Reviewer #1

Summary:

The paper presents a new analytical model accounting for the vertical variations in horizontal wind speed to assess wind turbine power production. The approach relies on the Taylor expansion of wind profile measurements in the rotor swept area obtained from Doppler lidar observations. Results from the analytical model are validated against OpenFAST simulations and measurements from wind turbines operating in the Gotthard Pass wind farm in the Swiss Alps. The topic of the manuscript is of interest since this method provides an estimate of turbine power production at a relatively low computational cost compared with other approaches. Moreover, the detailed knowledge of the turbine mechanical characteristics is not required, and the wake effects between turbines can be addressed by this new method. However, the manuscript lacks clarity, organisation, and rigour, which hinder its readability and reproducibility. More work is needed to improve the manuscript by addressing the following comments:

We sincerely thank the reviewer for this thorough, constructive, and thoughtfully written review. We greatly appreciate the positive and encouraging tone throughout, as well as the clear and detailed feedback provided. The comments are insightful and have helped us to better structure and clarify the manuscript, improving both its readability and scientific rigor. We are grateful for the time and care invested in this outstanding review and have addressed all points raised below:

Major comments:

1. Key explanations are missing or not clearly formulated. While the Taylor series decomposition of the wind profile is well detailed, the complete methodology is not fully described. In particular, the extension of this decomposition to the wind variance profile is not explicitly explained. Furthermore, it remains unclear how this variance decomposition is used to predict turbine power output and how the power curves are obtained. All these points should be addressed in the revised manuscript.

   Since this comment overlaps significantly with the suggestions made in Major comment 3, we refer the reviewer to this comment for a detailed summary of changes that have been made.

2. Several points raised by the editor were not addressed in the manuscript. In particular, the suggested revision to the opening sentence of the abstract was not applied. Furthermore, while a few references were added, the proportion of self-

citations remains high, and the paper still does not introduce or discuss previous studies focusing on alternative rotor equivalent wind speed formulations.

This initial editor comment has now been resolved. We believe this was confused with sentence 1 of the introduction before. We apologize for this oversight.

Regarding the second point, we acknowledge the suggestion to broaden the discussion of alternative rotor-equivalent wind speed formulations. In response, we will substantially revise the introduction to better incorporate and discuss relevant studies in this area.

At the same time, we would like to clarify that we do not consider the proportion of self-citations to be excessive. The cited works primarily reflect the limited body of literature directly related to this specific research topic and are included where they are most relevant to the methodology and context of the present study. Nevertheless, we will carefully review the references to ensure a balanced representation of the existing literature.

3. The current structure of the methodology section (section 2) is confusing. The analytical model is introduced (section 2.1) before the input data is presented. The OpenFAST configuration is split between sections 2.2 and 2.5, making it difficult to get an overall view of the numerical modelling approach. Moreover, the current section titles (e.g., "Analytical method", "Numerical method") are imprecise and should be more descriptive. Here is a suggested restructuring:

2.1 Wind profiles

2.1.1 Idealized wind profiles

2.1.2 Gotthard Pass wind profiles

2.2 Power prediction through analytical method

2.2.1 Rotor-averaged cubic wind using Taylor expansion of the wind profile (which corresponds to the current paragraph before section 2.1.1)

2.2.2 Turbulence modelling

2.2.3 Power curve fitting (current section 2.1.1)

2.2.4 Application to idealized and real wind profiles

2.3 Power prediction through numerical modelling (OpenFAST, current section 2.2 and a part of the 2.5)

2.4 Analytical model validation strategy (current section 2.5.1, to merge with the current introduction of the results and discussion part (section 3))

We thank the reviewer for this very thoughtful and constructive comment. We particularly appreciate the effort taken to propose a detailed restructuring of the methodology section. This was extremely helpful and served as a strong foundation for improving the clarity and flow of the manuscript.

We fully agree that the previous structure of the Methodology was confusing, especially regarding (i) the introduction of the analytical model before sufficient context was provided, and (ii) the fragmentation of the OpenFAST configuration across multiple sections. In response, we have substantially reorganized and rewritten the entire methodology section and parts of the results and discussion.

While we closely followed the reviewer's suggested structure, we made a few small deviations from the suggested structure. Specifically, we chose to retain the introduction of the analytical model at the beginning of the Methods section. Since this model represents the central contribution of the paper, we believe that introducing it upfront provides the reader with a clear conceptual framework before moving into input data and evaluation strategies. This ordering helps guide the reader through the manuscript in a more logical and intuitive way, which additionally makes sense chronologically.

