Reviewer

General comments

We are pleased to see that the remarks we made about the organization of the manuscript and the necessary explanations of all the aspects of the method have now been addressed. Most of the comments on this version are minor ones. The scientific points that still deserve an answer concern the computation of the turbulence integral length scales and the comments about these results (see detailed comments on Sec. 4.4 and App. B).

We thank the reviewer for the positive assessment and for the remaining detailed comments. We have addressed the minor comments throughout the manuscript. Following the editor's recommendation, we have also removed the discussion and analysis of the turbulent integral length scales from Section 4.4 and Appendix B and simplified the OpenFAST turbulence comparison.

Detailed comments

Section 1. Introduction

1. Lines 98-100. "The contribution of the study…": in this sentence, it would be better to remain more general, i.e. not mention OpenFAST and the Gothard Pass site, that have not yet been introduced, but rather numerical simulations and a mountainous site. The transition to the next sentence would remain natural.
   We have made the description of the contribution more general by replacing the references to OpenFAST and the Gotthard Pass with numerical simulations and a mountainous site.

2. Lines 101-104 and 105-107. As already said in the previous review, one-sentence paragraphs should be avoided.
   We have merged the two one-sentence paragraphs.

3. Lines 112-114. "The results then compare…": a reference to Sec. 4 should be added in this sentence.
   We have added a reference to the Results and discussion section.

Section 2. Background and theoretical framework

4. In our opinion, it would be more logical to present the two paragraphs of Sec. 2.2 in the reverse order. Also, subsections 2.1 and 2.2 could be merged: it is possible to introduce the rotor power very short subsections should be avoided).

We have merged the two subsections and reordered the paragraphs as suggested.

5. Line 147. It is incorrect to say that "the instantaneous wind speed at height z" is given by Eq. 3. Actually, it gives the probability density function that models the distribution of instantaneous wind speed values at height z.
   We agree that Equation 3 describes the probability density function rather than the instantaneous wind speed itself. We have corrected the sentence accordingly.

6. Line 182. Since this is the first time the authors mention an appendix in the text, it should be Appendix A instead of B, and the order of appearance should be reversed in the Appendix section.
   The analytical derivation is now the only appendix and is Appendix A. The former integral-length-scale Appendix B has been removed.

Section 3. Methodology

7. Lines 184-197. Instead of using (i), (ii), etc., it would be clearer to refer to the actual subsection numbers.
   We have replaced the numbered list with references to the actual subsection numbers.

8. Line 198. Same remark: point to the actual subsection number instead of a vague "below".
   We have replaced the vague reference with the relevant subsection number.

9. Line 208 / Eq. 8. What does U denote here? Is it the hub-height wind speed or the equivalent wind speed? We get it that it comes from the turbine technical specifications, but it would be good to precise what it is.
   We have clarified that U is the scalar reference wind speed at which the turbine power curve is evaluated and that it is set to U_{eq} in the rotor-integrated calculations.

10. Line 212. The website url should be included into a reference and not directly in the text.
    The turbine website is now included through a bibliographic reference rather than appearing directly in the text.

11. Lines 223-224. "the mean wind-speed profile and the wind-speed standard-deviation profile are both represented [by a Taylor expansion] over the rotor layer. The words within [...] are missing for the sentence to actually bring some

information. A colon is also missing at the end of line 224.
We have added that both profiles are represented by Taylor expansions and added the missing colon.

12. Line 226. Writing about the Taylor coefficient for the mean wind while the formula just above does not use them is confusing. It would probably be clearer to say that the the wind-speed standard-deviation profile is represented by a Taylor expansion in a similar way to the the mean wind-speed profile, using coefficient noted as c_{sigma_v,i} instead of c_{v,i}.
We have rewritten the paragraph to explain separately that the wind-speed standard-deviation profile is represented by a Taylor expansion using the coefficients $c_{\sigma_v,i}$.

13. Lines 229 and 231 / Eq. 11 and 12. Are the <P_z> numerical values or analytical expressions? If they are numerical, then P_turb is a discretized integral and the z step should be precised.
The quantity <P>_z is continuous in height, not a set of discrete values at the LiDAR gates. Because both the mean wind-speed and standard-deviation profiles are represented by continuous Taylor expansions, <P>_z is defined continuously across the rotor layer. We added a sentence explicitly stating that Equation 12 does not require discretisation into height bins.

14. Section 3.4 was better in the previous version, i.e. the cases description and formulas that have been moved to Section 4.1 should have stayed here. We understand that the authors wanted to give only the methodological framework in Sec. 3, and show the application cases in Sec. 4. However, for readability, it is better to give all the analytical formulas in the methodology section, so that Sec. 4.1 can focus on the results in terms of power.
We have moved the definitions and analytical formulas for the idealised shear and veer profiles back into the methodology section. Section 4.1 now focuses on the resulting power differences.

