Reply to the referee comments

Investigation of higher order statistics in wind turbine wakes using Large Eddy Simulations

Referee’s comments (RC) in blue
Autor’s response (AC) in black

Referee #1

The manuscript presents a comprehensive investigation of higher-order statistics in wind turbine wakes and provides interesting insights into the concept of the intermittency ring. The methodology is sound, the analyses are thorough, and the conclusions are well supported by the presented results. I believe this work is suitable for publication after addressing a few technical corrections.

1. The novelty presented in the abstract could be stated more explicitly. The abstract currently mentions 'a new method', but it is not sufficiently clear what this new method actually refers to. I suggest briefly describing the key methodological innovation here. The authors statement that the proposed approach reduces the computational cost by approximately 20% should be accompanied by a short explanation of why this reduction is achieved, so that readers can better understand its significance.
The Abstract is revised to clarify the improvements of the new approach. Also, the explanation in the main text is extended (line 173-182).

2. Please carefully check the formatting of mathematical variables throughout themanuscript. Several variables that should be typeset in italics appear in upright font. For example, in Figure A1, the symbol D in '3D' should be italicized.
All ‘D’ references were italicized.

3. The figures in the Appendix could be improved for readability. For example, in Figure C1, the subfigures are arranged too closely together, making the y-axis labels difficult to read. Increasing the spacing between panels or adjusting the layout would significantly improve the figure quality. Please also check other figures through the manuscript.
The spacing between the subfigures are increased in Figure A1 and Figure D1 (formally C1 in the preprint).

Referee #2

The article presents LES using both actuator-line (AL) and blade-resolved (BR) approaches to model a wind turbine wake. The investigation of higher-order statistics, including skewness, kurtosis, and intermittency, is interesting and provides valuable insight into wake dynamics, particularly through the proposed intermittency-ring characterization. My primary concern is the inclusion of the proposed BR-based methodology, whose suitability for the present analysis of higher-order statistics is not sufficiently demonstrated. In addition, the manuscript provides limited information regarding turbine control under the selected inflow conditions, and a dedicated discussion of implications and limitations is lacking. I therefore recommend major revisions. In particular, the authors should either provide stronger justification and validation of the BR methodology or consider restricting the analysis to the AL results.

General comments

1. BR simulation and "new method":

The paper is also occasionally unfocused, primarily due to the presence of the BR simulations. Novelty alone does not necessarily justify the inclusion of a new methodology. In its current form, I do not see a clear benefit of the proposed method for the scientific objectives of this study, and its added value should be better justified. People typically use BR to validate AL. The authors compare AL and BR results, however the BR approach relies on a linear superposition of a laminar BR wake and synthetic turbulence. The manuscript itself acknowledges limitations of this assumption in the near wake (line 284). Consequently, it is unclear to what extent the BR results can be considered representative and valid in the region where a blade-resolved simulation would normally provide the greatest value.

That AL and BR reproduce the correct flow field in the near wake (line 155) depends entirely on the load distributions of AL and BR, and how the force smearing in AL is performed. The load distributions should be shown. The assumption of superposing a Mann-generated turbulent field with a BR wake generated under uniform inflow requires validation beyond being stated as an assumption. I consider the underlying assumption to be fundamentally problematic for the objectives of the present study. The methodology effectively neglects nonlinear wake–turbulence interactions while subsequently analyzing higher-order statistics that arise from nonlinear interactions. As a result, I am not convinced that the BR results can be interpreted as physically representative without substantially stronger validation. Obviously, such assumptions are frequently made for simple engineering models, but the BR is supposed to be the highest fidelity. If the BR was performed with inflow turbulence, there would be substantial variance of much larger time scales than 30 s (line 164).

The authors could strengthen the manuscript by demonstrating that this approximation does not affect the conclusions regarding the higher-order statistics. In the absence of such validation, I believe the manuscript would be stronger if the analysis focused solely on the AL simulations. AL is sufficient for the far-wake, where the most interesting aspects are shown by the authors.

