Euler method), that the simplification can be done.
   Later on, solving the quadratic solution is not presented in any more forms or details, since we believe it to be trivial. Unfortunately, we can only speculate what to improve, since you do not specify what seems to be the mathematical error.

2. *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.*

Thank you. We have also considered a radial symmetric approach. Then, $\Delta z \rightarrow r$, the radius of the rotor plane. The problem that arises is that we then have to specify the lateral momentum flux. While we think in reality there is one. This flux is caused by the WEC itself, while the downward facing momentum flux is a (more or less) constant atmospheric parameter (Regardless of the presence of the WEC). So we want to argue that the wind deficit decay may be influenced in part by lateral momentum flux, but the momentum sink aloft the WEC is the main driver of the wind deficit decay. We also only consider the centre line of the wake, assuming the wind energy conversion is taking place along the centre line (which it does not exactly).
Let us consider a radial symmetric ($\approx$ rectangular, for the sake of flux calculations) approach, then Eq. 4 in the manuscript would be:

$$\frac{\partial u_r^2}{\partial x} + \frac{\partial(\overline{u'w'})}{\partial z} + 2\frac{\partial(\overline{u'v'})}{\partial y} = \frac{\partial u_r^2}{\partial x} + \frac{\partial(\overline{u'w'})}{\partial r} + 2\frac{\partial(\overline{u'v'})}{\partial r} = 0 \tag{1}$$

∂r can then be substituted with ∂z. In our argumentation $\overline{u'v'}$ is neglectable. However, we incentivise a comprehensive field study to measure these fluxes next to a WEC and also in the undisturbed flow. We think this is one of the main goals for a scientific study and a future improvement of the model. If $\overline{u'v'}$ and $\overline{u'w'}$ would be measured precisely and reliably, one could even think about solving the above equations for each x/D, without substituting the Reynolds stress term(s). But this is something we learnt after evaluating all data. For now, we have to deal with the method presented in this manuscript.

As you stated in your first comment, we replace $\overline{u'w'}$ by an empirical relation (Gradient method). Here, we stretch the validity of Eq. 6 ($K_m = \kappa \cdot u_* \cdot z$), which we will mention in the new manuscript. However, what it then comes down to is to find a value for u_*. Ideally, a method to calculate K_m at hub height would be great. But the measurements were not really suited to do so (e.g. obstacles on the ground like dike, high-voltage power lines, vegetation and industrial buildings in the inflow, resulting in no available free ranging flight path in the undisturbed flow). Also, we could not find a method to calculate K_m at hub height. u_* is defined for a surface measurement, and a multiplication by z states a linear increase with height (in the Prandtl layer). So, since we stretch the applicability of this equation by multiplying with a value for z (the hub height h what implies that we assume the WEC to be still in the surface layer), we receive an over estimate of K_m. This means in return, that we under estimate Δz, once we fit the model to the data ($\alpha = K_m/\Delta z^2$). To battle this dilemma we made the argument that Δz shall be the rotor radius of the WEC. From thereon the only parameter to influence α is K_m. Its determination then is described in the manuscript using the vertical virtual potential temperature profile, to derive a reasonable and typical u_* for these atmospheric conditions. It is easy to argue, but for now very hard to proof that u_* should be smaller and Δz be larger. In the end, it would not effect the value of α. This is were the measurements come in and show their value as the foundation for the model (fitting). They allow the determination of α using the assumptions stated above. Alternatively, one could even simply chose any numeric value as α and use any best fitting method. Yet, we believe, we have done the best, to back the calculation of α scientifically.

On a side note:
The determination of Δz in this model approach, but also in the Emeis (2010, 2017) (E10) model, is one of the remaining scientific tasks (s.a. Platis et al. (2018)) which once solved, will improve the model and all the statements that can be derived by its results (e.g. internal boundary layer heights above wind parks, influence of inversion height in wind wakes etc.).

3. *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 Δz. Δz is defined as the vertical 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 Δz 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).*

   We cannot find any integration in Emeis (2017). Emeis (2017) uses essentially the same approach up to Eq. 7 where he substitutes $dx = dt/du$, with $du = u_0 - u_r$.
   Concerning the Δz confusion. We do not divide any Eq. by Δz. Eq. 4 is taken, then we use a first order approach and assume continuous differentiability (which we will mention in the next iteration and try to make it more clear) and go from differentials to differences (bulk parametrisation). Now, we insert Eq. 5 into Eq. 4 and get Eq. 7.
   As also mentioned above on a previous bullet point, K_m is a function of height z. You are correct that Eq. 7 is then only valid at hub height. We shall mention this in the new manuscript.

