Reviewer #1

Major remarks

We are disappointed that our remarks on the previous version of the article have not been taken into account, contrary to what the “answer to reviewer” document says. Some section titles have been changed, and some content has been moved, but the order in which the paper flows is still the same, meaning the methodology section remains messy and difficult to follow. In particular, the OpenFAST configuration is still split between two sections (2.2 and 2.4.2), and the power curve retrieval and analysis is split between three sections (end of 2.1.2, 2.1.3, and 2.2.3).
We can understand that, given the report produced by the second reviewer (that says the paper is clear), a choice was made to make minimal corrections and keep the article structure unchanged. However, we also requested more explanation about the way turbulence is taken into account in the analytical model, which were not provided by the authors. In particular, the article text does not allow relating the theory to the numerical implementation, i.e., the outputs of the boxes in the block diagram (Fig. 6) do not match the mathematical notations introduced in Sec. 2.1.

Firstly, the output from the “Power curve fitter” box on Fig. 6 is the power coefficient Cp, so that Sec. 2.1.2 should explain how Cp is determined after the Ppc(v) curve fitting (or a reference provided).

Secondly, the outputs of the “Taylor expansion” box on Fig. 6 are two variables called cv and cσ_v that are not defined in Sec. 2.1. My guess is that they are the Taylor coefficients resulting from the expansion of the wind profile (cv), but also of the expansion of the wind standard deviation profile (cσ_v), but no explanations are given about this second Taylor expansion anywhere.

Sec. 2.1.2 must absolutely explain how the dependency of wind standard deviation upon altitude is taken into account in the analytical model (see also detailed comments about Eq. 7 and 8).

Similarly, the idealized wind profiles (Sec. 2.3) are not associated with an idealized turbulence profile, so it is not known if turbulence was included or neglected in these test cases (see also detailed comments about this section).

At this stage, readers wanting to implement the method proposed in this paper using another programming language would need to do some reverse-engineering of the code provided on Zenodo to really understand what was done. The point of an article is to allow the reproducibility of the work (not just running the code provided by the authors, but really understanding what it does), and this paper does not provide sufficient

explanations to fulfill this purpose, no matter what the other reviewer said about its clarity.
Therefore, our recommendation is again “major revision”, hoping that our remarks will be understood and properly answered this time.

Chapter 2 & 3: We thank the reviewer for this detailed and candid assessment. We regret that the previous revision did not address the structural and methodological concerns sufficiently. We have therefore treated this comment as a major revision request and have substantially overhauled the structure and content of the methodological part of the manuscript.

Following both the reviewer’s criticism and the editor’s recommendation, the former Methodology section has now been split into two separate sections. The first section, “Background and theoretical framework”, introduces the established concepts and data sources needed to understand the study, including rotor-equivalent wind speed, the turbine power equation, vertical inflow variability, turbulence notation, the Gotthard field site, and the OpenFAST/TurbSim framework. The second section, “Methodology”, now describes the actual implementation used in this paper, including the Taylor-series rotor integration, power-curve fitting, power-coefficient retrieval, analytical treatment of turbulence, idealised profile experiments, OpenFAST simulations, and the Gotthard application. This separation is intended to remove the previous back-and-forth between theory, numerical setup, and observational application.

We have also revised the analytical description so that the notation in the manuscript now matches the workflow diagram. In particular, the output of the power-curve fitting step is now described explicitly: the fitted power curve (P_{pc}(U)) is first obtained from the turbine power-curve data, after which the corresponding power coefficient is computed as (C_p(U)=2P_{pc}(U)/(\rho A U^3)). This clarifies how the (C_p) input used in the analytical rotor integration is obtained.

In addition, the Taylor-expansion step has been rewritten to define both sets of coefficients shown in the block diagram. The coefficients (c_v) now refer to the Taylor expansion of the 10-minute mean wind-speed profile (\bar{v}(z)), while (c_{\sigma_v}) refers to the Taylor expansion of the height-dependent wind-speed standard-deviation profile (\sigma_v(z)). The manuscript now explains explicitly how (\sigma_v(z)) is represented over the rotor layer and how this height-dependent turbulence statistic is combined with the turbine power curve to estimate the expected power contribution from turbulence.

Finally, the idealised profile section has been revised so that each test case states whether turbulence is included or neglected. Uniform, shear, veer, and logarithmic wind-speed profiles are now treated as non-turbulent unless otherwise specified, while the homogeneous-turbulence case is defined separately using a uniform mean wind profile together with a prescribed wind-speed standard deviation. We believe that these

changes make the methodology substantially more reproducible and allow the reader to implement the proposed method without needing to reverse-engineer the archived code.