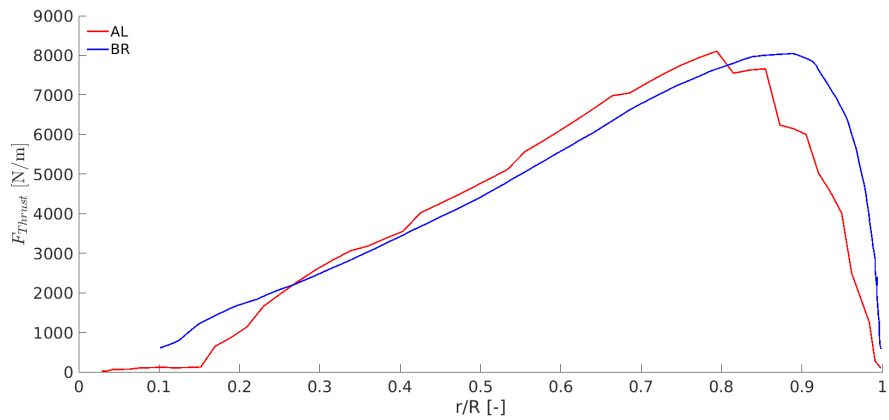
The referee has three points of criticism:

1) The unclear advantage of the new method
Both the abstract and the main text have been substantially revised. Now we mention the computational effort for the AL main simulation at 6.5 days using 768 CPU cores, and an estimated computation time of over 7 months using 768 CPU cores for a BR simulation (line 175-177).
This fact highlights the need for a new method to enable a BR study. Our new method makes this possible. Furthermore, we explicitly state the computational advantage of 20% over AL. This provides the freedom to carry out more investigations (line 185-187).

2) Inadequate validation
It has been criticised that the “a-posteriori-style” validation, based on the agreement of the statistical moments and the intermittency ring with the AL simulation, is insufficient. We acknowledge that the validation can be improved, and have substantially enhanced our work by Appendix B:
There we have carried out a BR simulation with turbulent inflow over a shorter time period (240 s) and compared the results with corresponding results from the AL simulation. This validation shows that, over this shorter duration, comparable profiles and a form of intermittency ring occur.

3) Force smearing
For the AL the turbinesFOAM library is used, where the optimal smearing width is calculated and implied automatically. Therefore, this parameter fixed for the given setup. This information is added to the manuscript in line 173.

As requested, the load distribution of the AL and the BR is shown here. Due to the fixed smearing, this figure has not been included in the manuscript.

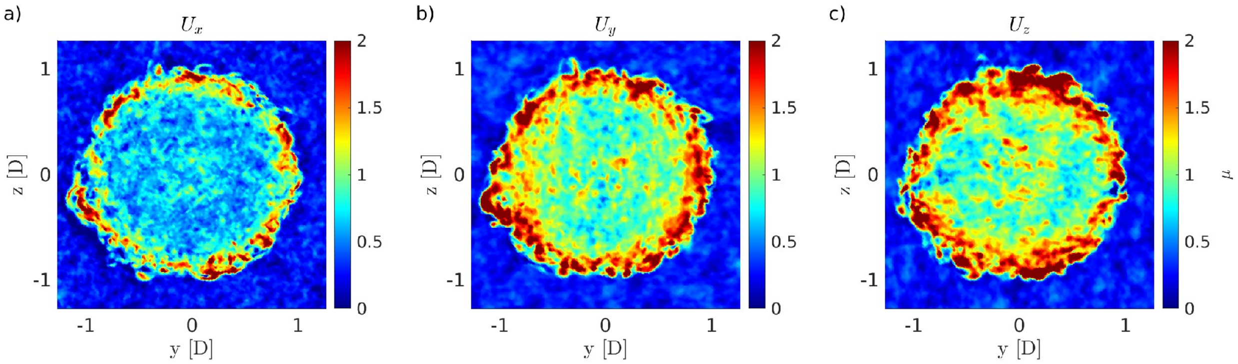


2. Turbulent statistics

As mentioned above, I find the statistical analysis interesting, but I believe some important aspects deserve further discussion.

For instance, this study follows the common approach of most studies to only investigate the streamwise velocity component (line 70). I believe the limitation associated with considering only the streamwise velocity component should be discussed more explicitly, as the lateral and vertical velocity components may also contribute to the higher-order statistical characteristics of the wake. Furthermore, turbulence characterization is not limited to probability distributions and their moments. Spectral characteristics are also fundamental descriptors of turbulent flow and deserve at least brief discussion in the context of the characterization framework presented in lines 65–70. For this reason, I find the statement that turbulence is "completely" or "fully" characterized by the probability density function overly strong, particularly in the non-Gaussian case that forms the focus of the present study.

