We would like to thank the reviewers for their time and their thoughtful comments. Our responses below are in blue text. Changes to the manuscript are also in blue. In response to the second reviewer, we have removed our discussions regarding wake pitch and blade-element independence. This has allowed us to focus more on the blade-element thrust derivation, and the new insights regarding radial velocity. Because the reviewers made overlapping comments, we have not attempted to separate our responses in terms of text colour in the manuscript.

Reviewer 1:

The authors have done a great job in modifying and correcting the article. However, there are still some (minor) issues that I would like them to consider.

- The paper is at places unnecessary complicated to read and comprehend. I suggest that the authors take a critical look and add some sentences to guide the reader more easily through the paper. An example of this is eqs. (5) – (7) on page 5, where eq. (5) is a relation and (6) and (7) are terms, and then it states that 'Equations (5) and (6) can be combined to give..(7)'. This is confusing, how can a relationship (5) and a term (6) be combined to give a term (7)?

  Thank you for this comment. We have attempted to make the derivation more fluid (pun not intended) throughout. We also believe that removal of content of secondary importance, as noted above, has made our key arguments easier to follow.

- As far as I can see, eq. (12) is only valid along a horizontal stream surface. But that implies that there is no expansion, and then the whole idea breaks down about formulating an alternative version of the momentum theory, as without expansion there are no influence of the pressure on the lateral part of the stream surfaces, and therefore the axial momentum equation can be use without any problems.

  We have clarified that our analysis is fully three-dimensional. The simplification of the vortex terms in the near wake is still valid when expansion occurs. Figure 2 has been added to illustrate the coincidence of streamlines and vortex lines in three dimensions. A two-dimensional projection of these vectors leads to Equation (15) in the revised manuscript.

- To be honest, then I have a problem in accepting eq. (24). Why should the integral of the radial velocity squared by equal to the integral of the axial induction squared at any arbitrary cross section upstream the rotor? I understand that this is a direct consequence of eq. (19). But, again, as far as I can see, the elimination of the vorticity is based on eq. (12) that assumes no expansion. With no expansion eq. (24) is clearly satisfied as here v=a=0. However, since eq. (28) is correct, as it can very easily be derived from the unsteady Bernoulli equation, I suggest that the point is that the right hand side of eq. (24) is outbalanced by the neglected vorticity terms. This, I think is still interesting, and deserves to be reported. In the opposite case, it would be an easy task numerically to show if eq. (24) is valid or not. Furthermore, the argument that eq. (25) is correct if a and v is continuous is somewhat vague, as it foremost demands that eq. (24) is correct at any cross section upstream of the rotor.

  To derive $\int_{S_U}(v^2 - a^2)dS = 0$ on any plane upstream of the rotor, it is not necessary to "eliminate" the vorticity; the flow upstream is naturally irrotational. The assumption needed to make the integral zero at or behind the rotor plane is that v and a are continuous across the rotor plane. This is enforced to be the case in actuator disk simulations, and we have cited four studies that show our prediction $v \approx a$ agrees with numerical results and one that directly supports the integral above (Equation (24) of the manuscript).

In reviewing our discussion of radial velocity, we noticed that the conclusion $\int_{S_U}(v^2 - a^2)dS = 0$ *implicitly assumes the flow to be circumferentially uniform. This fact is now highlighted at the beginning of section 4.1.*

- I am not sure that I understand the content of Appendix C. Could you try to explain what control volumes you are using, what is xo,BE and what is the purpose of the appendix?

  We have removed Appendix C.

Reviewer 2

General remarks:

The authors aim to develop a "Kutta-Joukowsky-equation", Eq. (28), by avoiding use of pressure. As the reviewer's first-round remarks, there are already derivations (or discussions) from Glauert (1935) Soerensen (2016) and van Kuik (2018). Nevertheless, this does not mean that a new derivation is meaningless, but it then should be "easier" to understand or lead to further progress.

Unfortunately, I do not see if this goal has been achieved.

We would like to thank the reviewer for the thoughtful comments. We have sought to clarify our contribution further, improve the organization of our arguments, and omit material of secondary importance. In particular, we emphasize that our derivation of the Kutta-Joukowsky (KJ) equation for blade-element thrust is more general than the classical derivation. Ours permits the wake to be rotational, which the classical derivation based on the Bernoulli equation does not allow. Our derivation also lends novel insight into the magnitude of the radial velocity as documented in the reponse to the first reviewer.

Edits to the manuscript are in blue for ease of reference. We have responded to the specific comments in blue text below.

Specific remarks:

A complete list of used symbols before the actual text would be desirable.

Unfortunately, this does not appear to accord with the journal style. We have endeavoured to clearly define each variable in the text at its first introduction.