Minor remarks

1. We do not agree with the removal of units in the text when referring to the numerical value of physical quantities, even if these quantities have been previously defined. For instance (line 435), writing “the sudden drop of the OpenFAST curve at v = 12.” instead of “v = 12 m s−1.” makes no sense. Following this logic, why not suppress the degree symbol after the shear angle values (lines 451-452)? Also, the m s−1 unit is still present in other sentences (line 498), so this unit-removal policy has not even been applied everywhere.
   Throughout the manuscript: We have harmonised the use of units in the manuscript

2. We agree that square brackets can be used to give the associated unit when defining the notation of a physical quantity, but brackets should not be used when giving numerical values. There are still some left in the text (e.g., with the surface roughness line 315 and in the legend of Fig. 7). Also, some units are still in italics (line 498) and should be turned into regular font. Other small points mentioned in the previous review have not been corrected (see for instance detailed comment about lines 315 or 457).
   Thorough work is still needed on the reference list to stick with WES guidelines, i.e., provide the DOI for all publications that have one and use journal name abbreviations. The latter can be found for instance here: https://woodward.library.ubc.ca/woodward/research-help/journal-abbreviations/. Also, many references are incomplete in the sense that the type of document is not identifiable (see detailed comments for examples).
   Throughout the manuscript: We have harmonised the units, brackets, font usage and reference list.

Detailed remarks

Introduction

1. Line 55 (Eq. 1). The notations Ai and A must be defined.

L56: "where u_i represents the wind speed measured within rotor-area segment i, A_i is the area of that segment, and A is the total rotor-swept area."

2. Line 75. Minus sign differs from a simple dash and must be coded as $-$ in Latex.

L77 and more: Changed as suggested.

3. Lines 100-105. Detailed information about the study site (altitude) should be given in Section 2. Same for detailed information about the lidar observations (e.g., vertical/time resolution). In the introduction, you can just say it is a mountainous site with complex terrain where wind profiles were measured using lidar during a field campaign.

L105: We agree that this belongs in Section 2. It is already described in there, and thus we replace this paragraph by briefly describing the complex nature of the site:

Sec. 2.4: "The analysis focuses on a mountainous site with complex terrain at the Gotthard Pass in the Swiss Alps. During a field campaign, wind profiles were measured using LiDAR, providing observations of vertically variable flow conditions relevant for wind-turbine power estimation in complex terrain."

4. Line 101. The acronym SCADA is not defined.

This acronym has been removed as it was not relevant.

5. Line 109. The introduction is supposed to end with an announcement of the article's plan.

L108-114: We have refined this final paragraph to better announce the article's plan:

"This article is structured as follows. First, we introduce an analytical formulation for estimating wind-turbine power generation by integrating wind velocity over the rotor plane, with particular attention to idealised wind profiles of vertical shear, veer and turbulence. We then benchmark this formulation against OpenFAST simulations under identical inflow conditions to identify remaining sources of uncertainty. Finally, we apply the approach to standard wind-profile measurements from the complex-terrain field campaign to evaluate its relevance for power prediction under realistic atmospheric conditions."

Section 2.1

6. Line 111. “Equation 2” should be abbreviated “Eq. 2” as it is in the text. The word Equation should be used only when it appears at the beginning of a sentence. The same holds for sections, figures, and so on, as mentioned in the WES guidelines. This was already said in the remarks on the previous version of the article, it should be good to take it into account and correct it throughout the manuscript.

Throughout the manuscript: We have addressed all cases where Equation was written in full before, this has now been replaced by Eq., except if it is in the beginning of a sentence. This also includes the mentions of Equations in Figures etc.

7. Line 112 (Eq. 2). There should not be a dependency upon z in P and v in this formula, because as you indicate, the wind speed is an equivalent wind speed over the rotor-swept area. It would be clearer to remove the z dependency and replace v with veq (which can be the Ueq from Eq. 1 or the new formulation you are about to introduce).

Eq. 2 and surroundings: We follow the reviewers’ advice to use U_eq for this case, and introduce the vertical dependency later as we introduce the idea of the vertical variations.

8. Lines 119 and 121. There are two redundant sentences here: “vertical variations across the rotor-swept area are usually the dominant factor, often far exceeding horizontal deviations” and “horizontal variations across the rotor disk are usually minor compared to the strong vertical gradient in wind speed,”

Sec 2.2 and 4.3: We thank the reviewer for pointing out this redundancy. We have combined this discussion into Sec 2.2 and 4.3.

9. Figure 1. There is a h remaining on the side of the triangle, please replace it with z for consistency.

Fig. 1: We apologize for this oversight, it has been addressed now.

10. Line 130. “Where R[m] is the radius of the rotor-swept area of the turbine,”. Here, you specify the unit for R, as if you introduce the unit of a new notation. However, R was already used on line 115 and its unit was not defined here.

Throughout the manuscript: We have removed the units in the specified line and moved them to the first time the variable is defined.