That said, we have implemented the reviewer's suggestions in the following ways:

1. Clear separation and consolidation of modelling approaches

The analytical model is now presented in a single, coherent section, where we systematically introduce all required components (including turbulence treatment, power curve fitting, and full formulation). All relevant equations and assumptions are now grouped together to provide a complete and self-contained description.

2. Reorganization of the numerical modelling (OpenFAST):

The OpenFAST setup, which was previously split across sections, has now been fully consolidated into the second section of the methodology. This provides a clear and comprehensive overview of the numerical modelling approach and facilitates direct comparison with the analytical model.

3. Improved methodological flow (models → evaluation)

After introducing both modelling approaches, we now proceed to the evaluation methodology, distinguishing clearly between:

a. Idealized wind profile experiments
b. Real-world application (Gotthard Wind Park)

This restructuring ensures a more logical progression: first understanding the models, then how they are tested.

Relocation of previously misplaced content:

Several elements that were previously included in the Results and Discussion section have been moved to the Methods section where appropriate, as suggested by the reviewer. In particular:

a. The discussion of power curve characteristics/curvature is now included in the methodology
b. The Gotthard workflow and processing steps are now fully described in the Methods section rather than partially deferred to later sections
c. The validation approach is now integrated within the methodological description of both the idealized and real-case analyses, ensuring that the evaluation framework is clearly defined before presenting results.

Overall, we believe that this revised structure significantly improves readability by introducing the core analytical model first, presenting the state-of-the-art numerical benchmark (OpenFAST) in a unified manner, and clearly separating model development from evaluation strategies, which are then applied to both idealized and real-world cases.

Minor Comments:

General comments on the manuscript (please refer to detailed comments below for details):

- Units are written in italics throughout the manuscript, but should be written in normal font.

  We went through the entire manuscript and corrected the italics as such, this also included missing spaces between some units.

- Units are sometimes specified in brackets in the text (e.g., line 83) but omitted elsewhere (e.g., line 267). It should be consistent throughout the manuscript. I recommend removing the brackets as this improves readability.

  We decided that we will introduce each variable exactly once with units. If this is done in the description of an equation, it makes sense to include the square brackets as this is standard notation and reduced redundant words such as « which has the units … » or similar. After the units have been mentioned once, they will not be added again, with the exception of the following:

  1. When they are mentioned in the Abstract or Conclusion. Such that readers can scan through these sections without being confused by missing units
  2. When they are mentioned in Figure or Table captions.

3. When it regards % or °, which are relevant arbitrary scaling from [-] and [rad] which could otherwise be confused for their SI unit equivalent.
4. When refering to the Enercon E92 2.35 MW and the OpenFAST 3.0 MW turbines, as we generally use kW as units for power. These two conventions of using kW in equations and MW in large turbines could otherwise cause confusion.

- Some formatting issues create large blank spaces in the manuscript (e.g., pages 4, 6, 10, etc), which should be corrected.

  This formatting is conforming to WES guidelines where in pre-print manuscripts we must use \clearpage in front of every table and figure, hence the large gap. This should be fixed in typesetting, however.

- The reference to equations, figures, tables, sections, and appendices in the text does not follow the WES guidelines, as for the format of the tables.

  We have checked all mentions of eq., equation, fig., figure etc... and made sure they aren't abbreviated and capitalized as written in the WES guidelines. Table 2, is now not an image anymore.

- There are several punctuation and capitalization issues throughout the manuscript. For example, there are unnecessary commas (e.g., line 216), incorrect capitalization where it should not appear (e.g., caption of Fig. 2), and missing capitalization where it should be used (e.g., line 651).

  We corrected the specific cases described here and performed another punctuation/capitalization check after all comments were implemented.

- Some words extend beyond the page margins on several lines and should be corrected.

  Both cases have been resolved.

- Some parts of the methodology appear in the results section, and conversely, some results are presented in the methodology section.

  These issues have now been addressed, including the suggestions in the Detailed comments below.

Detailed comments:

1. Line 84-85: "ρ [kg m−3] is the air density which is assumed to be constant throughout the swept area of the blade". The specific value of ρ should be specified here, or a reference to Table 2 should be made.

   Value (0.996 kg m-3) is now added.

2. Line 94: the sentence “we integrate over the rotor-swept area…” should specify which quantity is being integrated. This holds for the caption of Fig. 1, which should be detailed.

   «we integrate over the rotor-swept area along the z-axis by dz», Figure 1 has been corrected to reflect dz instead of dh.

3. Figure 1: a unique notation should be used for the vertical coordinate (z, h, or Z) and should remain the same in the following equations.

