Dear Editor,
Thank you very much for organizing the review of our manuscript. We are also grateful for the constructive reviews given by the reviewers - we highly appreciate all of your efforts. Based on the reviews, we have made several modifications of the original manuscript and thereby improved it significantly.
We hope that you will find the new revision ready for publication in WES.
Please find below our response (in blue) to the reviewers comments (in black).

Reviewer 1

General Comments:

This paper presents a simplified vortex model for the analysis of influence on local power coefficient and wake behaviour of a sheared inflow. The paper begins by describing some contradicting results in the literature regarding the influence of sheared inflow on global power generation and wake behaviour as predicted by medium and high order numerical methods. The paper presents a relatively detailed description of an analytical model based on the treatment of the wake as a set of disjoint wake regions separated by infinitely thin vortex sheets. The analysis shows that a number of the results predicted by classical 1D momentum theory can be reproduced within this theory and thus certainly has scientific significance.

It is concluded that the results from 1D momentum theory as used within a BEM approach be applied locally and that global corrections such as average disc loading and induction should be avoided. It is furthermore concluded that, according to the herein presented theory, there should be no cross-shear deflection of the wake.

The reviewer finds that the conclusions made regarding model inconsistencies by applying rotor-averaged corrections are correct, and that under the assumptions of the model, no wake deflection should occur. In this sense this simpified theory represents a fundamental contribution to numerical treatments of wind turbine rotor and wake aerodynamics. Emphasis should be placed on the fact that this analysis is valid for an idealized rotor & conditions (for example the statement made in the abstract that locally 1D momentum theory is valid for non-uniformly loaded rotors may be somewhat ambitious). As with classical 1D momentum theory, underlying the simplified vortex model presented here are numerous physical assumptions. The vortex cylinder treament for example assumes a semi-infinite, inviscid vortex sheet. The analogies between the model types are certainly valuable, and as with classical 1D momentum theory, there is a suitable area of application for this theory.

We agree with your comment about the validity of 1D momentum theory for non-uniformly loaded rotors. In order to clarify that it is just our simplified model, which predicts that locally 1D momentum theory is valid for non-uniformly loaded rotors we have rephrased the sentence as follows:

Our model predicts that 1D momentum theory should be applied locally when modelling a non-uniformly loaded rotor in a sheared inflow.

Specific Comments:

- Perhaps more emphasis should be placed on the model assumptions, in order to ensure that the theory is correctly applied in future works.

The model assumptions are described in detail in the beginning of section 2 and again mentioned in the conclusions. However, we have now included an additional paragraph at the end of section 4 to discuss the impact of the model assumptions on the finding presented herein. See also our response to comment number 15 by reviewer 2.

- Is the wake sheet suggested in Section 3.3 consistent with regards to Helmholtz's theorems?
Yes, the wake sheets consist of two horseshoe vortices starting at the rotor and ending infinitely far downstream. The sheet generating the shear is starting and ending at infinity and hence is also consistent with Helmholz's theorem (as is also described in section 2.1.2). To clarify this we have added the following sentence when describing how a step shear can be obtained in section 3.3:
Note that such a sheet is consistent with Helmholtz's theorem because it starts and ends at infinity.

- Suggest to use a different term than cylindrical to describe disjoint wake regions. The theory has been applied to cylindrical sections in the literature, however in the work here this is applied to general "extrusions" of areas in the rotor plane.
Good point! We have changed vortex cylinder to *extruded vortex surface* throughout the manuscript