1) The drawback of the streamwise velocity analysis is now directly mentioned in line 73-76. Additionally, the effect of the velocity component is now covered as an outlook in Appendix E. There, the intermittency parameter is determined for the three velocity components.



2) Accordingly, we have added explicitly the spectral characterisation and their role for the turbulence characterisation in line 71-73.

3) We have never (wanted) to claim that turbulence is completely characterized by the PDF. In line 77 we state that: “The one-point statistics of (componentwise) velocity values U are completely described by the probability density function (PDF)”. To make this section clearer we added the following footnote: “Despite statistics being a powerful tool, the spatial and scale dependence is not covered by it.”

Line 17 emphasizes that turbulence requires higher order statistics, which I agree with. However, the inflow turbulence is generated using the Mann model, which is fundamentally a second-order description and relies on a number of assumptions. The manuscript would benefit from a more detailed discussion of why these assumptions are considered acceptable in a study focused on higher-order statistics. In particular, the relevance of homogeneous and isotropic inflow assumptions to realistic atmospheric wind-turbine wakes should be discussed more critically. Many current LES studies of wind turbine wakes employ precursor simulations to generate more realistic boundary-layer inflow conditions, including shear and turbulence structures, which are subsequently used as inflow.

1) The second-order description of the Mann model is not an issue for a LES investigation on higher order statistics. A previous work showed, that the Gaussian Mann model converges to an correct intermittent state, when used as inflow for LES. Exactly this fact is mentioned already in the preprint; Now explicit in line 314, (implicit in line 139, 299) with the corresponding papers cited there. Furthermore, Appendix D (Figure D1 d) reproduces these findings and also the intermittency parameters in the corners of Figure 12 align with it.

2) The focus of the study is the pure wake profil. Therefore, shear is not included in this first study on the intermittency ring in LES. Additionally, the K41/K62 model assume HIT. To make that more clear for the reader Sect. 3.2 (line 149-153) was adopted accordingly by including the argumentation mentioned here.

I am not fully convinced that 24 minutes of data is sufficient to ensure convergence of all investigated statistics, particularly those related to rare extreme events and intermittency. I follow the arguments based on integral time scales, but these are determined from the Mann box and might not be representative of a 5 MW turbine operating in the real atmosphere. While the argument based on integral time scales is reasonable, additional discussion of statistical convergence would strengthen the manuscript, especially given that substantial uncertainty may remain even for considerably longer simulations. I also find the connection to IEC recommendations somewhat indirect. The IEC standard recommends six 600 s simulations for load analysis, and even under those conditions substantial uncertainty in statistical convergence remains. See, for example, Figure 6 in Liew and Larsen (2022), https://iopscience.iop.org/article/10.1088/1742-6596/2265/3/032049/meta, and Mozafari et al. 2023, https://doi-org.proxy.findit.cvt.dk/10.1177/0309524X231163825.

The requested discussion of the statistical converges is already included in the manuscript in Appendix A, where a convergence study is carried out of the velocity profile and the intermittency ring. Both quantities show convergence from 960 s onward with the difference in the velocity is less than 0.05 m/s (<1%) across the radius (see Figure A1 b). The same applies for the higher order statistics: the fully developed intermittency ring and reasonable values in the free stream appear from 960s onwards (see Figure A2).
The convergence length as an output of Appendix A is now added to the main text in line 164.

3. Inflow conditions and control

The selected inflow velocity of 11.4 m/s corresponds to the rated operating condition of the NREL 5 MW turbine. Given the presence of turbulence, the turbine is expected to experience both below-rated and above-rated conditions during the simulation. Since turbine control can significantly influence wake development in this operating regime, the manuscript should provide additional information regarding the implemented controller. The use of a constant rotor speed alone is difficult to interpret and justify without further context.
The focus of this work is on the flow phenomena in the wake. Therefore, we removed the uncertainty from the controller by keeping the operational state constant. i.e. no controller, constant pitch angle, constant rotational speed.
A more precise description of this operating state was added to the manuscript in Sect. 3 (line 129-133). Additionally, works with a similar approach where cited.