   This is corrected and now aligns with the previous comment

4. Equation 3: Variable N is not defined in the text. It can be specified in the sentence above by adding “at the order N”.

   one can expand the wind profile into its Taylor series of order N

5. Line 104: “The full derivation is given in A…”. Here, the authors refer to the appendix, but A corresponds to the rotor swept area.

   Changed to « Appendix A »

6. Line 106: “expansion coefficients i, j & k.” Please replace “&” with “and”. Same remark for line 113.

   We changed two ampersands (&) with and. This now replaces all ampersands in the manuscript.

7. Table 1: this table is present two times in the paper (in the main text and in the appendix). It should be removed from the main text.

   Removed from main text as suggested

8. Line 114: “recommended at N = 3”. Is there a reason for this specific value?

   «recommended at $N=3$ providing a good trade-off between capturing essential behaviour and avoiding overfitting»

9. Line 125: “. It has been shown that a linear scaling factor of 1.81 applied to σ_v effectively accounts for the heavier tails of the Student-t distribution in the case of the Gotthard Transect Experiment for all wind directions and speeds.” A reference is missing for this statement.

   Reference now included.

10. Equation 6: some terms are not explained. Please detail what v_{in}, v_{out} and P(v) are here. Same remark for Eq. 7, where Pr is not defined. In this equation it is not clear whether v refers to v or v . Moreover, the P(v) in Eq. 6 seems to be different from the one in Eq. 4. If this is the case, another notation should be used.

Changed notation of P(v) to P_pc(v) to reflect the « power-curve » of the wind turbine. P_r is now explained as the rated power. Vin and vout are described for both eq.6 in detail and re-referenced briefly in eq. 7 as it is specifically mentioned there. With the change to P_pc(v) , there is no more confusion with Eq. 4.

11. Line 135: "showing good agreement with the analytical solution (Fig. 6b, further detailed in the Results)." This sentence should be in the results section.

    This sentence is removed here. In the results section this has already been described clearly.

12. Line 178: "When comparing to the measured power output of the turbine (Ptrue), setting the shaft tilt to zero introduces a small bias of approximately 8kW for the 2.35MW turbine." Here, the discussion refers to a turbine model that is not yet introduced in the paper.

    We have removed this from the methodology, which now only states that we change the shaft tilt to 0° instead of the default 5°.

    We moved this to the Results and Discussion section, sub-section Robustness and Sensitivity Tests : « a small bias of approximately 8 kW for the 2.35 MW turbine. However, since the wind profile is based on a LiDAR measurement located roughly 100m from the turbine, any slight decrease in horizontal inflow or the inclusion of vertical wind components caused by shaft tilt would not significantly improve the accuracy. Therefore, we chose to retain the simpler configuration which are comparable to the idealized wind profiles. »

13. Table 2: how was this specific density value selected?

    Note that this is now Table 1. (see specific comment 7)

    As described in specific comment 1, we have now added this explanation around the introduction of \rho in eq. 1.

    « \rho [kg m-3] is the air density which is assumed to be constant throughout the swept area of the blade. The air density is set at \rho=0.996 kg m-3 based on the average air density that was measured during the campaign described in van Schaik, 2025a.»

14. Line 208: "constant and uniform wind profile with no turbulence, which is detailed in section 3.2." The reference case is actually defined in the same section, from lines 212 to 215.

    We removed this sentence, since the reference case is described in the next paragraph, no new reference is made.

15. Line 211: What is [a)]?

    This was a LaTeX package issue, it was meant to introduce the enumeration with a), b) instead of 1., 2.. This is fixed now. Apologies for this oversight.

16. Line 219: "and z = 0m at the nacelle of the turbine". This sentence should be moved and included in the description of Fig. 1.

    We have now added this description around Fig. 1. We chose to keep this description in this subsection as well, as it helps understanding eq. 9 more easily.

17. Line 232: please correct "Von Kárman" to "von Kármán". On the same line, how did the authors select the value of z0 = 0.1 m?

    Name corrected.

    Secondly, « and z_0 = 0.1 [m] is the surface roughness, which was chosen based on logarithmic fits of the wind profiles measured by the LiDAR on the Gotthard pass.

18. Line 239: "indicated orange in Figure 3a". There is no a) or b) on this figure.

    Apologies for this oversight. a) and b) are now included in Fig. 3.

19. Line 242: "In 2023, the site hosted a Doppler wind LiDAR campaign in which the instrument was deployed at 10 locations near the turbines over a three-month period (Figure 3)." The lidar model and the dates of the field campaign should be briefly indicated.