- There appears to be an averaging across a full revolution required for the derivation of equation (8). Does this contradict the approach that distinct wake regions have distinct convection velocities?
Good question and we agree with you that this is not clearly explained in the original manuscript. To clarify that Γ is a local quantity that does not require integration nor summation overall azimuth angles we have made the following modifications:
1) We have added the following paragraph at the end of section 2.1.1
In the case where the bound circulation is allowed to vary not only as a function of radius, r, but also as a function of azimuth location, θ, it is noted that the local thrust coefficient, Equation (4), and the local power coefficient, Equation (6) (and/or Equation (7)), remain unchanged. In this more general loading case, it is noted that the physical interpretation of the local quantity $\Gamma(r,\theta)$ is the amount of bound blade/rotor circulation that pass the rotor disc location (r,θ) during one revolution of the rotor. It is noted that the local $\Gamma(r,\theta)$ is not equal to the azimuthal sum of the bound vortex strengths at radius r: no azimuthal averaging is required to obtain the local $\Gamma(r,\theta)$. The general local definition of Γ is used in the remainder of this work.
2) We have rewritten Appendix B completely and thereby removed the confusing integration over 2π.
The above modification should also answer the question you pose in the pdf about the constant zdot over a rotor revolution.
Technical Corrections:
There are a small number of typographical errors in the manuscript which are highlighted in the attached .pdf file.
Thanks for pointing out these errors. We have fixed them all. We have addressed the comments and questions you have made in the pdf related to model assumptions as part of our response to reviewer 2.

Reviewer 2

The content of this paper has scientific relevance to theoretical and modeling advances in the field. I recommend this paper for publication, after minor revisions to help readibility.
Typos
• "recent validation studies[ ]Boorsma"
Noted and changed

• "using various free vortex vortex tools"
Noted and changed

Comments/suggestions to improve readibility

• The description of Figure 1 is clear in the text, but the diagram shown in the figure itself is confusing. Showing either the rotor disc or the position of the blade in the diagram would make it easier to understand.
To clarify this, we have inserted a text arrow, which indicates the rotor plane and stated that the blade is pointing out of the paper in the figure caption.

• Eq. 2: What is d? Are the dr in numerator and denominator typos? They feel like it. Why are they there? Also, bold Gamma was defined, but what is the non-bold Gamma? The magnitude of bold Gamma? If so, please mention that in the text.
We have now clarified that dr is the radial width of the considered annulus of area $dA = 2\pi r dr$ by inserting the following sentence to introduce Eq. 2:
The force from all blades on the annulus of the rotor at radius r and radial width dr is $N_b \mathbf{f}_b(r) dr$ *and hence the local forces per unit area on the rotor disc becomes*

To clarify the difference between non-bold and bold letters, we have inserted the following sentence in the paragraph between Eq. 1 and Eq. 2:
Please note that in the following we use bold letters to represent vectors and non-bold letters to represent scalar values and length of vectors.

• "Letting Ω go to infinity…" that would lead to an infinite Mach number along the blade, breaking the assumption of incompressible flow.
Good point. The correct assumption is that the tip speed ratio goes to infinity. This is now corrected.

• How is Eq. 4 useful? It was introduced without further use at this point.
Eq. 4 is used in the derivation of equations 7-9 as is also stated in the manuscript.

• "To do this we will be using the following result, which is proven in Appendix B and illustrated in Figure 2:" Is there something missing here or is that colon a typo?
There is nothing missing. The result we are referring to is the sentence following the":", i.e. *For an infinitely long extruded surface ......is* $\gamma/2$.. To clarify, we have removed the newline after the ":" and written the sentence in italic.

• Swap Appendix A and Appendix B according to the order that they are mentioned in the text.
Noted and changed

• "… any change in bound vorticity in the spanwise direction…" Fig 3a refers to it as radial direction. Change either the text or figure caption for consistency.
Noted and changed

• Paragraph between Lines 120 and 125: What do you mean by "each side of the vortex sheet"?
We mean above and below the sheet. This is now clarified

• Fig. 7 is difficult to understand. Please add a 3D view (like in Fig 8) to make clear what is

meant by the two different strengths.
We have decided to keep this figure as it is and instead modified figure 8 so that it more clearly shows what is meant by the two strengths. When introducing Fig. 7, we then add the following wording to help the reader:
A 3D sketch of the case is shown in Figure 8, which outlines the different vortex strength contributions

• "...can readily be extended to a slightly more general case of two regions that both have borders with each other and with the outer flow...": I am confused. I thought that was what is shown in Fig. 7 already. Please make a figure that clarifies what is meant by this, and/or redo Fig. 7.
We agree that this is confusing. We have now removed this sentence.