11. Equation 5. To what speed exactly does the v dependency of P and Cp refer? Is it the equivalent wind speed in the rotor swept area, which is actually the cubic root of the summation term in the formula? If so, it should be said, and a more explicit notation than v should be used in Eq. 5. If otherwise, please explain.

Eq. 9 and surroundings: Indeed, the Cp(v) dependency on v refers to the Ueq, as we have now changed that notation in equation 2, and 3 according to the notation of

equation 1 (see Specific Comment 7), this now naturally follows the derivation. U_eq is already defined for equation 1, and now two different variables are used to show the difference in dependencies between v(z) and Ueq.

12. Lines 157-158. “(recommended at N = 3 providing a good trade-off between capturing essential behaviour and avoiding over-fitting)”. This has already been said above and should be removed here.

Removed as requested.

13. Line 164. “We refer the reader to the Data Availability Section of this manuscript for more details.” There is no “Data Availability” section, but rather “Code and data availability”. Moreover, this section should appear after the Appendix.

Code and data availability section: We have changed it to Code and data availability Section, and move the respective section after the appendix.

14. Lines 167 and 171. “Student-t distribution” the dash is at the wrong place: “Student t-distribution”.

L153: Fixed the positioning of the hyphen.

15. Equation 7. It is not clear if this wind speed distribution applies to the equivalent wind speed in the rotor-swept area or to the wind at each altitude of the profile. In the first case, the notation v bar actually corresponds to the cubic root of the summation term in Eq. 5, which should be said (and a more explicit notation such as Veq should be used). And then, you need to explain how the variable σv in Eq. 7 is computed from the profile of wind speed variances using, I guess, the Taylor expansion of this profile. In the second case, you need to explain how the wind speed distribution at each altitude is related to the wind speed used as an integration variable in Eq. 8. These explanations are mandatory to add because their absence makes it impossible to understand the processing of turbulence in your analytical method.

Sec 2.3 and Sec. 3.3: We have revised this subsection completely to reflect the two different Taylor expansion for the mean wind speed and turbulence.

16. Lines 181-191. Explanations about the power curve model should be moved to Sec. 2.1.3. As said in the major comments, Sec. 2.1.2 must explain instead how the dependency of wind standard deviation with altitude is taken into account.

Sec 3.2 and 3.3: We have revised this subsection completely based on a suggestion from the editor to split our original methodology in two chapters.

17. Lines 193-194. “The analytical formulation of Equation 9 is fitted to the published power curve of the Enercon E92 2.35 MW wind turbine using manufacturer data from the technical specifications.“ A reference is missing here.

L212: We have added the reference in question.

18. Lines 195-196. It should be specified which parameters are fixed following the turbine technical specifications (likely vin, Pr, and vout) and which parameters are actually fitted (B and C).

Eq. 8 and surroundings: The reviewer is correct in identifying the right fixed constants. This is now defined more clearly.

19. Figure 2. The legend is wrong “Fit (Eq. 7)” should be corrected as “Fit (Eq. 9)”.

Fig. 3: This error has been corrected.

20. Sec. 2.2.2. Please avoid one-sentence paragraphs.

We have removed the erroneous line endings.

21. Line 249. “from 1 to 25” missing wind speed unit.

This part was replaced and not relevant anymore.

22. Lines 255-256. “Remaining with Figure 3, the OpenFAST simulations cannot be based on the Enercon E92 directly, as the required blade geometry to simulate power production is proprietary. Instead, the open-source WindPACT 3.0 MW turbine is used.” All this has already been said, please do not repeat.

Repetition has been removed.

23. Figure 3a. Here, Cp is called coefficient of performance, while it was defined as power coefficient at the beginning of Sec. 2. Pointing it is the same thing somewhere might be good.

Chapter 3: We are now consistent in using power coefficient for Cp.

Section 2.3

24. Equations 10 to 12. If we follow Eq. 10, then ΔPshear,an should simply be noted ΔPshear, precising that Pref corresponds to the uniform profile. Please be consistent with your mathematical notations.

Eq.15: We have removed the “, an” from eq. 11 and 12.

25. Lines 302-303. “−R and R and the bottom and top of the swept area of the blade”. This has already been defined at the beginning of Sec. 2. The point of defining

unequivocal mathematical notations is to be able to use them throughout the paper without having to re-explain them each time they appear. While we are talking about notations, v bar usually denotes the speed average from Reynolds averaging, so that vhub or v0 would be a better choice to denote the wind speed at hub height.

Sec 4.1: We remove the additional explanation of -R and R as it is redundant.
We have replaced this \overline{v} by v_0 as the reviewer suggests (including in the appendix).

26. Lines 309-312. What is the wind profile associated with the homogeneous TI? And conversely, what is the profile of TI associated with the other idealized wind profiles? It seems to us that for each idealized profile, both the wind and the TI profile should be defined. If you assume the absence of turbulence in the uniform, linear shear/veer and logarithmic profiles, it should be said.

