Author's Responses 1

Thank you for the valuable feedback on our manuscript. We addressed the remarks in the comments below.

Please note that reviewers comments are written in green, and the authors' responses are written in black.

Please also note that all line numbers in the responses refer to the line numbers in the marked version of the manuscript.

General comments:

Thanks to the authors for answering most of my questions in the response and considering my suggestions in the revised version of the manuscript! However, the authors did not provide a sufficient explanation for the conspicuous behavior of the root bending loads (Fig. 10a), where, in contrast to the other evaluated loads, no heavy tails can be observed. The authors mention that fluctuations do occur also in the RootFlap loads, but with smaller amplitude and a different frequency, which is not re- solved with the selected specific step size. What time step size would be required for this and why was the step size not adjusted in the results presented to visualize the effect? How big are the differences in the amplitudes and frequencies of the different loads (quantitatively)?

Thank you for this remark. The energy spectra of the thrust, torque, and tower fore-aft bending moment show peaks at the 3P frequencies and higher whereas the blade root flapwise bending moment also shows a peak for the 1P frequency. This carries over to the kurtosis spectrum shown in figure 11, which shows also the 3P frequency for the thrust. For the blade root flapwise bending moment, the frequency of fluctuations relates to the 1P frequency of the load energy spectrum. Therefore, the time step was changed in Fig. 10(a) to make the intermittency of the blade root flapwise bending moment visible in the plots. This was clarified more in lines 441-447. Also, Appendix B was added for more explanation.

The authors have now added the important information in table 3 that a tilt angle is considered in the turbine model, which leads to deterministic 1P or 3P load fluctuations. Whether gravity forces were considered remains open.

For the BEM simulations the gravity was enabled but the blades were considered rigid. These two points are now added to Turbine's characteristics in Table 3.

The authors mention 3P loads in the reply to the reviewer, but not in the revised manuscript. I would like to ask the authors to explicitly and completely mention the modeled turbine characteristics that lead to deterministic force fluctuations and to add the influence of deterministic load variations in the discussion and interpretation of Figs. 10 and 11. I would likewise ask the authors to better explain the strikingly different characteristics of the RootFlap loads compared to the other loads. Perhaps the time series of the calculated four loads can help here. I would have expected that the intermittency of the wind would be transferred to the loads, but this inconsistent behavior of the RootFlap loads requires an explanation if the turbine loads are to be included in the manuscript. The development work on the combination of the CTRW with the Mann model is independently relevant and worth to be published.

Thank you for this remark. The energy spectra of the thrust, torque, and tower fore-aft bending moment show peaks at the 3P frequencies and higher whereas the blade root flapwise bending moment also shows a peak for the 1P frequency. This carries over to the kurtosis spectrum shown in figure 11, which shows also the 3P frequency for the thrust. For the blade root flapwise bending moment, the frequency of fluctuations relates to the 1P frequency of the load energy spectrum.
In order to clarify this more in the manuscript, we have added plots for the energy spectra of the blade root flapwise moment and the thrust in the appendix. Further, we have adapted the choice of the time step for the plotting in figure 10a in order to show the intermittency more clearly. To clarify this, we have added a text in line 441-447. Also, Appendix B was added for more explanation.

Minor remarks

1.) L. 309: If the iterative procedure, as described in the reply to the reviewer, is not explained in the cited publication by Schwarz, I suggest to add a sentence on the procedure in the manuscript.

Thank you for the suggestion. We added a sentence in lines 315-318.

2.) L. 319: I can't see an addition of the definition of κ and U here.

The definition of κ and U is now added to line 329.

3.) L. 339/340: In your reply to my last review, you attributed the differences between the Mann and Time-mapped Mann models in Fig. 6(b) and (c) to the interpolation in the x-direction. That may be. However, I still cannot understand the second part of the sentence "...and in this case due to the low number of grid points in the transverse directions".

You are right, this sentence needs more clarification. We mean that the deviations between Mann and Time-mapped Mann fields happen due to interpolation errors that are more obvious in the transverse directions due to the low number of grid points, and accordingly low number of interpolation points, in the transverse directions. This clarification has been added to the manuscript in lines 348-351.

