Review of WES-2021-52:

Some effects of flow expansion on the aerodynamics of horizontal axis wind turbines,

by David H. Wood and Eric J. Limacher

Reviewer: Gijs van Kuik, TU-Delft

Introduction

Performance calculation of wind turbines is based on momentum theories developed over the last 100 years, with the turbine's rotor replaced by a disc. Most easy is the calculation for the entire rotor (disc) with a high tip speed ratio of the rotating force field, giving disc-averaged results. The calculation methods for low tip speed ratio and for the distribution of velocities along the radius of the disc are less developed. The paper addresses both areas, so it is very much welcomed.

As far as I have checked there are no mathematical flaws. The reference list shows a good knowledge of the relevant literature. Being a theoretical paper, it is not easy to read. The has a logical structure, but at several places accuracy of the text could be better, and the explanation in physical terms could be expanded (below I have addressed this).

The reviewer

The authors refer several times to van Kuik (2018) and van Kuik (2020), so to my own work. This is one of the reasons to act as a non-anonymous reviewer: in this way authors and readers know that the reviewer discusses the results of the paper with his own work in mind. At some places in the paper I conclude that the results of the authors and reviewer differ. The intention of these remarks is not to say that the authors' results are wrong (it might turn out that my results are wrong), but that an explanation of the differences is required.

General remarks

1- Disc or rotor or turbine: all are used, but sometimes in a confusing way. E.g. line 30 : 'a circumferentially uniform rotor'. I advise to use the word disc for this, as a rotor is characterized by a finite number of blades. When this number increases to infinity, or when 'circumferentially uniform' is used, it is a disc. The word turbine in the first sentence of the Discussion is not well in place. I propose to change the title of section 3, as there are no blade elements. The words 'blade element' has the annotation of an aerofoil with a finite length chord, with lift and drag acting on it. None is present here, so better would be: Local thrust in expanding flow. This improves the structure of the paper: section 2 treats the entire disc and stream tube, section 3 the local properties or the distribution along the chord. It took me a while to recognize this structure using the titles as they are. A suggestion for section 4 is: start a new section Results at line 278.

2- At several places the authors claim that a topic or result was lacking in the literature, or presented by them for the first time. Please be extremely careful with this. In the detailed comments presented below I give some examples where the claim is not entirely justified.

3- The list of assumptions at page 3: This is very much appreciated, as in general we do not like assumptions so it is good to list these explicitly. However, in the remainder of the paper more assumptions are being made which worry me much more than the ones in the list. These extra assumptions allow the derivation of the results and a numerical assessment but maybe at the cost of the results:

1- the pitch of the convected vorticity is constant even in the expanding part of the flow,
2- the pressure gradient due to the swirl results in undisturbed pressure at the boundary of the far wake, and
3- the pressure jump across the disc is anti-symmetric: P_U is assumed to be - P_D.

Please list these assumptions as explicitly as those at page 3, at as convenient place. These assumptions may be the cause of the remarkable results in Table 1. Although the calculated distribution of the velocity is very familiar to results in literature, the performance results in Table 1 are not. For lambda = 14, the effect of swirl in the wake is known to be very minor, so the result for the disc with infinite lambda should be recovered, which isn't. This is something to elaborate on. Which assumption has which effect on these results?

4- The authors use a nice result of their preceding paper, eq. (2), to quantify what is called the redistribution of momentum. This is the result of the pressure acting from one streamline to another, resulting an axial contribution to the local momentum balance. I fully agree with this, but not with the conclusion that 'momentum is redistributed from the external flow to the turbine'. The external flow is the flow not passing the actuator disc, so the stream tube boundary splits the flow in external and disc-passing flow. As known from literature and confirmed by the authors, the contribution of the pressure - acting on this boundary - to the momentum balance of the flow passing the disc, is 0. There is no momentum redistribution from the external flow to the stream tube, irrespective of the tip speed ratio.
5- It is appreciated when an explanation in physical terms is given of what redistribution is, where it takes place. The authors conclude that the redistribution is 'complete by the time the far wake is reached'. This is a consequence of the fact that the pressure only can contribute when the flow is expanding, partly upwind of the disc, partly downwind. At several places the authors emphasize that it is the upwind part of the flow responsible for the redistribution. Given the choice of the control volumes in figure 1 the upwind part (including the disc) is used to derive results, but physically there is no reason to exclude the downwind part (see also detailed remarks below).

Detailed remarks

Line 27: the connection of the sentence starting with 'This ..' in connection with the sentence in lines 26-26 is not clear to me. In CV1 and CV2 there is no axial component of the pressure acting on the boundary with constant Rcv .

Line 28-30: About the distribution of momentum by pressure forces acting on the control volume: The word 'redistribution' is new as far as I know, and well in place. I disagree with 'it is lacking'. It has been treated and quantified in vanKuik(2018), section 5.2.4 & figure 5.13 for Froude discs (infinite tip speed ratio) and section 6.4.5 & figure 6.13 for Joukowsky discs (finite tip speed ratio). The momentum balance per annulus contains the contribution by the pressure at the boundary of the annulus. The pressure contribution was numerically calculated.

Line 39: 'was assumed', why not 'is assumed'? You use this property in line 72.

Page 3, Assumptions:

1: here are two assumptions: about the flow and about the rotor (disc?)

3&5: as 1 gives inviscid flow, these are superfluous

4: I agree, but it follows from the continuity equation for incompressible flow

6: follows by the assumption that the flow far upwind of the rotor is uniform

8: as vorticity is absent upwind of SU, this applies to the space between SU and SD? Am I right? About blade element: see general remark 1

Missing (?) assumption: I assume that you apply the Joukowsky model for the wake throughout the paper, as indicated in line 218. If so, it is worth to mention in the list.