L239-247: It is over the reference case, i.e. the uniform profile. The item now includes this.

27. Line 315. “Von Kármán” should be corrected to “von Kármán” with a lowercase v, as already indicated in the previous review.

L175, 244, 257: Removed the capital letter.

28. Lines 318-321. A three-line section is not relevant. Either you need this section to explain the metrics used to assess the model performances, or you can merge 2.3.1 and 2.3.2.

L303-310: We have removed this subsection header and merged it with 2.3.1

Section 2.4

29. Lines 329-330. “This wind farm is situated 2106 m a.m.s.l., north–south-oriented Gotthard Pass in the central Swiss Alps.” Some words are missing, please correct.

L158-162: We have fixed this sentence.

30. Equation 13. It would be better to replace the constants 50/46 and 12.478 with appropriate notation and define their values in the text. This would help readers to adapt the method to other sites/turbines with different altitudes/sizes.

Eq. 16-17: We have adapted the equation by adding an extra = with only the variables of the blade length (R), and nacelle height (h) of WP3.0 and E92. These additional variables are now introduced in the text below.

Section 3.1

31. The discussion has significantly improved in this section, notably by making better connections with the analytical formulas.

Sec 4.1: We thank the reviewers for the positive feedback, we appreciate your input and agree that your comments have significantly improved the manuscript.

32. Lines 455-457. “The ΔP curves of homogeneous turbulence, shown in Figure 7c, has a characteristic shape which has previously been described to follow this relationship between power curve curvature and power contributions related to turbulence in similar works such as ?” This sentence is awkward. Maybe: “The ΔP curves of homogeneous turbulence, shown in Figure 7c, have a characteristic shape: such a relationship between power curve curvature and power contributions related to turbulence has been previously described by [reference].”

L355-358: We have adopted the reviewers’ suggestion and replaced the sentence as requested.

33. Line 457. The missing reference signalled in previous version of the paper is still missing here.

L356: Our apologies for the typo in the bibtex reference.

Section 3.2

34. Lines 522-523. As previously mentioned in the first review, the percentages provided here are incorrect. Even if you specified the formula in the new version of the article, it is not clear how you obtained a minimum bias reduction of 8.6 % and a mean of 25.4 % (should be 7.8 % and 27.2 % instead). Please correct the values accordingly (in the abstract and conclusion as well).

L10, 440-466, 580-619: We corrected the values in the text, conclusion and abstract to the correct values. Secondly, the text has been adapted to explain how we calculate the minimum and mean from the equation that we added in the last version:

Section 3.4

35. Line 579. “the simpler configuration which are comparable” should be “which is”.

L531: Fixed

References

36. Line 771, Albers. If this is a book, a publisher should be indicated.

References: It is an article in proceedings from Windguard, we have added this publisher and a link to the article.

37. Line 772, Antoniou et al. The web link points to a presentation, if this was shown at a conference/workshop, the name of the gathering should be provided.

References: This presentation is a Siemens workshop, which has been added to the citation onw.

38. Line 774-775, Belu et al. We are unable to say what type of document this is. Was it a conference proceedings, a journal article, or something else? A web search only points to a PDF document available on ResearchGate, which cannot be considered an official repository.

References: It is a conference proceedings, it is in fact not only available on ResearchGate, one can find the proceedings here: https://ieeexplore.ieee.org/document/6304638. We have improved the citation accordingly.

39. Line 776, Bot. This one is a report, with a report number, so it should be cited as "ECN report number XXX" or something alike rather than simply "ECN". Also, it is available online (https://publications.tno.nl/publication/34629395/v931oD/e16041.pdf), so the link could be provided.

References: We have added the report number ECN-E—16-041, and improved the publisher to TNO with the specific location. We also added the url.

40. Lines 777-779, Brown et al. Journal abbreviation should be used, missing page number, and missing DOI.

References: We have improved this citation.

41. Line 780-781, Choukulkar. Journal abbreviation should be used, missing DOI.

References: The journal abbreviation for Wind Energy by John Wiley & Sons is "Wind Energy", which is how it is written in the manuscript. We have added the DOI.

42. Line 786, Emeis. This one is from the Deutsches Wind Energie Institut magazine, so it should be cited as it, with the issue number.

References: Fixed this.

43. And so on… we are not going to go through the full list, but all references should be carefully checked to ensure they comply with the WES guidelines and make the type of document clearly identifiable.

References: We have reviewed every reference and redownloaded the citations from the original source and replaced them in the reference list. After which we have made sure they comply with WES guidelines (journal abbreviations, doi etc…).

Typos:

44. Some section titles (and the article title itself) have capital letters at the beginning of every word, but only the first word should be capitalized.

Title and Section Titles: We have addressed the capitalization as requested.

45. Line 559. The minus sign should be coded in LaTeX math mode (looking like a long dash instead of a short dash).

Throughou the manuscript: We have changed this to a long dash.