4. *In Eq. 8.1 an integration is performed after rearranging the Δx 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.*

   $\Delta u_r/\Delta x$ is the result after going from ∂ to Δ. We have changed it up in the new manuscript and do now use d instead. We are dealing with first order approaches here (Gradient method in Eq. 5). We will state this more clearly, and then it should be fine.
   Concerning ignoring the dependency of u_r along x. We have to disagree. In the next 10 lines following the said integration we explain thoroughly why we simplify this integration and how we deal with it. We introduce a numerical validation calculation (Euler method) to estimate the error that we introduce by treating u_r as a constant over Δx. This is the Eulerian analogy of the Lagrangian simplification done by Emeis (2017) considering the air parcel travelling at constant velocity through the wake, when solve the time dependent exponential solution behind the wake (e.g. for calculating velocities at distant x/D in the wake of a wind farm as in Platis et al. (2018); Siedersleben et al. (2018)).

5. *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 Δz, c, u_*, which is extended with a variable Δz 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.*

   Okay. We can see that Eq. 14 (or Eq. 11) need a bit more of an introduction in the manuscript. The motivation for Eq. 11 is, that we needed a function that somehow represents the decay of the remaining WEC turbulence along x. In a first approach, we used a hyperbolic function to implement an asymptotic method for the remaining turbulence. Investigating the change of the decay rate along x is a goal for a stand-alone study, facilitating wake measurements along the whole wake (e.g. 1 - 10 D). We have added a brief explanation alongside Eq. 11.

6. *While c may be computed more or less accurately from simulations and the sensitivity of u_* on the result may be small as stated in lines 1-3, page 15, the parameter Δz should have a major influence on the modelled velocity deficit. Δz is assumed to be the rotor radius, but no explanation is given for this. As Δz 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.*

   Thank you for the comment. The parameter c does not need a computation. It can be measured or a literature value (e.g. Betz' law) can be used. This is indeed not trivial. Choosing Δz or even to define where it should begin or end is not easy to determine (s.a. Platis et al. (2018)). Please keep in mind that we need to simplify reality. So, we assume an instant velocity jump at the border between the wake and the undisturbed flow. Consequently, in the model, the momentum in-flux comes directly from the layer aloft the WEC wake. In reality, there might be a multilayered internal boundary layer (especially at wind parks) aloft the wake. We agree that the parameter should be more discussed.
   We will add an explanation in the manuscript. We also discussed the matter of Δz in a previous comment.
   The sensitivity of the model toward u_* is completely reworked in the new manuscript.

7. *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.*

   Since this point is a summary of the previous bullet points we do not see an explicit need to address the issues, once again. Yet, we want to respond. A lot of the issues pointed out by referee #2 are legit and need to be addressed. However, some of which are based on misunderstandings. We have put a lot of effort to lay a physical foundation for the model. The mathematical simplifications are validated by the Euler method. We have used all scientific tools available to us (physical, mathematical and numerical instruments fit together). The in-situ measurements can be seen as proof for the physical correctness. However, we can see that the fact that the measurements do not cover the whole wake length may raise some concern.

Measurements

Note: For the sake of clarity the authors restructured the measurement comments section to answer the raised issues more or less individually.

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

1. *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.*

   We want to refer to Comment #1 above, where we have already explained the circumstance. The manuscript has also been updated to clear the issue for future readers.

2. *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 it make sense to determine

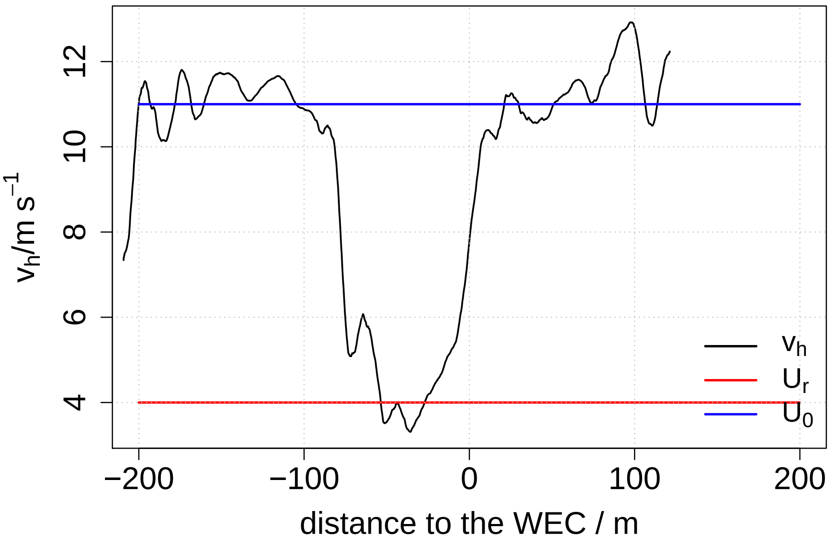

Figure 1: Horizontal wind velocity (v_h) behind the E-112 converter at $x/D = 2$. The UAS data is smoothed out with a moving average over 50 data points to reduce turbulence effects and make an evaluation easier. The averaged reduced wind speed is calculated by u_r/u_0 (red and blue line).

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.

