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.

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.

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.

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

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`.

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.

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.

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

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

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.

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.

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.

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.

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.

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.

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.

On Turbsim and USRVKM

The spectral model USRVKM should be explicitly defined as the von Karman model with user-defined 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 re-generating 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-on-the-calculation-of-the-length-scales-of-turbulence)

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)?

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.

Page 19: Tursbim simulations: The IEC standards typically requries 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.

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)

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

Resutls 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 %”

References

Eberhard, W. L., Cupp, R. E., & Healy, K. R. (1989). Doppler lidar measurement of profiles of turbulence and momentum flux. *Journal of Atmospheric and Oceanic Technology*, *6*(5), 809-819.

Fu, W., Sebastiani, A., Peña, A., & Mann, J. (2022). Dependence of turbulence estimations on nacelle-lidar scanning strategies. *Wind Energy Science Discussions*, *2022*, 1-21.

Solari, G., & Piccardo, G. (2001). Probabilistic 3-D turbulence modeling for gust buffeting of structures. *Probabilistic Engineering Mechanics*, *16*(1), 73-86.