Editor

1. **Abstract:** The opening sentence of the abstract is misleading as currently written. Multi-height measurements from meteorological masts and lidar are widely used and are addressed in standards. If the intended point is that standardized power-performance evaluation ultimately reduces vertically varying inflow to a single representative wind speed, such as hub-height wind speed or rotor-equivalent wind speed, please revise the sentence accordingly.

L1-L4: We thank the reviewer for this clarification. We agree that the original wording was misleading, as multi-height measurements from meteorological masts and lidar are indeed widely used and addressed in standards. Our intended point was that standardized power-performance evaluation ultimately reduces the vertically varying rotor inflow to a single representative wind speed, such as hub-height wind speed or rotor-equivalent wind speed. We have revised the opening sentence of the abstract accordingly.

2. **Notation and units:** When introducing notation, for example in Eq. (1), there is no need to include units in square brackets. Line 113 contains expressions such as [kW] and [m s−1]; these can be omitted for brevity and to avoid errors. For example, in the present version, the power output is expressed in watts, not kilowatts.

Eq. 2, Tab. 1., L120-124: Fixed!

3. **Formatting of units:** Units such as m s−1 should not be written in italics. In LaTeX, please write m s^{-1} rather than $m s^{-1}$, so that the unit symbols remain in upright roman font.

Throughout the manuscript: Fixed!

4. **Line 315:** The equation for the logarithmic wind-speed profile should be presented as a separate numbered equation.

Eq. 14: We have introduced the logarithmic wind profile as a number equation now.

5. **Line 359 and elsewhere:** When expressing dimensional quantities, units should be written without square brackets. For example, 12.478 [m] should be written as 12.478 m.

L289-296: Fixed!

6. **Introduction:** The novelty claim should be formulated more carefully. The general idea of replacing hub-height wind speed by a rotor-equivalent or rotor-integrated wind-speed measure is well established, and previous studies have already considered shear, veer, turbulence, yaw misalignment, lidar-derived profiles, and

their effect on turbine power. The manuscript should therefore avoid implying that rotor-area integration or REWS-based power estimation is new. The contribution appears instead to lie in the specific analytical formulation, its computational simplicity, and its application/validation for the Gotthard Pass complex-terrain case study. The abstract and introduction should be revised accordingly, with a clearer distinction between established background and the specific contribution of the present work.

L89-100: We agree that the original wording overstated the novelty of the general concept, since rotor-equivalent wind speed, rotor-integrated inflow measures, and corrections for shear, veer, turbulence, yaw misalignment, and LiDAR-derived profiles have already been considered in previous studies. We have therefore revised the abstract and introduction to distinguish more clearly between established background and the specific contribution of the present work. In particular, **L94-100** reflect this better now.

7. **Line 365 and surrounding text:** A simulation without turbulence does not require the use of TurbSim. I suggest removing the mention of IECKAI, i.e. the IEC Kaimal spectrum, since this becomes unnecessary information once the reader understands that the inflow is non-turbulent.

L173-175, L252-258: We have replaced IECKAI with IEC Kaimal spectrum.

8. **Table 1:** Please ensure that the tables follow the formatting requirements of Wind Energy Science.

Tab. 2: Although it is not completely clear what formatting issues are present in Table 1, we have removed the horizontal lines that are not at the top, after the header or below the table in Table 2.

From the existing formatting requirements on WES Author guidelines we are unable to identify any other issues. We ask the editor to be more specific in the present issue in the proofs if it still persists.

9. **British vs American english:** British english should be used for paper submitted to Wind Energy Science.

Throughout the manuscript: It is now in British English

10. **End of the introduction:** The last paragraph of the introduction should describe the structure of the paper more specifically and explain how each part addresses the research question. At the moment, the paragraph is rather vague and contains terms such as “benchmarking”, which can be ambiguous. Please revise it to make the paper structure and objectives clearer.

L108-114: In combination with the next comment, we have revised this paragraph to reflect the new structure.

11. **Section 2, title and structure:** The title of Section 2, “Methodology”, is somewhat misleading because the section appears to contain both established background material and elements of the methodology developed in the paper. I suggest splitting this material into two sections: (1) one describing the relevant established background, the instrumentation, the measurement site, the literature and (2) another describing how the present paper addresses the research question.

Chap. 2 and 3: We have rewritten this chapter into two chapter as you suggest

12. **General wording:** As a rule of thumb, the word “very” is often unnecessary in scientific writing. Please consider removing it or replacing it with more specific wording where appropriate.

We have removed all mentions of the word “very” in the manuscript.

13. **Goodness of fit / R^2:** The reported coefficient of determination, R2=0.9996, is unusually high for this type of comparison and should be reconsidered. If this value was computed from binned or averaged data, I recommend not using it as evidence of model performance, because binning removes much of the point-to-point variability and can artificially inflate the apparent agreement. Model performance should instead be assessed using the original, unbinned data, preferably with a scatter plot of predicted versus measured power, residual analysis, and error metrics such as RMSE, and bias. If binned data are shown, they should be used only as a visual aid and not as the basis for the main goodness-of-fit statistic.