The turbulence intensity should also be stated explicitly and consistently.
The discussion of TI is reported on the following places:
line 13: Motivation
line 79: Definition
line 149, 152, 153: Inflow field
Figure 3 b): spatial TI distribution
Figure 4: TI profile (BR)
Figure 6: TI profile (AL)
Figure 7 b): normalised TI profile
Figure B1 b): spatial TI distribution
Figure C1 b): spatial TI distribution at 6D
Figure C2 b): spatial TI distribution at 8D
Figure D1 a): TI development

4. Lack of discussion

The manuscript would benefit from a dedicated discussion section addressing both the implications and limitations of the findings.

For example, the observation in line 339 regarding the potential influence of the intermittency ring on downstream turbines is particularly interesting, but it is introduced only in the conclusion. Such implications deserve discussion earlier in the manuscript.
The potential effect on downstream turbines and the layout planning is added to the introduction in line 38-40.

5. References:

- Line 19 emphasize the importance of unsteady flow features, but also that the importance is increasing, yet 2 of 3 references are more than 10 years old, so it's hard to follow that it is increasing. I think it would be beneficial with more recent references to support this otherwise good point. Examples could be Biswas and Buxton, JFM, 2026, https://doi.org/10.1017/jfm.2026.11665, and Hodgson et al., Physics of Fluids, 2023, https://doi.org/10.1063/5.0162311

The references for that point were updated accordingly in line 20.

- Sorensen and Kock, 1995, deals with actuator disc, so it is not an appropriate reference for actuator lines (line 61). Actuator line modelling was introduced in Sorensen and Shen, 2002, https://doi.org/10.1115/1.1471361

Thanks for that note. That was our mistake. The reference is corrected.

- Line 200 states "is often evaluated in wake studies". Such statements require references.

References to prove that point are now included.

- Wake meandering is more than just a result of a turbulent environment as assumed in Larsen et al., 2007 (line 214). I suggest the authors investigate the review paper by Yang and Sotiropoulos, 2019, https://doi.org/10.3390/en12244725

Having reviewed the proposed paper and given the matter further consideration, we have come to the conclusion that the Larsen quotation is not only inadequate at this point, but that no quotation is required here at all. Therefore, we have removed the reference there.

- On self-similarity (Section 4.2), the paper by Xie and Archer, 2014, is also relevant https://doi.org/10.1002/we.1792

References were included to Section 4.2 / line 252.

Minor comments:

The article could improve with general proof-reading, so here are just some examples:

- The blue line in Figure 10 is hard to see

The colour of the line is changed to make it more visible (Figure 10 preprint = Figure 11).

- In Figure 9 and similar figures the caption reads that the distributions have been shifted horizontally. I believe the distributions have been shifted vertically rather than horizontally.

Yes of course they are shifted vertically instead of horizontally. Corrected.

- line 347: "ware" should be "wear"

Corrected.

Referee #3

Review of ”Investigation of higher order statistics in wind turbine wakes using Large Eddy Simulations”

The manuscript presents an analysis of higher order statistics of the streamwise velocity component in wind turbine wakes and investigates the intermittency rind around a wind turbine wake. Overall the manuscript presents interesting and relevant results and is well written. However, I have some concerns on the chosen methodology and recommend minor improvements to the manuscript. I recommend the article for acceptance after major revision.

Major
The motivation for the chosen methodology is not clear to me. The comparison between blade resolved and actuator line seems not very conclusive due to the different set ups and the BR approach does not have a clear superiority in fidelity due to the linear superposition of synthetic turbulence and laminar wake flow. The approach to include the BR results disregards the effects of inflow turbulence on the vortices generated by the blade which will trigger non-linear instabilities much sooner than in a laminar case. Furthermore, I don’t see the advantage of resolving the blades but then disregarding turbulent inflow. Wouldn’t one of the benefit of resolving the blades be the ability to resolve the transient effects of turbulent inflow on the airfoil lift and drag, which the standard ALM cannot represent? Finally, the appendix highlights another shortcoming of comparing the two approaches as the intermittency parameter of the empty domain evolves with downstream distance. Since the turbulent inflow is introduced at two different positions, the background flow evolves differently between the two approaches. I think the approach either needs to be validated more to highlight the benefits, or, in light of the negligible difference in the results, the BR results should not be included. This would also help to straighten the manuscript.

1) The unclear advantage of the new method
Both the abstract and the main text have been substantially revised. Now we mention the computational effort for the AL main simulation at 6.5 days using 768 CPU cores, and an estimated computation time of over 7 months using 768 CPU cores for a BR simulation (line 175).
This fact highlights the need for a new method to enable a BR study. Our new method makes this possible. Furthermore, we explicitly state the computational advantage of 20% over AL. This provides the freedom to carry out more investigations (line 184).