    «In June to September 2023, the site hosted a Doppler wind LiDAR (VAISALA WindCube v2.1)…»

20. Line 295: "we have selected a four hour window of 10-minute data". It is not clear whether the analysis is based on the mean profile over this period or on the 24 individual profiles available.

    New sentence to clarify the methods: « For each case, we selected a four-hour window of 10-minute data, yielding 25 LiDAR-derived wind profiles from the LiDAR location closest to the turbine, along with the corresponding turbine power measurements at each time step. An exception is LiDAR location 10 under northerly wind conditions, where only 6 profiles were available due to limited data.»

21. Line 324: "For the analytical model, we align with the Enercon E92 2.35MW wind turbines installed at the Gotthard Wind Park. Power curve data is taken from the Enercon E92 specification sheet and is fitted (Equation 7), resulting in a smooth

analytical expression (Fig. 5a)." These sentences, except the reference to Fig. 5a, describe the methodology and should be moved to the methodology section.

We now refer to Subsection Gotthard wind data: analytical and numerical workflow and continue directly to Fig. 5a.

22. Line 330: The term "power curve" is incorrectly written as "powercurve" several times in the text.

Corrected all terms.

23. Figure 5: the text indicates that a scaling between the Enercon E92 and the WindPACT turbines is applied to allow comparison of the results. This figure should therefore be plotted using this scaling. Moreover, including additional points on the curve in Fig. 5b (for example, between 9 and 14 m s^{-1}) would result in a smoother curve. The wind speed unit should be written as ms-1 instead of m/s; this should be corrected in the x-axis label.

We have ensured that all figures follow this same styling that aligns with the WES standards.

24. Line 359: "The OpenFAST model consistently increases with shear…". I think the word "power" is missing here.

Indeed this was missing, now corrected to: «The power estimated by the OpenFAST model consistently increases with shear…»

25. Line 376: the description here refers to Fig. 6c and not Fig. 6b.

Thank you for highlighting this issue. Fixed now.

26. Line 376-377: "a characteristic shape which has previously been reported in similar works such as Gasser et al. (2025)". This reference does not seem relevant here, as no curve (or result) similar to the one presented in Fig. 6c is presented in Gasser et al. (2025).

While Gasser et al. Does not exactly plot this curve, it does describe the relationship between the second derivative and the power production related to turbulence as such.We refer the reviewer to section 4.3 of Gasser et al, for this exact description. We change the sentence accordingly:

« has a characteristic shape which has previously been described to follow this relationship between power curve curvature and power contributions reltated to turbulence in similar works such as Gasser et al. 2025.»

27. Line 391: brackets are missing around the references.

Fixed!

28. Line 392: the authors likely mean Fig. 6d.

Indeed, apologies again for this oversight. Fixed now.

29. Line 395: “from the uniform inflow amounting to only about 15 kW.” This value is right for the analytical model, but the deviation reaches 20 kW in the case of the OpenFAST simulations.^

« A noticeable observation is the relatively low impact of the logarithmic profile, with the maximum deviation from the uniform inflow amounting to only about 15 kW and 20 kW for the Analytical and OpenFAST models, respectively »

30. Figure 6: The same remark as above applies here for the x-label unit. As explained before in the text, different values of a, max, and TI are considered, however, the impact of these different values is not discussed. The meaning of these parameters should also be briefly indicated in the caption.

(Figure 6 has become figure 7 in the new manuscript)

The figure is corrected as suggested.

Regarding the different values of a, alpha_max and TI, we add a few sentences in the subsection, relevant to the discussion of each subfigure.

For 6a : « With increasing shear coefficient a, we see similar increases in magnitude of \Delta P for both models, and the peak values scale as expected with \propto a^2 as described in Equation 9. »

For 6b : « As we vary the maximum veer angle \alpha_{\max}, the analytical model follows the expected square dependence (\Delta P \propto \alpha_{max}^2) as described in Equation 10. The OpenFAST simulations, however, show slightly different magnitudes, with relatively larger power losses at smaller angles (\alpha_{\max} = 5°) and smaller losses at larger angles (\alpha_{max} = 22.5)° This deviation is most likely due to the more detailed modelling of airflow over the blades, but it remains of secondary importance compared to the dominant quadratic dependence on \alpha_{max} in determining the overall turbine power output. »

For 6c : « As we vary the turbulence intensity (TI), the magnitude of the turbulence-induced power variations increases in proportion to the second derivative of the power curve, as expected for both the analytical and OpenFAST models. A slight exception occurs for the OpenFAST model near the peak positive contribution to wind power around v = 9-10 m s-1. With increasing TI, this peak shifts toward slightly lower wind speeds. This behaviour is likely due to the more detailed modelling of the flow over the turbine blades in OpenFAST and represents a secondary effect.»