L389-421 and Fig. 7: We thank the editor for raising this point. However, we would like to clarify that the reported (R^2=0.9996) is not computed from binned or averaged SCADA measurements, nor is it used as evidence of predictive model performance against operational turbine data. It refers only to the fit of the proposed analytical power-curve function to the published/tabulated (P)-(v) power-curve data of the Enercon E92 turbine. In this context, a high (R^2) is expected, because the objective is to reproduce a smooth manufacturer power curve with an analytical approximation.

We have adapted the figure by added RMSE and Bias first, whilst also keeping the correct R^2 = 0.9996 value in there to show excellent agreement in all error metrics.

14. **Section 2.1.2:** My expertise is in atmospheric flow and turbulence, but the first paragraph of Section 2.1.2 is unclear to me. Turbulence is often approximated as a Gaussian stationary random process, which is usually an acceptable approximation, and turbulence generators such as TurbSim also often rely on this

assumption. If the paper aims to introduce non-Gaussian features, this should be explained more clearly. Such features can be introduced through transformations of the generated time series, for example, but the use of a linear scaling factor of 1.81 is unclear in the present version and needs further justification.

L142-157, L223-237: We have clarified this section by presenting it as a Gaussian assumption with a small paragraph noting that we have found a different distribution for the Gotthard LiDAR dataset specifically, and that we did find a linear scaling factor to be satisfactory to use the standard deviation of the Gaussian, even in this case.

15. **Section 2.2:** For clarity, I suggest starting sentences with the subject whenever possible. I also recommend merging paragraphs composed of single sentences into longer, more coherent paragraphs. This would improve readability and reduce fragmentation. I agree with the reviewers that there is some back-and-forth between Sections 2.2 and 2.4.2. These sections should be reorganized to avoid repetition and to present the methodology in a more logical order.

Chap. 2 and 3: Based on your previous suggestion, we have have split the methodology into two chapters where in the background and theoretical framework chapter which has greatly reduced repetition.

16. **Background**: The velocity components (u,v,w), i.e. the along-wind, cross-wind, and vertical velocity components, should be introduced in the Background section rather than first appearing in Section 2.4.2.

L179-181 and in more detail in L740-755: Based on your previous suggestion, we have split the methodology into two chapters where in the background and theoretical framework chapter we describe this clearly.

17. **On Turbsim and USRVKM**: The spectral model USRVKM should be explicitly defined as the von Karman model with userdefined scaling. The choice of length-scale values must be clearly justified. The values used here, L=[3,4,6,9,13,13,13,...] m, are unclear and appear excessively small for atmospheric turbulence at wind-turbine scale which are often around 100-200 m. Moreover, these values seem to come from the TurbSim example input file, which explicitly describes the profiles as “completely made up profiles”. They should therefore not be used as physically meaningful input values. I recommend regenerating new turbulence wind field. The ones used currently would be unrealistic with the current integral length scale values. It should be possible to estimate the integral lengths cale using the autocorrelation function (https://se.mathworks.com/matlabcentral/fileexchange/108944-a-note-onthe-calculation-of-the-length-scales-of-turbulence)

Sec. 2.5, 3.5, App. B and Table B1: We thank the editor for this important comment. We agree that the spectral model and the origin of the length-scale values needed to be

stated more clearly. We have revised the manuscript to define USRVKM explicitly as the TurbSim user-specified von Kármán spectral model, in which the turbulence length-scale profile and component scaling are prescribed by the user. We also agree that the original values (L=[3,4,6,9,13,13,13,...] m) should not be interpreted as physically meaningful atmospheric integral length scales. These values were taken from a TurbSim example configuration and were used as part of the initial numerical comparison, but we now clarify that they are not intended to represent a measured site-specific turbulence climatology.

To address this point quantitatively, we estimated effective integral length scales directly from the 1 Hz WindCube lidar data during the inflow periods used for the OpenFAST simulations. For each lidar height, the line-of-sight radial velocities were separated by beam position. The opposing (0° & 180°) beams were sign-corrected and combined to derive an effective streamwise component, the (90° & 270°) beams were treated analogously for the lateral component, and the vertical beam was used for the vertical component. A CNR threshold of (-23 dB) was applied as suggested by the WindCube manual. The resulting component time series were divided into 10 min windows, the mean was removed, and the autocorrelation function was integrated up to its first zero crossing. The integral timescale was then converted to an effective integral length scale using Taylor's frozen-turbulence hypothesis, $L_i(z)=U_{mean}(z) * T_i(z)$, where ($U_{mean}(z)$) is the mean reconstructed horizontal wind speed at the same height.