15. Line 261. “cross-streamwise” is just “cross-stream”
Corrected to “cross-stream”

16. Line 268. “its influence on the results is presented” -> “will be”
This wording was corrected in the previous revision. In the final revision, the surrounding integral-length-scale discussion has been shortened further and this sentence is no longer present.

17. Line 269. “As shown later in Sec. 4.4, and relevant for the methodological choices made here,” this makes an awkward transition from the previous sentence ending by “Sec. 4.4”. Consider replacing by something more simple like “In particular, it will be shown that…”
    The transition was simplified in the previous revision. In the final revision, this extended length-scale discussion has been removed from the Results section altogether.

18. Line 281-282. “the inflow advects over several kilometres”: this sentence is awkward. The verb “to advect” should be used in passive form, e.g. “the air mass is advected over several kilometres”.
    This sentence was corrected in the previous revision. The paragraph in which it appeared has now been removed as part of the shortening of the turbulence-length-scale discussion.

19. Lines 292-293. The numerical values are given in Table 1 so there is no need to remind them here, nor give the numerical formula (Eq. 17). You can just say that h_E92 and h_WP3.0 are the hub heights and R_E92 and R_WP3.0 are the rotor diameters of the E92 and WP3.0 turbines, respectively, which values can be found in Table 1.
    We have removed the repeated numerical values and the numerical form of the transformation equation. The hub heights and rotor radii are now defined using the notation in Table 1.

20. Figure 5. Please carefully check that the scheme is consistent with the formulas in the text and that all relevant parameters are mentioned. As for now, there are still small mismatches between the math notations above the arrows and the text: for the power curve fitter, v should be U and P(v) should be P_cp(U); for the wind profile scaling, the r should be R; for the outputs, P_AN should be P_an. Some of the parameters, like Δz_lidar or v_in / v_out, do not appear on the scheme so far.
    We have revised the workflow figure to make the notation consistent with the manuscript. The power-curve input is now U and P_pc(U); the turbine radii use R; the analytical output is P_an; Δz_lidar is shown in the profile-scaling input; v_in and v_out are indicated for the power-curve fitter; equations references have been updated accordingly.

21. Lines 307-310. A reference to Sec. 4.3 should be added in this paragraph.
    We have added a reference to Section 4.3.

Section 4. Results and discussion

22. Line 721. There must be a space to remove between the parenthesis and the dot.
    Fixed.

23. Table 2. The minus sign differs from the dash symbol, please use a proper minus sign for the negative values (to pick up in the special characters in Word).
    Fixed.

24. Lines 529-530. “the individual 10-minute estimates show large variability, ranging from a few metres to several hundred metres depending on height, component, and inflow period”: this statement should be supported by (a) figure(s) (see the comment about App. B, line 759) as Table B1 does not give a full picture of the length scale variability.
    Rather than expanding this secondary analysis, we have removed the quantitative LiDAR-derived length-scale results, the associated table, and the former Appendix B. The revised manuscript no longer uses this variability statement to support the model.

25. Lines 541-543. “The tested configurations include the original short-scale USRVKM example profile, a modified component scaling of (1.0,0.8,0.6), a length-scale profile increased by a factor of ten, and a combined case in which both modifications are applied.” So the length scales derived from the lidar observations were not used for the simulations? A comment would be needed, saying for instance that these values are covered by the ×10 case.
    Correct. The LiDAR-derived length scales were exploratory estimates and were not used directly as the main OpenFAST input. We now state this much more clearly by removing the derived profile and its sensitivity exercise entirely. The final manuscript does not claim that LiDAR-derived integral scales define the TurbSim input.
    This exercise served as an argument in the last iteration to show that the length scales of the user defined input are insensitive for the final results and therefore do not affect the baseline to which we compare our novel contribution, thus not influencing the conclusions of our work.

26. Lines 546-547. “from 1151 kW in the base case to 1151 kW for the modified component scaling, 1151 kW for the increased length scales, and 1150 kW when both modifications are applied.” As the values are all the same (or almost), they are not useful. The results should be directly given in terms of difference to the base case, only reminding the base case power value at the end of the paragraph.
    The former multi-configuration length-scale sensitivity paragraph has been removed, so these nearly identical absolute values are no longer reported. Instead, the revised methodology reports only the directly relevant IEC Kaimal

check for the representative high-turbulence case.