31. Line 413-414: “Compared to the turbulence-free case (Fig. 7b), this model version achieves much better agreement with the measured power production, particularly at high wind speeds.” This conclusion is not obvious from the figure alone. Although there is indeed a slight improvement when turbulence is included in the model, this only becomes clear when considering the bias values reported in Table 3, since the order of magnitude of the biases is much smaller than the scale of the figure.

    Note that Table 3 has changed to Table 2 due to previous detailed comment.

    We have softened this statement around the Figure 7b description and refer to the Table bias values as suggested.

32. Figure 7: the caption does not describe what is shown in the figure. Given that the inclusion of turbulence in the model has a limited impact, it would be interesting to present the power curves obtained with the analytical model and/or OpenFAST in comparison with the one derived from the standard approach. This would confirm the statement in lines 432-433.

    (Please note that this has become Figure 8 in the new manuscript)

    We have changed the caption to reflect the contents of the figure better: “The differences in power production from the Analytical and OpenFAST models, and compared to standard approaches. (a) Comparison of the two models including turbulence and (b) excluding turbulence. (c) Standard nacelle power estimates from turbine anemometer and LiDAR and (d) Model cross-correlations.”

    Furthermore, we have considered including a plot essentially combining (a) and (b) so that the improvement of the turbulence can be more easily observed (as written in l. 432-433 in the pre-print), however we have decided not to include this as it would constitute an additional figure (it would become too congested in the original figure), and this additional figure would not include original information for the manuscript. In fact the following table on the next page (Table 2), includes this information in a different form: The Bias from measured power [kW] columns for “Analytical” and “Analytical No Turbulence” show this exact improvement already.

33. Line 423: “the third subfigure (Fig. 7)” corresponds to Fig. 7c.

    (Fig 7 became 8 in new manuscript) Included 7c instead of 7.

34. Table 3: it seems that this table is included in the manuscript as an image. If this is the case, this should be modified, and the table format should follow the WES guidelines.

    We have changed the table from an image to a regular table to adhere to WES guidelines.

35. Line 434-435: it is not clear how the values of 8.6% and 25.4% were obtained. They should therefore be verified.

    Note that Table 3 has changed to Table 2 due to previous detailed comment 34.

    We now describe this derivation in detail in this paragraph:

    «As shown in Table 2, the wind power bias reduction per site relative to the IEC standard ranges from 8.6% to 46.4%, depending on site complexity, with a mean bias reduction of 25.4 \pm 13.0%. The bias reduction is computed as the relative difference between the analytical approach and the IEC standard, defined as (Analytical - IEC) / IEC, using the corresponding bias values from the first and last column of the Bias section of Table 2.»

36. Line 454: "In Figure 8a, the original LiDAR wind measurements are shown in orange". One could understand that the authors refer to the measurements from turbine 1 under southerly winds, but this should be stated in the text.

    We agree that this was left unclear. We change both the figure label of Fig 8.

    «Wake filtering of LiDAR location 9 with Turbine 1 under Southerly winds (a) wind profile and (b) power production comparison. »

    As well as the text:

    «In Figure 8a, the original LiDAR wind measurements from LiDAR location 9 for southerly wind conditions, relative to Turbine 1, are shown in orange, together with a subset of points selected outside the wake-contaminated region. »

37. Line 457: "the wake from Turbine 2 intersects the LiDAR beam but consistently bypasses Turbine 1". Please clarify how this was established.

    « Specifically, for Turbine 1 and LiDAR location 9 under southerly wind conditions, the wake from Turbine 2 intersects the LiDAR beam but consistently bypasses Turbine 1. This becomes evident when considering the turbine position angle of Turbine 2: extending a line downstream on the lee side of the turbine, while accounting for the swept area of the rotor, shows that the wake passes over LiDAR location 9 but does not intersect Turbine 1. »

38. Line 490: the link between the discussion in this paragraph and the analytical model is not clear. This should be better explained in a revised version of the manuscript.

    We aim to clarify that, although our analysis focuses on relatively small deviations in wind power production compared to the IEC standard, it is important to recognize that the common assumption of using 10-minute averaged wind profiles is not exact. While this standard is widely adopted in wind energy applications, it inherently smooths short-term variability that can influence power estimates. Our sensitivity test shows that using different averaging intervals slightly alters the wind profiles and leads to changes in analytically derived power on the order of 4%. Although this effect is modest and does not warrant further detailed investigation within the scope of this study, it highlights a potential avenue for refinement. However, since turbine power data are typically only available at 10-minute resolution, a more detailed assessment is currently not feasible.