This analysis showed that the site-specific effective streamwise length scales are indeed substantially larger than the originally used example values, but also smaller than the generic 100-200 m range suggested in the comment for the selected inflow periods. The median streamwise length scale varied between approximately 52 and 72 m over the lidar height range, with large window-to-window variability, as indicated by standard deviations of approximately 70-100 m. This variability is important: it shows that there is no unique length-scale profile that can be regarded as the physically correct choice for all cases. A case-by-case recalibration of the turbulence length scale for each OpenFAST inflow profile would introduce an additional, poorly constrained source of variability and would shift the focus of the study from the proposed analytical power-estimation framework towards turbulence parameterisation in TurbSim.

We therefore revised the manuscript in two ways. First, we added a clear statement that the originally used TurbSim example length scales are not physically representative and should not be interpreted as site-specific atmospheric integral length scales. Second, we added a sensitivity analysis motivated by the LiDAR-derived effective length-scale

estimates. Because the LiDAR-derived 10-minute estimates show large variability, ranging from a few metres to several hundred metres, we did not prescribe a separate case-specific length-scale profile for every OpenFAST inflow period. Instead, we used an order-of-magnitude increase of the original length-scale profile, together with tests of the component scaling, as a conservative sensitivity experiment. This directly tests whether the OpenFAST mean-power comparison depends on the short USRVKM example length scales. The purpose is to assess the robustness of the comparison, rather than to claim a uniquely optimal turbulence parameterisation. The main conclusions of the manuscript do not rely on the original short length-scale profile, and we have amended the text accordingly.

We additionally performed sensitivity analyses in which the original length-scale profile, L = [3, 4, 6, 9, 13, 13, 13, ...] m, was increased by a factor of ten, and in which the component scaling was changed to (1.0, 0.8, 0.6), as suggested in a later comment. These tests were carried out for the cases where the largest deviations in the velocity spectra were observed, corresponding to wind profiles around 9 m s^{-1}. Even for these deliberately conservative cases, the effect on the OpenFAST mean power was very small. The largest change was approximately 3 kW when the length scale was increased to 10L, approximately 2 kW when the increased length scale and modified component scaling were applied together, and only about -0.6 kW for the isolated component-scaling change. These deviations are negligible compared with the mean OpenFAST bias of approximately 200 kW, corresponding to at most about 1.5% of the bias. We also repeated the simulations using six turbulent realisations to quantify the realisation-to-realisation variability. The standard deviation across the six seeds was approximately 1 kW for the base case and the isolated component-scaling change, and increased to approximately 4 kW when the length scales were increased. Even this larger spread corresponds to only about 2% of the OpenFAST bias. We therefore conclude that the tested turbulence-length-scale and component-scaling choices have an insignificant influence on the results and do not affect the conclusions of the manuscript. We have clarified this in the revised text, while avoiding an extensive discussion of TurbSim parameter tuning because the main contribution of the paper is the comparison between the proposed analytical power-estimation framework and OpenFAST as the numerical benchmark.

18. **Line 370:** it is unclear to me how the standard deviation of the wind speed are estimated in the paper. Was is based on e.g. the approach by Eberhard et al. (1989), further applied for nacelle lidar in Fu et al. (2022)?

L142-146, L223-229: We have revised this subsection and clarify that we take the standard deviation based on the WindCube LiDAR manual which states that the standard deviations are estimated by the “wind speed dispersion” parameter.

19. **Figure 8:** Figure 8 should include quantitative error metrics, such as bias, RMSE, and possibly R2, if computed from the original unbinned data. If the figure presents a single comparison, subcaptions are unnecessary; a single main caption should be sufficient. These numbers would complement Table 2.

Sec 3.7, around Tab. 2 and Fig. 8, comparison in Sec 4.2: We thank the editor for this suggestion. We agree that quantitative error metrics are important for interpreting this comparison. These metrics are already reported in Table 2, where the bias, RMSE, MAE, and R2 values are computed from the original unbinned 10-minute data. Figure 8 shows the same comparison visually using binned curves for readability, while Table 2 provides the corresponding quantitative evaluation. We therefore decided not to duplicate the same values inside Figure 8, because this would make the figure more crowded without adding new information. To avoid ambiguity, we have clarified in the manuscript that the statistical metrics are computed from the original unbinned data and are reported in Table 2, whereas the binned curves in Figure 8 are used only for visualisation.

20. **Page 19:** Tursbim simulations: The IEC standards typically requires 6x10 min aeroelastic simulations for normal wind turbine operations. The same may be needed for wind power production. Using multiple seeds allows reducing random errors through ensemble averaging. It may be a good idea to use also six different realisations.