27. Lines 551-552. “This was done for the representative case in which the largest deviations in the velocity spectra were observed”: does this sentence only refer to the seed test or to all the turbulence sensitivity tests? If it is the latter case, then this information should be provided earlier, around line 541.
    The former seed and length-scale sensitivity exercise has been removed. The remaining turbulence-model check is now identified explicitly in the Methodology as the T3-L10 northerly case, selected because it has the strongest turbulence among the Gotthard validation cases.

28. Line 552. “corresponding to wind profiles around 9 m/s” -> “speeds”, not “profiles”.
    The sentence has been removed with the former turbulence-length-scale sensitivity section.

Appendix B

29. The references relative to the method described in this appendix must absolutely be provided.
    The detailed LiDAR integral-length-scale method is no longer part of the manuscript. The short description that remains concerns TurbSim behaviour and now cites the TurbSim User’s Guide (Jonkman, 2016).

30. The notations must be clarified, as the text oscillates between the zonal/meridional and the streamwise/cross-stream decomposition of the wind.
    The detailed component decomposition has been removed together with Appendix B, so this ambiguity no longer appears in the manuscript.

31. Lines 741-742. “the five beam azimuths” rather than “positions”. Also, “V” should be vertical, please avoid jargon.
    The detailed beam-by-beam description has been removed together with Appendix B. We therefore no longer use the “positions”/“V” terminology in this context.

32. Lines 743-744. The way you explain it, u and v do not appear to be the streamwise and a so-called “lateral” wind component, but the meridional and zonal wind components (and by the way, the usual notation are the reverse). If you did compute the streamwise and cross-stream components, it should be said more clearly. The formula do not need to be reminded, as this decomposition is common, but a reference should be provided.
    The detailed reconstruction and component notation have been removed. No

streamwise/cross-stream integral-length-scale decomposition is used in the revised manuscript.

33. Line 744. The “V” can be removed after “vertical beam”.
    The sentence has been removed with Appendix B.

34. Line 749. The index i is not u, v or w, as we actually have u_1=u and not u_u, u_2=v and not u_v and u_3=w and not u_w. It would probably be easier to directly introduce the u_i’s in the previous paragraph.
    The notation and equations in question have been removed with Appendix B.

35. Line 752 / Eq. B3. The autocovariance function computed from the radial wind speeds recorded in a single beam direction is impacted by the lidar instrumental noise at zero lag (Bonin et al., 2016; Lenschow et al., 2000). In this case, the autocovariance function must be extrapolated towards the zero lag instead of using the zero-lag value as it is. How is this point dealt with when computing autocovariance after combining several beams? Did you perform any kind of instrumental noise correction?
    No explicit zero-lag instrumental-noise extrapolation was applied in the exploratory analysis presented in the former Appendix B. We agree that this introduces an additional uncertainty in the derived integral length scales and limits their quantitative interpretation. Importantly, however, these LiDAR-derived values were not used as exact TurbSim inputs; they served only to establish that plausible Gotthard integral scales could span a much broader range than the original USRVKM example values and thereby to motivate deliberately wide sensitivity tests. An instrumental-noise correction would therefore shift the derived estimates within, or potentially further into, the broad range already explored rather than alter the purpose of those tests. The resulting sensitivity experiments showed that even order-of-magnitude changes in the prescribed length scales produced only very small changes in 10-minute mean power. Nevertheless, because the reviewer correctly identifies an additional source of uncertainty in the LiDAR-derived values, we no longer present these estimates as quantitative site-specific integral length scales or use them to prescribe TurbSim in the revised manuscript.

36. Line 758 / Eq. B5. This equation suggests that u an v are the streamwise and cross-stream components of the wind, which is in contradiction with lines 742-743. Please clarify.
    The equation and the associated component-scaling analysis have been removed with Appendix B.

37. Line 759. If we understand correctly, turbulence length scales derived for all the lidar positions within the Gotthard Pass, for all inflow directions and for all hours of the day have been used altogether to compute the median and percentiles. Did you investigate if the observed variability in length scales could be related to the lidar positions, the main inflow directions, or the night/day contrast? We would like to see figures allowing to see the variability and distinguish the different sites, inflow directions and hour of the day, for the streamwise length scale and the scaling factors, though this could be put in supplementary.
We did examine part of this variability, including differences between measurement locations and the main inflow directions, and these checks confirmed that the inferred integral length scales vary substantially across the Gotthard dataset. However, the purpose of this analysis in the present study was only to establish a broad, physically plausible range for sensitivity testing of the OpenFAST turbulence setup, rather than to develop a site-specific turbulence climatology. A full stratification by lidar position, inflow direction, and day/night conditions would therefore require a substantially more detailed turbulence analysis that is beyond the scope of a paper focused on the analytical rotor-integration method. To avoid giving this secondary aspect disproportionate weight, we have removed the former Appendix B and the combined length-scale statistics from the revised manuscript. The remaining discussion uses the lidar analysis only as qualitative motivation for the wide sensitivity bounds that were tested, not as a definitive characterisation of Gotthard turbulence.