Sadly there is no met mast in the area available. But it also is not necessary. We use the UAS data to compute a value for u_0. Therefore, as you already suggested, an updated value is calculated for each flight leg (meaning for each distance x/D). This is beneficial, since the mean horizontal wind always varies a bit. In this study the horizontal wind deviated around $\pm 1.5 - 2$ m s^{-1} (UAS data). The undisturbed mean horizontal wind has to be calculated with what is left of the flight leg and reaches into the free flow at hub height. This can be 100 m or 20 m. The analytical model is set up with an average horizontal wind $u_0 = 10.5$ m s^{-1}.
In addition we got the SCADA data (10 min averages) from the manufacturer. This data needs to be treated highly classified and the manufacturer also does not want to be mentioned in the paper. The average wind speed on top of the nacelle measured with a sonic anemometer is ≈ 10 m s^{-1}. However, this measurement is biased by an internal boundary layer around the nacelle and can only be used as a rough estimate. The maximum value measured on top of the nacelle has been 12.7 m s^{-1}. Yet, the wind velocity variations comply with the UAS measurements.

3. *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.*

   We have a plot for each flight leg. Here, an exemplary wake measurement evaluation plot is added (s.a. Fig. 1). When adding all plots to the manuscript, clarity will suffer, we suspect. However, we have implemented a data evaluation description in the manuscript.

4. *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?*

   The measurement flights 1-3 needed about 20 min. In this time span the wind turbine did not change its conditions. The E-112 WEC was operating near its rated conditions (rated wind speed: 13 m s^{-1}) with an average horizontal wind speed of 10.5 m s^{-1}. The average blade angle from SCADA is 1° with a rotational speed of ≈ 11.7 rmp.

5. *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* $0.7\ \mathrm{m\ s^{-1}}$. *Applying this to the measured wind speed at 1D in Figure 7, which is* $0.3 * u_0 = 3.15\ \mathrm{m\ s^{-1}}$, *would yield an uncertainty of 22%. I recommend to insert a discussion on this.*

   The manufacturer of the five-hole probe claims an accuracy of $0.1\ \mathrm{m\ s^{-1}}$ Rautenberg et al. (2019). In-flight conditions may vary this value a bit. But in general with the improvements made in design, IMU and GPS positioning an in-flight accuracy of $0.2\ \mathrm{m\ s^{-1}}$ can be expected. The components used in MASC-3 and the aircraft design can not be compared to the the UAV by Subramanian et al. (2015). Also path accuracy is an important parameter when calculating the 3D wind vector. See also next comment. We have added a short error consideration in the discussion.

6. *From my understanding, the height of the flight paths should be more or less constant. What is the tolerance here?*

   Yes, the flight path is more or less constant and tracked by the auto-pilot. An accuracy of ± 2.5 m in altitude deviation is achieved. This also depends on the level of turbulence. The movement of the UAS (e.g. up-down acceleration) is logged and also accounted for in the 3-D wind measurement calculations. This information has been added to the manuscript.

7. *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.*

   At $x = 2$ D there is too much variance in the velocity measurement, therefore a clear statement can not be made. We suspect tip vortex influence while entering and leaving the wake together with a too short flight path prohibit a reasonable determination of u_r/u_0.
   Regarding the missing point at $x/D = 1$, we are very thankful. There was a decimal error in the used data frame. Because we calculated the residual wind velocity to $0.4u_0$ at 117 m behind the WEC (it was set to $4u_r$).
   The data have been checked for similar errors. Non were found. We will update the graphics.
   Concerning flights 2 and 3: the flight track alone can not be seen as an indicator for a successful measurement. At the day of the measurement, the strong wind made manoeuvring in and out of the wake in a confined region more difficult as anticipated (hence the flight path adjustment). For a successful measurement the yaw, pitch and roll angle also must be up to specs. So it can happen that due to a tip vortex hit the calibrated angles (and pressures) for the five-hole probe are out of their specifications. This can lead to NAs in the measurement. It is rare, but it happens.
   Other measurements could have been used, if the flight path would have been longer. The flight pattern was set up for a wind direction of 90°. But from planning the flight pattern to starting the UAS and the measurement the wind direction changed slightly. Therefore, some measurements where corrupted by a too early turn of the UAS.

General comments

1. *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. Doesn't this mean, that the present model is also not really including the influence of the ABL characteristics?*

   Thank you. A similar issue was raised by referee #1. The introduction has been complemented with a clear motivation for this model. We also have added a complete reworked sensitivity study concerning u_*. The previous discussion around a change in u_* was considering distances up to 5 D where the differences are not that significant. When including the far wake, there are considerably differences to see in wake length. This is also the expected behaviour. In the next iteration of the manuscript the new related section is redone.

2. *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.*

   We have added information/literature of previous UAS measurement campaigns (e.g. in the introduction). We simply do not like to seemingly bloat the reference list.

Conclusions

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.
At this point we want to thank you for the detailed review again. It is apparent that you have put a lot of effort into following the manuscript in its first iteration. Unfortunately, the manuscript was not as easy to follow as we have wished. But we think with the points raised in this review – and the answers as well – the manuscript gained a lot.

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