    «Finally, we assessed the impact of temporal averaging on estimates of \overline{v^3} by aggregating the 1 s LiDAR wind-speed data over a range of averaging intervals (1, 2, 5, 10, 20, and 30 minutes, and 1 hour). Changing the averaging window slightly modifies the resulting wind profiles, which in turn affects the analytically derived power production. We find that these differences remain relatively small, on the order of 4%. While this effect is not explored further, it is worth noting as a minor source of uncertainty. In line with standard wind-energy practice, 10-minute averages are adopted throughout this study.

    This choice is consistent with the processing of both LiDAR measurements and turbine data used in this work, ensuring comparability between inflow characterization and power estimates. It also aligns with the temporal resolution at which the analytical model is applied, such that any averaging-induced bias remains small relative to the overall variability in wind conditions across the study sites.»

39. Line 510: “such as shear, veer”. Please specify linear shear and linear veer.

    Adjusted as suggested.

40. Equation A4: the lower limit of the summation is missing. The same applies to the remaining summations in the appendix.

    Lower limited included in all summations (…=0)

41. Equation A5: this equation is basically the same as Eq. A3 and should not be repeated.

    Removed this equation and the subsequent line above.

42. Equation A6: standard functions as sin, cos, and arcsin should be written in normal font rather than italics. The same holds for the differential in the integrals.

    We have changed all sin, cos, arcsin and differential d in the integrals to normal fonts in both the main text as the appendix.

43. Equation A11: it should be specified that the function Q depends on the indices i, j, and k. Qm can then be used in the following equations.

    Thank you for this excellent suggestion ! We have implemented the notation as suggested.

44. Line 580-585: the demonstration can be simplified by not splitting the integral into two parts, as it can be directly evaluated using the Beta function with x=s+1/2 and y=3/2.

    We applaud the reviewer for finding this elegant shortcut. We implement it as suggested.

45. Line 591: it should be specified that the second term is included.

    As suggested before with the Q_m notation, this has been addressed.

46. Line 596: another variable should be used since z already corresponds to the vertical coordinate in the main text.

    We replace z by x as dummy variable, which hasn't been used otherwise.

47. Equations A25 to A27: these equations should not be detailed here since the formula given in Eq. A28 is well known.

    Removed as suggested.

48. Line 634: I recommend introducing a notation for this fraction (e.g., F(m)) and defining it explicitly, since it is currently unclear whether the factor 1/2 is included or not.

    We choose to define it as K_m, and define it in the Appendix. We believe this is also beneficial to separate in the main text, which has also been changed accordingly.

49. Line 649: a detailed calculation of the integral should be provided to confirm that statement. The same applies to section A3 of the appendix.

    We added a reference back to the original integral from Eq. 2 and wrote out the cubed wind speed term for both A2 and A3, and explain how all subsequent terms remain or drop out to result in the final equation.

50. Equation A9: since the term O(h4) is only used once in the manuscript, I suggest removing it from the equation, replacing "=" with "≈" and specifying "Taylor expansion at the order 2" in the sentence above. Moreover, the choice for the vertical coordinate notation should be consistent in the manuscript (h or z).

We removed this O(h4) notation as suggested and replaced with \approx and expansion to second order.

We have corrected the mistake of h^4 to z^4.

Reviewer #2

Paper presents a new method for estimating wind measurements, improving on single point measurements, for use in power curve assessments. The paper is well written and provides clear explanation of methods and experimental setup. The proposed methods are demonstrated in a clear way in studies using collected field data. All in all a well-written paper and a good contribution.

We thank the reviewer for the constructive feedback that has helped to improve the structure of the manuscript.

General comments:

Good that the paper makes frequent comparison to the standard approach in EIC 61400. Is there an intention to submit this work for consideration to a future standard?

Thank you for this suggestion, we have not yet considered to submit this to the future standard, but will get in contact with authors of this standard to inquire about the possibilities.

Specific comments:

Page 2 Line 50: "These discrepancies scale to wind-farm level and can lead to significant instantaneous and cumulative power prediction errors" -- This depends a little if the error per-turbine is biased in one direction, it could also cancel out.

Good point, we have rephrased this to reflect the random nature of this error:

"These discrepancies can propagate to the wind-farm scale and may lead to significant instantaneous and cumulative power prediction errors, although their aggregate effect depends on how errors vary across turbines."

Page 3 Line 65: "developed by NREL" (could note now NLR per name change)

Since all sources still refer to the laboratory as NREL, we have decided to keep NREL as the reference in all cases except the first mention in the introduction:

"...an open-source wind turbine model developed by National Laboratory of the Rockies (NLR), formally known as National Renewable Energy Laboratory (NREL)..."

