

Review of manuscript WES-2025-105

Comment on “Glauert’s optimum rotor disk revisited – a calculus of variations solution and exact integrals for thrust and bending moment coefficients” by Tyagi and Schmitz (2025)

by

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1.) General Comments

The author comments on a peer-reviewed and accepted paper of Tyagi and S. Schmitz on Glauert’s general momentum theory of wind turbine rotors. A thorough discussion of this model, including its (sometimes hidden) assumptions and limitation was published earlier in [1]. The author criticizes that “no new physical interpretation” has given, “favouring abstract derivations over engineering context”. In a last paragraph he notes, that parts of the paper have been previously published which has not been mentioned in the paper under comment.

It must be noted that – to the knowledge of the referee – all commercial or scientific BEM codes used for wind turbine analysis and/or design use Glauert’s approach including use of a’. An exception may be found in [3].

2.) Specific Comments

a) Paragraph 1, Lines 4 to 11

Calculus of Variation has a long and important history in theoretical, engineering and fluid mechanics. It can only be speculated why Glauert did not use this elegant and useful method in his original derivation. Therefore, it is of interest to fill this gap. Concerning “new physical interpretation” the referee has to note that important work on clarifying and explaining the somewhat dense text of Glauert has been published by J.N. Soerensen and G. van Kuik.

b) Paragraph 2, Lines 13 to 24

It may be of interest to note, that the asymptotic structure ($\lambda \rightarrow \infty$) of $c_p(\lambda)$ has been investigated already in 1955 to forth order in inverse TSR with apparently no connection to Glauert’s approach [2].

In addition, it does not seem to be clear what the author means by “very high tip-speed ratios” (TSR). As far as the referee knows, there are a lot of designs with TSR in the order of 10, which may be regarded as a high TSRs.

c) Paragraph 3, Lines 26 to 38

The referee agrees that this regime ($\lambda \rightarrow 0$) is more delicate and - as implied in my statement above - of minor if no practical interest.

d) Paragraph 4, Lines 40 to 46

Here we also do not agree with the author for reasons also implied in what was written above.

e) Paragraph 5, Lines 48 to 58

The referee agrees that details of the calculation could have been part of “supplementary material”. It would also be interesting to know whether some specific codes (MATHEMATICA, etc.) have been used. Publishing a possible source code would increase transparency. Again, we do not agree that Glauert’s model “has no place in any realistic wind turbine analysis”.

f) Paragraph 6, Lines 60 to 68

The paper of Tyagi and S. Schmitz has been reviewed by two experienced scientists. A press release of Sliman (2025) is not under consideration here.

g) Paragraph 7, Lines 70 – 81

This is a serious point but has already been addressed by the editorial board of Wind Energy Science.

3.) Technical Corrections

This comment is clearly written and needs no technical corrections.

4.) References

[1] J.N. Soerensen, General Momentum Theory for Horizontal Axis Wind Turbines, Springer (2016)

[2] G. Schmitz, Theorie und Entwurf von Windrädern optimaler Leistung (Theory and Design of Wind Wheels with optimum Power), pp 379 – 391, Wiss. Z. d. Univ. Rostock, Germany (1955)

[3] J.G. Leishman, Principles of Helicopter Aerodynamics, 2nd Ed., Ch. 13, Cambridge University Press (2006)