38. Lines 759-760. “uncertainty margins are calculated from the 16th–84th percentile range”, why was this peculiar percentile interval chosen?
The percentile-based uncertainty calculation has been removed together with the LiDAR-derived integral-length-scale analysis.

39. Line 766. The notation α already refers to the veer angle. Using another letter would be clearer.
The component-scaling notation in question has been removed together with Appendix B.

Typo

40. The font used for indices in math notations should be italic when it refers to a variable (c_v, K_m…) but regular when it is a precision in the variable name (P_an, v_in…).
We have revised the subscripts throughout the manuscript so that variable indices are italic and descriptive subscripts are upright.

41. Some references to equations still do not follow the WES guidelines (eg., “Eq.2” in line 126 that should written as “Equation 2”). The same holds for appendices, that should not be abbreviated (line 182 and further in the text).
    We have revised the references throughout the manuscript to follow the WES guidelines, including writing “Equation” and “Appendix” in full.

Reference list

42. For conferences, “International Conference” is usually abbreviated as “Int. Conf.”, then you should try to build an abbreviated conference name based on similar journal names, for which you can check the normalized abbreviations e.g. from:
    https://woodward.library.ubc.ca/woodward/research-help/journal-abbreviations/
    We have abbreviated the conference names consistently with the journal guidelines.

43. Letizia et al.: “Journal of Physics: Conference Series” should be abbreviated as “J. Phys.: Conf. Ser.” as for Clifton et al. or Niayifar et al.
    We have changed the journal abbreviation to “J. Phys.: Conf. Ser.”

44. Murphy et al. et Troldborg et al.: “WES” is not the proper abbreviation for Wind Energy Science, but “Wind Energy Sci.”
    We have changed the journal abbreviation to “Wind Energy Sci.”

45. Redfern et al.: “Monthly Weather Review “ should be abbreviated as “Mon. Weather Rev.”
    We have changed the journal abbreviation to “Mon. Weather Rev.”

46. Stull: the title “An Introduction to Boundary Layer Meteorology” is repeated at the end of the reference, please correct.
    We have removed the duplicated title from the reference.

References cited in this review

Bonin, T. A., Newman, J. F., Klein, P. M., Chilson, P. B., and Wharton, S.: Improvement of vertical velocity statistics measured by a Doppler lidar through comparison with sonic anemometer observations, Atmospheric Measurement Techniques, 9, 5833–5852, https://doi.org/10.5194/amt-9-5833-2016, 2016.
Lenschow, D. H., Wulfmeyer, V., and Senff, C.: Measuring Second- through Fourth-Order

Moments in Noisy Data, J. Atmos. Oceanic Technol., 17, 1330–1347, https://doi.org/10.1175/1520-0426(2000)017%3C1330:MSTFOM%3E2.0.CO;2, 2000.

References cited in this response:

Jonkman, B. J.: TurbSim User's Guide, Version 2.00.00, National Renewable Energy Laboratory, revised 1 June 2016.

Bonin, T. A., Newman, J. F., Klein, P. M., Chilson, P. B., and Wharton, S.: Improvement of vertical velocity statistics measured by a Doppler lidar through comparison with sonic anemometer observations, Atmos. Meas. Tech., 9, 5833–5852, 2016.

Lenschow, D. H., Wulfmeyer, V., and Senff, C.: Measuring Second- through Fourth-Order Moments in Noisy Data, J. Atmos. Oceanic Technol., 17, 1330–1347, 2000.

Editor

Dear Brandon van Schaik and co-authors,

Thank you for revising the manuscript. I have now received the report from the remaining reviewer, who recommends a minor revision. I am therefore pleased to recommend a minor revision, with the revised manuscript and point-by-point response to be assessed by the editor (me). Please carefully address all the reviewer's comments in your response.

Regarding the treatment of turbulent integral length scales, I recommend removing this discussion, including the associated analysis in Appendix B and Section 4.4. The baseline profile L=[3,4,6,9,13,13,13] m is taken from the TurbSim USRVKM example file. That file explicitly states that these values are completely arbitrary and should not be used in simulations. They therefore cannot provide a scientifically defensible baseline, even for a sensitivity analysis. Demonstrating limited sensitivity to modifications of an arbitrary baseline does not validate the baseline itself.