We briefly mention the difference as well in first mention in the Methodology.

Page 3 Line 69: "2106m a.m.s.l." please define

Defined as requested. There is a second use of a.m.s.l., where we've now removed the definition as it is defined before.

Page 3 line 70: "while modelling a virtual wind turbine with open-source blade geometry", do you mean modeling the Enercon E92s as this turbine, or seperately modeling this turbine?

This paragraph has been rewritten on request of the other reviewer. We believe that this ambiguity has been addressed accordingly with the following sentence:

“SCADA data from five Enercon E92 (2.35 MW) turbines are used alongside simulations of a comparable turbine with open-source blade geometry.”

Page 4: "where horizontal variations across the rotor disk are usually minor compared to the strong vertical gradient in wind speed." -- perhaps unless wakes are present?

“This dominance of vertical variations is well established in boundary-layer meteorology (Stull, 1988) and wind energy practice (Wagner, 2014, IEC 2022), where horizontal variations across the rotor disk are usually minor compared to the strong vertical gradient in wind speed, although this assumption may not hold in the presence of turbine wakes, which are not considered here.

Page 7: "given arrays of measured wind speeds v and heights z, t", are the wind speeds v instantaneous or averaged measurements?

We refer to mean wind speeds over 10 min intervals as standardized in wind energy engineering:

“...a) given arrays of measured 10 min mean wind speeds v...”

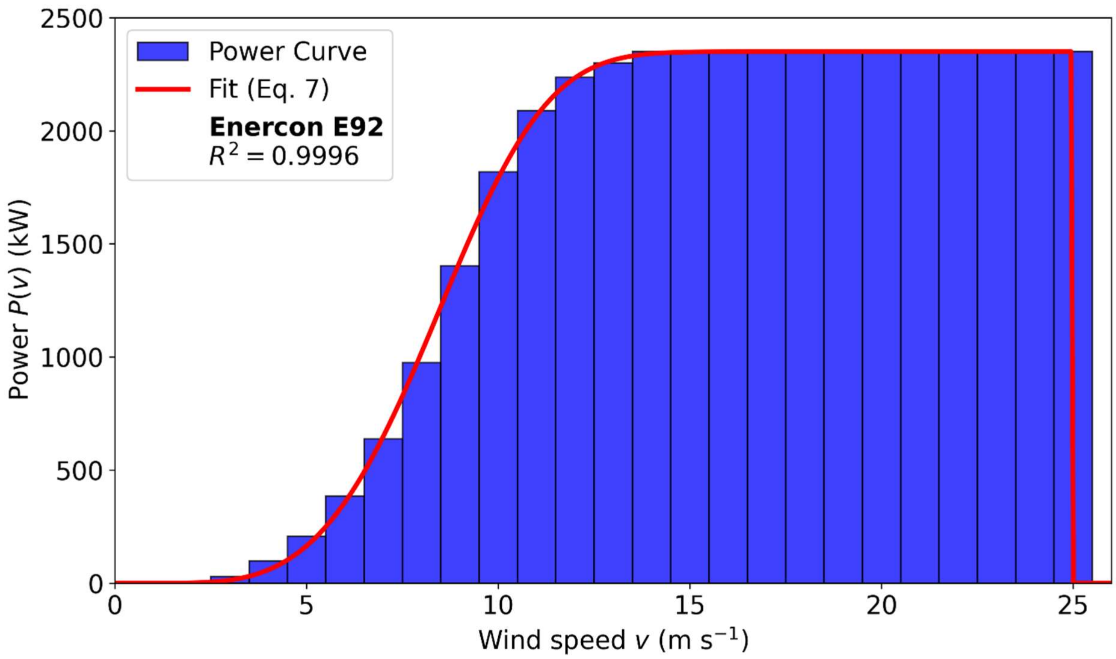
Page 7: "We refer the reader to the Data Availability section of this manuscript for more details." Thank you for this contribution to open-source and data availability.

We are glad to be able to contribute!

Page 8: Equation 7, I think a figure comparing the data-based, or binned power curve against the functional approximation would be helpful to see. Does the equation have a range of wind speeds for which it is valid or does it work cut-in to cut-out?

Thank you for this useful suggestion. We realize that we have not mentioned the complete piecewise definition of this equation, and changed that accordingly. This now includes that P = 0 for v < v_in and v > v_out. We have also included a new figure including the Enercon E92 binned power curve and the subsequent fit of our function which shows the goodness of fit (R^2 = 0.9996), as well as the fit parameters used in for this wind turbine. This is now Figure 2 in the manuscript.

