Prof. Gordon Leishman's comment reviews and critiques the paper *"Glauert's Optimum Rotor Disk Revisited – A Calculus of Variations Solution and Exact Integrals for Thrust and Bending Moment Coefficients"* by Tyagi & Schmitz, which was accepted and published in the *WES* journal in 2025. In his comment, Prof. Leishman raises concerns about the paper's limited new insight, novelty and practical value, as reflected (at least) in the following statements:

"While (the paper) it ultimately offers a seemingly mathematically rigorous reformulation, it contributes limited new insight to the wind energy field".

and:

"Although mathematically elaborate, their derivation of "exact" integrals for thrust and bending moment coefficients has limited practical relevance and does not materially advance rotor theory or wind turbine engineering"

and:

"Furthermore, the paper fails even to acknowledge the real possibility of tip losses, finite blade count, profile drag, wake expansion, or non-uniform and yawed inflow"

Having carefully reviewed the manuscript, the two peer reviews, and Prof. Leishman's comment, I find myself in agreement with his preceding comments/statements that the paper's contribution is incremental relative to Glauert's original work on the actuator disk method and that although mathematically accurate it does not have a substantial practical value. The work follows closely the lines of the original actuator disk method and provides some addendum to this work by analytically investigating the limiting behavior of the method and providing analytic expressions for the optimum rotor loads. I would even argue that the use of the term "amendment" is inappropriate, as it implies a revision of the original method rather than an elaboration. Nevertheless, even if the contribution is modest and does not introduce new insights or directions for actuator disk theory—serving instead to expand upon the existing formulation—the crucial question for the review process remains whether the contribution is indeed novel and whether it deserves attention and broader dissemination within the wind energy community. The answer to this question ultimately determines the decision to accept or reject the paper. On this point, considering the large and active research community within the wind energy sector that continues to explore theoretical aspects and variations of actuator disk–based models developed/originated by Glauert, I would not oppose publication. My reasoning is based on the fact that the mathematical derivations presented in the paper represent original work that, to the best of my knowledge, has not been published elsewhere by other researchers. Admittedly, a point of concern is the high overlapping degree of the present work with the authors' prior conference paper; however, I defer to the editorial board's judgment on this matter.

In summary, the paper does not constitute a major advancement of the actuator disk method and does not address limitations inherent in Glauert's original assumptions. However, it provides a mathematically rigorous elaboration on aspects of the original method that have not previously been treated in the literature and, in this respect, is of interest to the wind energy community. Using Prof's Leishman own words, "while the work may be of limited academic interest to those studying the historical development of rotor theory," I would assure him that, to the best of my knowledge, this interest is by no means limited within the wind energy academic community.

In the second paragraph of the comment, the author of the comment addresses the relationship between the circumferential induction factor and the torque generated by the rotor. This is my primary point of disagreement with his criticism, as I believe the author of the comment misinterprets the way the paper employs the notation λ. The authors, on multiple occasions, either the notation λ or λ_r. The former denotes the operational tip speed ratio of the rotor, while the latter represents the local tip speed ratio, referring to the non-dimensional radial position along the blade span rather than the rotor's operational state.

To obtain any integral quantity of the rotor for a given operational condition (as indicated by the λ value), one must integrate the radial distribution of λ_r from 0 to the respective λ value. Therefore, the tendency of a' towards zero in Figure A1 (the figure implied by the author in his comment) does not imply that a' approaches zero as λ tends to infinity; rather, it indicates that a' tends to very small values toward the tip of the blade. As seen in the figure, a' remains significant near the blade root. It should be noted that this refers to the dimensionless axial induction coefficient and not to the swirling velocity toward the tip, which, when multiplied by $\omega \cdot r$, becomes non-negligible. Furthermore, it only refers to the optimal induction distribution.

By integrating the local moment distribution along the blade span, which depends on the local a' values, one can obtain a consistent calculation of the rotor torque. If the distribution of a' is that of figure A1 then torque is maximized. To further support this point, I would add that it can be readily shown that applying the angular momentum equation—which is used to calculate rotor torque— is equivalent to applying Bernoulli's law in the rotating frame (as done in the paper and expressed by Eq. (4)), yielding exactly the same a' distribution as that calculated in the paper when the flow assumed inviscid. I would also add that as earlier mentioned, the theoretical background of the work presented in the paper does not deviate from the original Glauert method. Therefore, obtaining zero torque values for very high λ conditions would indicate an unknown limitation of the original actuator disk method.

Based on the above, I would recommend the author to revise his following comment:

"The authors incorporate swirl into their equations through the angular induction factor; however, in the high-l limit, they assume a'->0, implying that the turbine imparts no swirl and, consequently, no torque. This issue introduces a fundamental contradiction in their theory, i.e., a turbine cannot extract power without torque. While the integrals they have derived may be mathematically consistent, they do not apply to a physically realizable situation."

The comments in the third and fourth paragraphs are reasonable and are consistent with the discussion presented at the beginning of my review. They address the practical applicability and relevance of the work to the design of modern wind turbines. Once again, the key question is not whether the paper provides groundbreaking information that will directly advance turbine design, but whether it presents material that is of interest and value to the scientific community.

I agree with the comment in the fifth paragraph concerning coefficients and decimals in the derived expressions and would recommend that the journal issue guidelines to ensure the transparency and reproducibility of the derived equations. I do not though agree with the last comment in the same paragraph:

“Their model assumes a wind turbine with an infinite number of blades that have a continuously optimal span loading at any λ. This is a mathematical abstraction that has no place in any realistic wind turbine analysis, particularly in the high and low-λ regimes that the authors specifically emphasize.”

The paper addresses the calculation of the loads of rotors (not a single rotor) —specifically, the integral rotor thrust and bending moment—with infinite number of blades, optimized for maximum torque operation at various λ values. The expressions for the integral rotor loads derived in the paper refer to the respective optimum/design λ value. Naturally, the actuator disk method, when combined with the blade element method, can also be applied to calculate rotor loads at off-design operating points, as the method itself imposes no such limitation. In summary, the information provided in the paper is the loading of different rotor designs (optimized for different λ values) at their design point and not the loading of one single rotor that operates optimally at all λ values. I would therefore recommend the author to revise his comment.

The comment in the final paragraph has already been addressed by the editorial board, so I have nothing further to add. For the sake of completeness, I would only note that the author of the comment is, of course, correct.

Turning to the comment in the sixth paragraph, I must admit that this is the most delicate point and deserves some discussion and attention. In the text of the paper, the authors never claim to present a groundbreaking work that would open new horizons or possibilities in rotor design. In my humble scientific opinion—and only if I am entitled to judge as I was not a reviewer of the original paper—this is indeed not the case. I hope the authors acknowledge this fact; otherwise, any further discussion on the matter would be pointless. Even the use of the term “amendment” instead of “addendum” appears to be inadvertent, probably without any intention to overestimate the significance of the work (at least it seems so). On this point, I can defend both the authors and the review team. However, I cannot agree with, nor endorse, any decision to promote scientific work in the way this particular study has been publicized in the press and media. Therefore, I agree with the comments of the author in this paragraph.

Whether the journal should implement a specific policy in similar cases in the future is, of course, a matter for the editorial board. That said, the adoption by the journal of the “peer reviewed comment” mechanism is already a step in the right direction.