Line 82: lambda = 0: this flow case has been studied by van Kuik (2018), section 6.3.1 and figure 6.2. The flow is blocked (axial velocity at the disc and in the wake is 0, so a = 1), and has a significant wake expansion. The wake has swirl with w = 1.648 R/r, so w^2 is not 4a. Please compare your solution with this.

Line 95-98: as for eq. (4) and (5) and section 4, the result mentioned in these lines depends on the pitch being constant. I have three remarks: 1: This restriction is not used by van Kuik (2018), section 6, still giving the same result that $a_\infty \approx 2a$ for infinite lambda, and < 2a for finite lambda. Please discuss the impact of constant pitch p in section Discussion. 2: Equation 4 is taken from Okulov&Sorensen (2008), but it holds for the Betz/Goldstein distribution of w, not the Joukowsky distribution applied here. Still this may be justified (and you are in good company as Prandtl's tip correction is based on the same crossover) but then this should be told. 3: see general remark 3.

Line 114, eq. (11) this equation is a crucial equation in the reasoning of the paper. When the first and third integral are combined and evaluated, they become the integral of PU, as it should be. As eq. 11 is valid for the entire disc, redistribution is not that clear to me as no information is involved regarding redistribution over the disc surface. The fact that (a2-v2) appears is not the same as redistribution, unless my understanding of redistribution is not correct.

Line 124, equation 13 holds for the upwind half of the stream tube. Using eq. (2) the right hand side may be converted to an integral from 1 to infinity, preceded by a minus sign. Using a momentum balance for the external flow combined with Bernoulli's equation, this modified eq. (13) is valid for all z, including downwind. My interpretation is that at any position z the axial contribution of the pressure at BS, integrated from upwind to z, equals the modified right hand side at that position z. Far enough downwind the right hand sides becomes 0, so the modified eq. (13) is a new proof that the pressure at the BS does not contribute. Do you agree?

Line 126: You write 'It is easy to generalize (13)' but is it that easy? Some intermediate steps would help to show that the Bounding Streamline in (13) can be replaced by any streamline leading to (14). If this interpretation is correct, line 128 is confusing, as BS belongs to (13) not (14).

Line 132-133: I do not see the need to use the unsteady Bernoulli equation. (16) follows by applying steady Bernoulli at a stream surface from the downstream disc side to the far wake.

Line 140, eq. 18: this holds for a Joukowsky wake, with a root vortex at the axis controlling w, see my remark on: page 3 Assumptions.

Line 147: this is a severe assumption, see general remark 3. This assumption limits the applicability to high tip speed ratios. In van Kuik (2018), section 6.2.3, it is shown that an additional pressure term is required to have the pressure undisturbed at the wake boundary. It is this term (combined with the expansion ratio R_∞/R) that determines the deviation from the classical result $a = 0.5\ a_\infty$ in case of low tip speed ratios, see section 6.2.5 of van Kuik (2018).

Line 153: see my remark at line 82.

Line 174: see remark at line 28-30

Line 180 - 185: if the redistribution term (a^2-v^2) expresses the pressure exerted by one annulus to another, it contributes only to an axial momentum when there is flow expansion. Indeed the process is done when the wake becomes the far wake. I am confused by the sentence about the redistribution probably being complete everywhere within the wake. What is 'within the wake'? In radial direction? In axial direction? A combination?

Line 197: As remarked before, I disagree. There is no redistribution from external to internal flow,. There is momentum transfer from one streamline to another. inside the stream tube. If that is what you mean I agree, but like to see the text changed accordingly.

Line 198: what are stationary rotors?

Line 199: remove 'because flow expansion is neglected'

Line 205-210: the relevance of this paragraph is not that clear, and the text is not that clear. Which first term? About lambda = 0: vanKuik(2018) shows that circulation Gamma is independent of x, section 6.3.1. Suddenly the number of blades plays a role. If N becomes important, there is no circumferential uniformity any more, as in the analyses so far. Is this something to consider?

Line 233: see my remark at lines 95-98.

Now I jump to:

Table 1: is the last column correct? Delta Ct is not explained but I assume it is the difference between the 7th and 8th column. If so, there is an error in the table. Even for lambda = 14.28 the deviation from the Betz-Joukowsky limit is significant. Although the plots in figures 4 and 5 show the same characteristics as those in van Kuik(2018) the performance results differ: for lambda = 5 the difference with the Betz-Joukowsky limit was <1%, decreasing with higher lambda. See my next remark

Line 298 The discussion is partly a summary of results. I would prefer a discussion about the consequences of the assumptions made, and how these are impacting the results in Table 1 (see also General remarks)

Line 301-302 It may be a matter of language, but this statement is too strong. Eqs (12,13,14) are new indeed, but they may be considered as another way of expressing the thrust. Methods used so far do not use P_D or (a^2-v^2) but express T in terms of the rotational speed or lambda and the circulation or swirl around the axis, see eq. 4.6 in Sørensen(2016) and 4.23 in vanKuik(2018).

Line 332/333: this statement is not correct. The ‘common derivation’ of the B-J performance is the derivation for infinite lambda. It does not need information about the distribution of the axial and radial velocity at the disc, nor does it predict the distribution. So what you use ‘ignores’ I would say ‘does not need’. Things are different when you consider annuli instead of the entire stream-tube. Indeed no closed form solution is available, but an analysis (with calculated pressure) is as mentioned in my remark on lines 28-30.

Finally: Abstract and Conclusions : I expect that the abstract and the conclusions will be modified when (some of) my remarks have been taking into account.