L540-561 and L615-619: We thank the editor for this suggestion. We agree that multiple turbulent realisations are commonly used in aeroelastic simulations to reduce random errors associated with the stochastic turbulence field. In the original setup, each OpenFAST case was run for 12 min, with the first 2 min discarded as spin-up and the final 10 min used for the mean-power comparison. During initial testing, we found that changing the TurbSim random seed had little influence on the 10-minute mean power. Nevertheless, following the editor's recommendation, we have now repeated a representative subset of the turbulent OpenFAST simulations using six different random seeds.

We did not rerun every OpenFAST inflow case with six seeds, because the purpose of this additional analysis was to quantify the magnitude of the realisation-to-realisation uncertainty rather than to turn the full OpenFAST comparison into a turbulence-ensemble study. We therefore selected the representative case where the largest deviations in the velocity spectra were observed, corresponding to wind profiles around 9 m s−1. This provides a conservative test of whether the stochastic turbulence realisation affects the conclusions.

The resulting variability is small compared with the systematic biases discussed in the manuscript. The standard deviation of the 10-minute mean power across the six seeds is approximately 1 kW for the base configuration and for the isolated component-scaling modification, and increases to approximately 4 kW when the length-scale profile is

increased by a factor of ten. Even this larger stochastic spread corresponds to only about 2% of the mean OpenFAST bias of approximately 200 kW. The ensemble-mean differences between the tested turbulence configurations remain only about 0.2–1.3 kW. We therefore conclude that the OpenFAST results are not controlled by the choice of random seed, and that a full six-seed ensemble for every inflow case would substantially increase the computational cost without changing the conclusions of the study. We have clarified this point in the revised manuscript.

21. **Line 378:** The scaling "(1.092, 1.000, 0.534)." is somewhat arbitrary and using the along-wind component as reference would requires the first scaling parameter to be equal to 1 so that target and simulated turbulence intensities are identical. I would recommend the following scaling (1.0, 0.8, 0.6), which is consistent with Description of turbulence onshore (See a list of possible coefficient, in table 3 in Solari and Piccardo, 2001)

Sec 3.5 L256-263, Sec 4.4 L540-570, App. B L770-780: We thank the editor for pointing this out. We agree that the original scaling (1.092,1.000,0.534) was insufficiently justified and could be interpreted as arbitrary. We have therefore revised the description of the scaling used in the TurbSim/USRVKM setup.

Rather than directly adopting a generic onshore scaling such as (1.0,0.8,0.6), we estimated the component scaling from the same WindCube data used to assess the integral length scales. For each selected OpenFAST inflow period and lidar height, effective component-wise integral length scales ($L_u(z)$), ($L_v(z)$), and ($L_w(z)$) were estimated from the autocorrelation functions of the beam-derived velocity components. The scaling was then defined as ($1, L_v/L_u, L_w/L_u$), and a single scaling vector was obtained by taking the median ratio over height. This gave (1.00, 1.04, 1.20), with uncertainty margins of approximately (0.00, 0.10, 0.26) based on the spread of the height-wise ratios.

These values differ from the generic onshore recommendation, but this is not unexpected. The lidar-derived values are effective component length-scale ratios obtained from sequential scanning lidar measurements in complex alpine terrain during selected inflow windows, rather than universal turbulence anisotropy constants. They should therefore not be interpreted as a replacement for standard onshore turbulence coefficients, but as a site- and period-specific diagnostic used to test the sensitivity of the OpenFAST comparison. Given the large variability in the estimated length scales, we do not regard any single scaling vector as uniquely correct. We therefore use the lidar-derived scaling as part of the sensitivity analysis and clarify that the OpenFAST results are not intended to provide a definitive turbulence-parameterisation study. This approach directly addresses the concern that the original scaling was arbitrary, while avoiding the equally arbitrary replacement of the site-specific measurements by a generic coefficient set.

We also explicitly tested the generic component scaling (1.0, 0.8, 0.6) suggested by the editor as an additional sensitivity case. This was done both in isolation and in combination with the increased length-scale profile used in the length-scale sensitivity test. The influence on the OpenFAST mean power was negligible: for the case with the largest spectral deviations, the isolated change to (1.0, 0.8, 0.6) changed the mean power by only about -0.6 kW, while the combined length-scale and component-scaling modification changed the mean power by approximately 2 kW. These differences are very small compared with the mean OpenFAST bias of approximately 200 kW. We therefore agree that (1.0, 0.8, 0.6) is a useful generic reference scaling, but our results show that replacing the lidar-derived component scaling by this generic set does not materially affect the OpenFAST power comparison or the conclusions of the manuscript.

22. **Table 2:** The bias may be rounded to the nearest integer for clarity.

Tab. 2: We have rounded the biases in the 10x4 part of the table, but keep the RMSE MAE and R^2 at their current decimal precision as they are still significant.

23. **Results section:** When presenting results, consistent digits should be used. For example, line 527, “ 0.86 % to 14.1 %” should be “ 0.9 % to 14.1 %”

Throughout the manuscript: We have rounded the subsequent value.