• "the following expression for the shear sheet condensation": What do you mean by condensation?
We have changed "condensation" to *additional induced shear vorticity*

• "... determined by integrating the vorticity entering and exiting an infinitesimal cylinder enclosing the junction between the wake border and the shear sheet layer from 0 to z...": Could you please make a figure showing what is meant by this?
Good idea. We have inserted a new figure to show what is meant. In addition we realize that it is not entirely clear how the infinitesimal cylindrical control volume is used to determine the axial vorticity. Therefore, in addition to the new figure we have replaced the sentence you refer to with the following wording:
The axial vorticity at a given streamwise position z can be calculated by considering the conservation of vorticity for an infinitesimal cylindrical control volume enclosing the junction between the wake border and the shear sheet layer from 0 to z as shown in Figure 9. Since all vortex filaments form closed loops or start and end at infinity it is clear that the total flux of γ through this control volume is zero. Therefore, the axial vorticity through the end faces of the control volume is in balance with the vorticity through its sides. Thus, the axial vorticity can be determined by integrating the vorticity entering and exiting the sides of the control volume from 0 to z.

• Please add to Section 4 a discussion/summary of the assumption taken to develop this model and its limitations.
We have added the following paragraph at the end of section 4 to discuss the assumptions and limitations:
Our model is based on several simplifying assumptions as outlined in section 2 and the above findings should of course be seen in this light. Most of the assumptions made about the rotor are fairly standard for engineering analyses and we do not expect that a more advanced rotor representation would change the overall findings of our model. Nevertheless, a consequence of assuming infinite tip speed ratio is that rotational effects are neglected and hence any impact of asymmetric development of the wake in sheared inflow is disregarded by our model. The effect of a finite tip speed for axisymmetrically loaded rotors in uniform inflow was assessed in (Branlard and Gaunaa, 2016) using a model based on the same building blocks as the present work. This work showed that the effect of the finite tip speed ratio is a second order effect for tip speed ratios typically used in modern wind turbines. Therefore, we expect that the impact of neglecting rotational effects is small for moderately sheared inflow and typical tip speed ratios of wind turbines.
On the other hand, out of the assumptions made about the flow, e.g. incompressible potential

flow, no wake expansion and constant transport velocity of wake vorticity, the latter two are obviously questionable. In reality the wake is clearly expanding while the transport velocity of the wake vorticity will be faster in the near wake than in the far wake. Thus, on their own these two assumptions are bad and leads to poor predictions. However, when used together the two assumptions leads to predictions that are consistent with 1D momentum theory. The reason for this is that while neglecting wake expansion leads to an under prediction of the induction in the rotor plane (Øye, 1990) the opposite is true when assuming a constant transport velocity of wake vorticity and thereby the two assumptions counteract each other. The agreement with 1D momentum theory indicates that our model is of the same order of fidelity as those typically used in BEM models. However, our model gives another perspective and is developed for non-uniform inflow and thereby it can be used to gain insight and guide future development of BEM models to correctly cope with sheared inflow.

• The reader could benefit from adding a few sentences on future work and possible applications/extensions of this model.
The main purpose of our model is to gain insight and guide future development of BEM models to correctly cope with sheared inflow. This is now stated at the end of the discussion section.

Appendix
- "and [d]s is an infinitesimal..."

Noted and changed

• "Since there for all segments on the cylinder surface is a corresponding segment..." grammatically wrong or at least confusing. Please rephrase.
We have rephrased the sentence to:
This holds true for any choice of azimuth position and $\pm\Delta z$ *and therefore it follows that the total induction of the vortex surface is in the* z*-direction, only.*