Figure 1: Please add a coordinate-system

We have shown the coordinate system on the left sketch of figure 1 and have introduced the coordinate variables in the text.

Eqs. (2) and (3): Please add statements about the regularity of the velocity and vorticity fields you imply, i. e. to which class C_1, C_2 . . . C_∞ they should belong, to make all integrals well-behaved. To put it more in terms of physics: What assumptions of vortex lines, sheets or so on are made implicitly?

These equations certainly apply to C_∞ *continuous velocity fields, but are often applied to discontinuous velocity fields (flows featuring vortex sheets or discontinuous changes in total head). However, we're not aware of a* *rigorous proof of their limitations. Your comment raises an important point. Up until Equation (14) in the revised manuscript, we have made no assumptions about discontinuities; the flow is treated completely as C_∞-continuous. At this point in the previous manuscript, we suggested that by treating the wake as a series of thin vortex sheets, we could simplify the vortex terms. In fact, the same simplification can be made without this assumption. We have now refined our argument, assuming only that diffusion on the downstream plane is negligible, so that Kelvin's circulation theorem applies to individual fluid elements. This makes streamlines and vortex lines coincident in the near wake without assuming the wake to be infinitely thin. We have added this line of reasoning to the text after Equation (14). This improvement sets the derivation apart from the classical derivation based on the Bernoulli equation, and we have emphasized this distinction.*

Eq. (12) Is this simply a definition of p or a non-trivial statement?
(An additional sketch would be helpful)

We have removed all discussion of pitch, and so p has been removed from this equation. We have also added a sketch to illustrate the similarity of velocity and vorticity triangles in the near wake.

Line 141: “the other dynamic conservation equation” Do you mean: “The other dynamical equation based on conservation of angular momentum”?

We have removed all discussion of angular momentum and torque to focus more clearly on the thrust.

Section 3 (lines 141 to 165): think about skipping it because you state “ . . . has not enriched the analysis”

Thank you for this suggestion. We have removed this discussion.

Eq. (24) Line 171: I'm afraid that from this INTEGRAL almost nothing can be concluded for local behaviour unless you make severe assumptions on v(x) and a(x).

We agree that we cannot make definitive local statements about a(x) and v(x). However, we have clarified our arguments after Eq. (24) for why $v = a$ is likely to occur in the vicinity of the rotor tip. Firstly, $v = 0$ at the rotor axis, and so $v < a$ over the inner rotor, but we also expect $v > a$ outside the wake. Thus, $v = a$ must occur somewhere, and seems likely that this would happen where a is rapidly decreasing at the edge of the wake. We have referred to four numerical studies that support these results and one that specifically verifies $\int_{S_D} (v^2 - a^2)\, dS = 0$ at the rotor plane.

While reviewing our arguments here, we also noted that the result $\int_{S_D} (v^2 - a^2)\, dS = 0$ carries the implicit assumption of circumferential uniformity. We have made note of this fact at the beginning of section 4.1.

Line 174: “. . . likely consequence v ≈ a at the edge of the wake”. As in Fig. 15/16 of Madsen et al. (2010) only results from numerical investigations are discussed, an analytical approach should give saver estimates about the flow esp. at the edge (x=1) of the actuator disk.

Yes, the results of Madsen et al. (2010) are numerical. Whether our analytic arguments or their numerical results are “safer,” we have not said. But the results of both approaches are consistent.

Line 220: How are Eq. (30) and Eq. (12) connected?

Equation (30) has been removed.

Line 233: “ . . . a constant-p wake defines an optimal rotor, (originally from Betz (1919) . . .).

Unfortunately, Betz, in his paper only investigated lightly-loaded propellers and not heavily-loaded wind turbines, as also remarked in Soerensen (2016).

You are correct, and this limitation does restrict the scope of our arguments more than we would prefer. In favour of sharpening our stronger arguments, we have omitted all of the discussion regarding wake pitch.

Line 268/269: "We are completing .." think about skipping this sentence

Done.

Line 217: How can x be large if $0 < x < 1$?

We were referring to locations near the tip, i.e. $x \rightarrow 1$. However, we have removed this discussion altogether.

Line 301 ff: " . . . approximately correct at high tip speed ration . . . "
Can this be made more explicit? Like $O(\lambda_{-n})$?

This discussion has been removed.

Line 302: " . . . cannot describe the runaway (raw) state …" As far as I know, this stat ($cP = 0$, TSR_raw $< \approx$) is excluded from this model at all, because cP increases (strongly) monotonously.

This discussion has been removed.

Line 303: "Thus the trailing . . . assumed in lifting line theory for wings." Isn't this statement trivial and superfluous, as pure translational rotational motion have nothing in common?

This statement alludes to future work that we are conducting, and we recognize that this statement does not add much to the central arguments we are making. It has been removed.