We copy the figure here for your convenience:



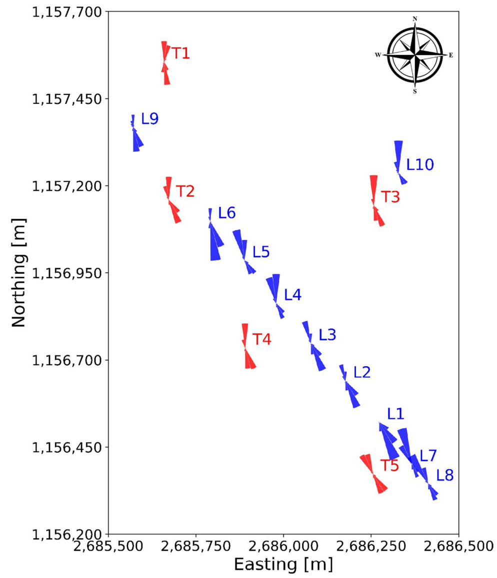
Page 8: "it naturally enables the removal of anomalous or clearly unphysical measurements before constructing the analytical profile", a more standard approach might be to only include data from wind sectors where the turbine is not waked right?

This is true, however the nature of the Gotthard wind park does not allow us to do that. While we don't show this detailed overview of the winds in this manuscript, we do cite our previous dataset paper (https://open-research-europe.ec.europa.eu/articles/5-9), where in Figure 8 (copied here for your convenience), one can see that the winds are strongly channeled along the pass. It is for this reason that we split the winds in "Northerly" and "Southerly".

To answer your question in the manuscript concretely, we will rephrase as follows:

"Another useful feature of the Taylor-expanded wind profile is that it naturally enables the removal of anomalous or clearly unphysical measurements before constructing the analytical profile. This enables wake filtering even when data from adjacent, wake-free wind sectors are not available, as is often the case in strongly channelled flow environments such as the Gotthard wind park."

Figure 8. Spatial progression of wind direction over the Gotthard wind park, illustrated by wind roses...



van Schaik B, Reynolds D, Huwald H and Lehning M. Resolving three-dimensional wind velocity fields in complex terrain using sequential wind-Doppler LiDAR, CFD and wind turbine

Page 11: "resulting in 25 simulations per power curve." how many power curves in total? A standard approach might include 4-6 random seeds per wind speed but that could be unnecessary here and I'm not saying you should do it. Just noting.

This method is used to generate the standard power curves and the idealized cases. The methodology section has been rearranged based on suggestions from the other reviewer. This has been also been addressed in the process. We only include a single simulation per wind speed bin, Based on our tests, OpenFAST stabilizes its wind power estimate after 2 minutes, after which we average over 10 minutes which allows us to say with significant accuracy that the produced power value is representative of the power curve value for any given wind speed.

Page 21: Line 387: , "so the controller cannot hold the blades exactly at the optimum." Since this is above rated operation, the controller would not be trying to keep the blade at optimum but instead be pitching toward feather to keep the turbine from exceeding rated power. If the torque controller is a simplified constant power (versus constant torque) operation at and above rated speed, then the explanation is more probably that the variations induce rotor speed excursions above and below the rated speed, but the power is clipped above but still falls below, leading to a downward bias.

Still I'm a little surprised these above rated power deviations can be on par with the deviations happening near rated. If you take a look at a standard power curve adjustment for TI you often see the pattern you see with an increase in power away from rated, a decrease near and slightly above rated (like a rounding of a sharp corner) and then minimal effect above rated. There could also be a bug in the controller.

We thank the reviewer for this excellent analysis of this downward bias. We agree that the suggested explanation is more probable and adopt it in the manuscript as follows:

"... The controller likely simulates the rotor speed varying above and below the rated speed, where the power is clipped to a maximum on the positive variations, but not on the negative ones. The result is a small shortfall from the rated power set point that increases with turbulence intensity."

page 25: "OpenFAST generates power earlier because it lacks a defined cut-in wind speed – a known unrealistic feature of the software", this could be defined in the controller. But it's not a major point worth the effort to fix.

Thank you for information! We were unable to find this option in the controller. We have performed some rough estimates related to this error that is included due to the missing cut-in wind speed, but the contributions are negligible as the power that is produced in these extremely low wind speeds is minimal. In our tests of the Gotthard wind park, we never have wind speeds in this range (as the results would be trivial with the turbines generating 0 kW) so it should not effect the results in any measurable way. We address this in the manuscript as follows:

"…. This should however not affect the results in a measurable way as the selected cases do not include the trivial case where $v < v_{in}$ where $P = 0$ kW."