2) Inadequate validation
It has been criticised that the a-posterior-style validation, based on the agreement of the statistical moments and the intermittency ring with the AL simulation, is insufficient. We acknowledge that the validation can be improved, and have substantially enhanced our work by Appendix B:
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Minor

• l. 122: From the text I infer that the turbine has a fixed rotor speed, this fact should be emphasized more. Furthermore, rated speed is by definition the point where the pitch controller starts to pitch the blades to the keep the rotor speed constant, changing the thrust coefficient and therefore the ratio of turbulence intensity to normalized wake deficit. I think for the purpose of this study, a lower wind speed well within region 2 of the controller, i.e. between 8 and 10 m/s would be more suitable as the thrust coefficient stays constant in that region. Therefore, either the choice should be motivated more clearly or a different condition should be chosen.

In the present study, we would like to focus exclusively on the flow phenomena in the wake and therefore remove the uncertainty from the controller. So the operational state is kept constant. i.e. no controller, constant pitch angle, constant rotational speed.

A more precise description of the operating state was added to the manuscript in Sect. 3 (line 129-133). Additionally, works with a similar approach where cited.

• Methodology: The methodology section is lacking some critical information. In particular the BR simulations leave a couple of questions: is a rotating overset mesh used? Is simulation really conducted using LES or is it DDES as in Bock (2026)? For the AL, the smearing width compared to the cord length would be an important quantity to judge the difference to the BR simulation.

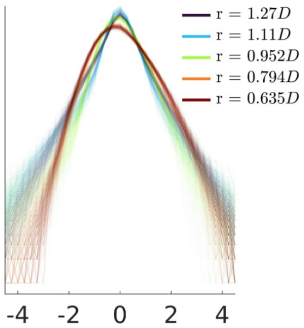
The requested information / clarifications are added to the manuscript. There is now a paragraph specifically explaining the setup of the BR simulation from line 191-195. There we write:

“The setup of the BR precursor-simulation is based on Dose et al. 2018 and Bock et al. 2026 and differs from the main simulation. It implies a DDES model, where the walls are modelled using a high-Reynolds approach with a resolution of 30 < y+ < 70. It uses a rotor mesh which is connected to the fixed mesh via an arbitrary mesh interface according to (Farrell and Maddison 2011; OpenCFD, 2024). To reduce the computational effort, a smaller refinement pattern is used than in the main simulations.”

For the AL the turbinesFOAM library is used, where the optimal smearing width is calculated and implied automatically. This information is added to the manuscript in line 173.

• ll. 254-261: I found the interpretation here somewhat difficult to follow since the discussed feature is not very easy to spot. Maybe the two distributions can be pointed out directly in one of the figures to better guide the reader. The differentiation into two groups is also not obvious to me. Is it based on the fact that there are 2 distributions? If so the sentence should be moved closer to the explanation.

Thank you for pointing this out. In response to our feedback, we have added a further illustrative figure (Figure 10) to the section in question and revised the accompanying text.



Technical Comments

• l. 32: $\lambda 2$ is not described.

L32 is just the introduction. A complete description is done in the fundamentals section Eq. (4) & (5)

• l. 52: p should be in italics.

Corrected.

• l. 62: The reference introduces the actuator disk, since it is an axisymmetric mesh. A line embedded in 3D space and the force smearing, which is a crucial part of the method in my opinion, were introduced in Sørensen and Shen (2002) (doi.org/10.1115/1.1471361)

Thanks for that note. That was our mistake. The reference is corrected.

• Eq 6: the parameter $\Lambda 0$ is not explained.

Corrected. Now we write in line 103: "This can also be determined from the velocity increments using the shape parameter λ^2 and a constant Λ_0^2 related to the boundary conditions […] ."

• l. 221: The nacelle can be modelled using an actuator point with a drag coefficient, see e.g., Churchfield et al. (2015) (https://arc.aiaa.org/doi/abs/10.2514/6.2015-0214)

Unfortunately, this feature is not implemented in the turbinesFOAM library, which is why we simulated the turbine without a nacelle.
https://github.com/orgs/turbinesFoam/discussions/365

• l. 319: I would remove this sentence from the conclusions as this is not a major point of the article in my opinion.

You are right. The sentence is removed now.