The LiDAR-derived estimates of the integral length scale seems too unreliable to be useful, unfortunately. The estimated streamwise integral length scales have medians of approximately 50–70 m but spreads of approximately 70–100 m. Moreover, the inferred scaling (L_w/L_u=1.20), implying a vertical-velocity integral length scale larger than the streamwise-velocity integral length scale, is not physical. Thus these data do not appear sufficiently robust to define component-specific integral length scales for TurbSim.

More importantly, the manuscript shows that the precise turbulence-spectrum and length-scale settings is not absolutely needed for the conclusions of the manuscript. I therefore suggest keeping the turbulence sensitivity analysis simple: A deterministic case without turbulence may be compared with a turbulent case based on the IEC Kaimal Normal Turbulence Model, using an appropriate representative turbulence intensity. There is no clear need to use the user-defined von Kármán spectrum or to prescribe site-specific integral length scales. Removing this secondary discussion would shorten the manuscript, improve its clarity, and avoid unsupported physical interpretations that are not necessary for the main conclusions.

Best regards,

Etienne Cheynet

We thank the editor for identifying this issue. We agree with the central point that the USRVKM example-file length scales are arbitrary demonstration values and should not

be presented as a physically defensible Gotthard turbulence parameterisation. We have therefore removed the former Appendix B, removed the detailed integral-length-scale and component-scaling analysis from the Results section, and removed all physical interpretation of those length-scale values. We also no longer use the LiDAR-derived integral scales to define TurbSim or to support any conclusion.

We additionally implemented the editor's proposed IEC Kaimal NTM check. The TurbSim documentation is important here: IECKAI defines its spectra from hub-height quantities and assumes the component standard deviations to be invariant across the simulation grid. When a numerical turbulence intensity is supplied, it therefore represents a single rotor-level turbulence intensity rather than the measured height-dependent standard-deviation profile. By contrast, USRVKM allows us to retain the measured vertical profiles of mean wind speed, wind direction, and wind-speed standard deviation. This distinction matters for the present paper because the analytical turbulence formulation is explicitly evaluated using the observed vertical variance profile.

To test whether replacing USRVKM by IECKAI would materially improve the OpenFAST validation, we repeated the T3-L10 northerly case, which is the strongest-turbulence Gotthard validation case. The measured LiDAR turbulence-intensity profile was reduced to a single representative value using rotor-area weighting and supplied to the IEC Kaimal NTM. After the same 2.35 MW/3.0 MW rated-power scaling used in the manuscript, the OpenFAST bias is −103 kW, compared with approximately −80 kW for the existing USRVKM simulation. The difference is modest relative to the overall spread of the 10-minute power estimates, and the point-to-point scatter is not improved. Thus, for this representative case, adopting IECKAI does not materially improve the OpenFAST comparison and necessarily discards the measured height dependence of the turbulence variance.

For that reason, we have retained the existing USRVKM turbulent OpenFAST simulations, but we have substantially narrowed how they are interpreted. The prescribed length-scale values are treated only as numerical settings and are not assigned physical meaning. The deterministic OpenFAST simulations are now explicitly identified as the primary numerical validation of the analytical response to controlled shear and veer. All turbulent OpenFAST results, including the idealised homogeneous-turbulence comparison, are explicitly secondary qualitative or mean-power consistency checks rather than a validation of turbulence spectra or integral length scales. For the Gotthard cases, USRVKM has the specific advantage that it retains the measured height-dependent turbulence-variance profile. We believe this addresses the underlying

concern while avoiding the replacement of measured vertical turbulence-variance information by a generic grid-invariant IEC representation that did not improve the representative OpenFAST comparison.

We would also like to emphasise the role of OpenFAST within the scope of the manuscript. The novel contribution of this study is the analytical rotor-integration method, while OpenFAST is used only as an independent numerical reference to assess whether the analytical response to controlled inflow variations is broadly consistent with a higher-fidelity aeroelastic model. This distinction is important in interpreting the turbulence discussion. In the previous revision, substantial changes to the prescribed USRVKM length scales produced only very small changes in 10-minute mean power, and the additional IECKAI test performed here likewise yields essentially the same bias without reducing the point-to-point scatter. Although neither turbulence-generator configuration can be regarded as a site-specific representation of Gotthard turbulence, these sensitivity checks indicate that the choice between them has little influence on the particular quantity for which OpenFAST is used in this study. We therefore retain the existing simulations as a secondary reference comparison, while avoiding any broader interpretation of their turbulence-spectrum or integral-length-scale settings.

The revised manuscript cites the TurbSim User’s Guide for these model properties and has been shortened accordingly.