Dear editor,

We have addressed the remaining comments as detailed below and verified that the equation corrections do not affect the reported results. We look forward to publication.

Dear authors,

Thank you for the revised manuscript. After a final review, I identified a few remaining inconsistencies that I would like you to address before publication.

- Page 11, lines 224-232: Equation (12) currently has units of power multiplied by area. It appears that a factor 1/A is missing. Otherwise, the dimensions are inconsistent. Furthermore, if Equation (13) is intended to represent the contribution of turbulence alone, I would expect it to approach zero as the turbulence intensity tends to zero. Please verify that this is indeed the case, including for a nonuniform mean wind profile, and check the consistency between Equations (11)-(13).

We have added the missing factor 1/A to Equation (12) to ensure dimensional consistency. We have also revised Equation (13) to subtract the corresponding zero-turbulence rotor-area average, evaluated using the same mean wind profile. This ensures that the turbulence contribution tends to zero as turbulence intensity vanishes, including for nonuniform mean profiles. Finally, we have clarified how this correction is added to the analytical mean-profile power estimate.

- Eurocode turbulence length scales: The revised formulation appears correct, but the manuscript does not specify the value of z_0 (roughness length) used in the simulations. This should be documented for reproducibility. Was z_0 = 0.1 m used, as in Fig. 6?

Yes, z_0 = 0.1 m was used in the simulations, consistent with the value used in Fig. 6. We have now explicitly stated this value in the description of the Eurocode turbulence length-scale formulation.

- Page 35, Appendix A3, lines 655-661: Shouldn't the coefficient c_2 have a negative sign? There appears to be a minor algebraic error in the derivation.

Good catch! We added the minus sign.

- Equation (A12): Please check the dimensional consistency. Since alpha_max is an angle (expressed in radians), the term containing alpha_max^2/R^2 is not dimensionless. Should R^2 be omitted from the final expression?

Indeed this cancels out with the R^2 in the previous expression. Thank you for catching this mistake.

- Page 25, lines 430-434: The expression "(Analytical - IEC)/IEC" should be presented

using proper mathematical notation, with clearly defined quantities. Please also verify the sign convention, as the reported positive bias reductions appear to be based on the absolute values of the biases.

We have clarified the calculation in the text. Bias reduction is expressed as the percentage decrease in absolute bias relative to the hub-height-based reference approach, with positive values indicating improvement.

- IEC reference approach: This is an issue I have raised several times during the review process. The manuscript repeatedly describes the reference method as "the IEC-standard approach", based on a single wind-speed measurement. This is potentially misleading, since IEC 61400-12-1:2022 already includes procedures involving rotor-equivalent wind speed and the treatment of wind shear, veer, and turbulence. I strongly recommend using the term "the hub-height-based reference approach" instead of "the IEC-standard approach" throughout the manuscript, where this accurately describes the method used for comparison.

We have removed every mention of the IEC-standard approach and changed it ot hub-height-based approach.

- Abstract: The statement "This equates to an annual energy production estimate that is 5.2 +/- 3.9% closer to reality" is an overstatement. The observational dataset covers only a limited number of selected wind events, which is insufficient to establish an improvement in annual energy production estimates. The statement should be restricted to the improved power or energy estimates for the investigated events.

The annual-energy claim has been removed; the reported improvements are restricted to the investigated events.

- Polynomial order: Section 3.6 specifies a third-order polynomial fit. However, page 27 states that the method can also be applied when only two measurement levels are available. Please clarify that a lower polynomial order would be required in such cases.

We have added the following clarification to Section 3.6: “Although third-order fits are used in this study, the formulation also supports lower polynomial orders when fewer measurement levels are available. A third-order fit requires at least four distinct measurement heights; with only two measurement heights, a linear fit (N=1) must be used unless additional constraints are imposed.”

- Computational speed: The abstract claims that the analytical model is at least 1,000 times faster than OpenFAST. Without a documented computational benchmark, this quantitative statement is insufficiently supported. I suggest removing the numerical speedup claim rather than introducing additional benchmarking at this stage.

The quantitative speedup claim has been removed.

Overall, I appreciate the efforts made to address the previous comments, particularly the revision of the turbulence length-scale formulation. I consider the manuscript suitable for publication once these inconsistencies have been addressed. I do not expect additional numerical experiments, provided that the corrections to the equations do not affect the reported results.

We appreciate your feedback, and are glad that our efforts of rerunning the full simulations of this paper have paid of to make a clearer turbulence length-scale formulation. We have verified that the identified equation errors were confined to the manuscript text. The implementation already used the correct formulation, and the reported results remain unchanged. No additional numerical experiments were therefore necessary.

Yours sincerely,
Brandon van Schaik
On behalf of all co-authors