Review #3 Rebuttal

It is refreshing to see a review from someone who has clearly studied both the article and my Comment in detail. Yet again, there is an admission by a reviewer that the original article is mathematically incremental, modest in its contribution, and of limited practical value. These reviews all reinforce my central point: the results have **no design relevance** because both limits treated are non-operational for wind turbines. Publishing exact integrals in limits where either the physics is invalid or the operating point is non-existent does not advance rotor engineering.

At the same time, it is evident from the tone of this review, as well as the two others, that the editors have gone to extraordinary lengths to solicit critiques of my Comment, searching for any conceivable flaw. One can only wish the original article had been held to the same standard. Had it been, the contradictions, mathematical mistakes, unexplained constants, and lack of physical relevance would have been obvious at the time of submission, and publication would almost certainly not have followed.

Reviewer:

The work [the originally published article] is original enough in derivations, modest, and arguably publishable for community interest. Overlap with the prior AIAA conference paper is concerning but left to the board's judgment.

Rebuttal:

The reviewer concedes the article is "incremental" and "modest." That alone supports my central argument that the work introduces no new physical insight and has no engineering value. A thorough review of the original article would have brought out the points that have now been disclosed, and almost certainly would have resulted in a decline to publish decision by any reputable journal. "Community interest" cannot substitute for novelty that advances rotor or wind turbine theory or informs design. As for overlap, the problem is not that a journal article follows a conference paper, which is common practice. The problem is the **failure to cite the 2024 AIAA conference paper**, which created the false appearance of novel new work and exclusive originality to the journal editors and its reviewers.

Reviewer:

You misinterpreted λ versus λ_r. Figure A1 shows a' tending to small values only near the tip, not globally as $\lambda \to \infty$. Integrated torque remains finite because a' is significant inboard.

Rebuttal (revised):

I acknowledge the mathematical distinction between the global tip-speed ratio $\lambda = \Omega R/V_\infty$ and the local value $\lambda_r = \Omega r/V_\infty$. But this distinction the reviewer makes is flawed and departs from the underlying assumption of momentum theory, where λ is treated as uniform across the disk. Introducing λ_r as a spanwise parameter effectively muddles actuator-disk theory with blade-element concepts, which is inconsistent with the theory's own foundation. This is not a misinterpretation on my part. If momentum theory is applied as originally formulated by Glauert, then in the limit $\lambda \to \infty$ the swirl factor a' tends to vanish, torque tends to vanish, and yet the mathematical formulation still suggests finite power, which is a contradiction exactly as I noted in my original Comment. However, the overarching point is that **real wind turbines never approach this regime and therefore never operate in it**. One can always arrange the algebra to be self-consistent, but this does not confer physical reality. A turbine cannot generate power without torque, and the global asymptotic limit remains a non-physical construct with no engineering meaning.

Reviewer:

Your critique of the low-λ limit becomes a general criticism of momentum theory. The article merely offers analytic expressions, which may be of interest.

Rebuttal:

My point is certainly not a general dismissal of momentum theory but a critique of its **misuse outside its valid domain**. In the low-λ regime, the rotor is operating under conditions of stall, turbulence, and powerful three-dimensional effects. The fundamental assumptions of the actuator disk model, i.e., steady, axisymmetric, one-dimensional (uniform), inviscid flow, **collapse entirely under such conditions**.

Presenting "exact" integrals in this regime does not extend the theory; it applies the theory where its assumptions are **invalid**. The fact that one can write down a closed-form solution does not confer physical meaning. No matter how neatly mathematically expressed, their momentum theory results cannot represent the performance of a stalled rotor with recirculating flows. The results, therefore, are disconnected from both experiment and design practice, and therefore have **no engineering value**.

Reviewer:

The article does not claim one rotor optimal across all λ, but rather a family of optimal rotors each tuned to a specific λ.

Rebuttal:

This reframing does not resolve the issue. Momentum theory is based on the actuator disk model, which is equivalent to an **infinite-bladed, inviscid, tip-loss-free rotor with uniform loading**. Within this framework, there cannot be a "family" of rotors; there is only a single idealized construct. The reviewer's notion that somehow there are multiple optimal rotors is therefore a contradiction not afforded by the theory itself. Whether described as one rotor or as many, the construct remains a mathematical abstraction that does not correspond to any realizable wind turbine. The distinction does not restore physical relevance to my original Comment on this matter.

Reviewer:

The unexplained constants should have been better handled, and the journal should issue guidelines.

Rebuttal:

Agreement noted. But this is not cosmetic. Numerical constants presented without derivation or explanation prevent reproducibility and transparency, so they appear

mysterious, even arbitrary, to the reader. Their publication without explanation is clear evidence of **superficial review**. A genuine derivation by the authors would have explicitly stated: "Evaluating at the lower limit $a = \frac{1}{4}$ gives the constants -10.5082 and -13.3272." Instead, the authors just give the numbers, and the journal prints them with no provenance. The journal already operates under a maze of confusing "guidelines" and a labyrinth of review procedures, which is unprecedented in my experience. We have already seen how these journal editors do not follow their own guidelines. Ironically, my Comment has been subjected to far more detailed and rigorous scrutiny than the original article itself, which suggests a double standard is another part of the journal guidelines.

Reviewer:

The two integration-limit errors are acknowledged.

Rebuttal:

Again, this confirms my point. Transforming variables without correcting integration limits is a basic mathematical error. The fact that both mistakes passed into print demonstrates that the review process was cursory.

Reviewer:

The media promotion of the article was inappropriate, but the journal cannot control press releases.

Rebuttal:

Agreement noted and crucial. A truly independent reviewer would not spend their effort defending the journal's procedures, but the issue remains and requires resolution. Under the banner of *Wind Energy Science* the authors themselves promoted the article as solving a "100-year-old problem" and unlocking "new design possibilities." I counted over 20 outlets that had broadcast this false claim to the general public. This was false information. Glauert never considered his

formulation a problem; it was already accepted that the limits had no practical design relevance. This article was first promoted in a Penn State University press release and then propagated widely across the internet, creating sensationalism around a "problem" that never existed. This misled both the public and the professional community. The journal cannot disclaim responsibility here: by publishing without correction and allowing such coverage to stand unchallenged, *Wind Energy Science* and its editors have given credibility to a false narrative.