Author's Responses 2

Thank you for the valuable feedback on our manuscript. We addressed the remarks in the comments below.

Please note that reviewers comments are written in green, and the authors' responses are written in black.

Please also note that all line numbers in the responses refer to the line numbers in the marked version of the manuscript.

General remarks

Point 1:

I might misunderstand the background for figure 1, so I'll summarize it here. The plot is compiled from data where the 10-minute average wind speeds ranges from 5 to 15 m/s and the turbulence intensity from 5% to 25%. Imaging that you only have one wind speed but that the turbulence intensity is 5% at night and 25% during the day, and during these periods the turbulence is perfectly Gaussian. Now you calculate the increment pdf which for both night and day are Gaussian with the night pdf being much narrower than the day pdf. But when you add them, they total pdf will become non-Gaussian with a positive kurtosis excess. The situation become more complicated with you included varying mean wind speed, but the example illustrates that you can get small- scale intermittency simply by combining Gaussian distributions with different widths. This is surely not what we are after. I wonder if one takes a long, stationary chunck of the data and do the same analysis whether you get strong kurtosis at all.

Thank you for this observation. In fact, 10-minute time series with such significant statistical differences, especially in terms of the standard deviation, might lead to a miscalculation of the kurtosis. In order to avoid this error, we now constrain the analyzed data to have comparable statistical characteristics of the wind velocity. Therefore, we selected a subset of time series whose values of mean wind speed and turbulence intensity are contained within a specific range. The range is defined as 9 ± 1m/s for the mean wind speed and 0.15 ± 0.02 for the turbulence intensity. Further, we also constrain the data in terms of their wind direction. For that, we consider the mean direction calculated over the 10-minute period. Then, we define a range of $\pm 20°$ from the main wind direction at the specific site. After filtering the data by mean wind speed, turbulence intensity and mean wind direction, 20 out of the 497 available time series are investigated.
Figure 1 has been modified accordingly. As can be observed, the values of the kurtosis significantly drop when considering the constrained measured data. In agreement with your comment, combining time series with highly different mean and standard deviation might result in values of kurtosis mainly driven by the superposition of Gaussian distributions with different widths, rather than by the phenomenon of intermittency in the wind. Nevertheless, values of kurtosis higher than 3 are still obtained after the filtering process of the measured data. That means intermittent features are identified in the statistics of the velocity increments which do not originate from the superposition of Gaussian distributions with different standard deviations. Consequently, figure 1 shows intermittent characteristics observed in the atmospheric wind, which we aim to reproduce in our work.
To clarify these points, lines 149-163 were modified in the marked version of the manuscript.

Another more puzzling point is the equation for the increment

$$v_{meas} = \sqrt{u_1^2(\boldsymbol{x}, t+\tau) + u_2^2(\boldsymbol{x}, t+\tau)} - \sqrt{u_1^2(\boldsymbol{x}, t) + u_2^2(\boldsymbol{x}, t+\tau)}$$

appearing in the text on page 6. So according to the definition, the authors take the length of the fluctuating part of the horizontal vector and subtract that at times separated by τ. This is a very strange procedure. Once could understand if one took the length of the total vector in which case the square root would be roughly equal to $U_1 + u_1$ (see L. Kristensen, J. Atm. Oc. Tech. 1998). As it stands now, any perfectly joint Gaussian u1 and u2 process would give a kurtosis excess of v_{meas}. I think the motivation section should be improved answering these critical questions.

The definition of the increments v_{meas} of the measured wind velocity was not correctly formulated. For an accurate formulation, we consider the definitions introduced for our previous reply in this document. Then, we define
$v_{meas} = u_{meas}(\mathrm{x},\ t+\tau) - u_{meas}(\mathrm{x}, t)$. This formula is now in line 164 in the marked version of the manuscript. Also, line 166 and 177-184 in the marked version were modified to reflect the correct idea behind this analysis.

Point 2:
The mathematics of the modification of the Mann model is quite understandable. However, it is not very physical. For example, why it the focus only on the kurtosis? The skewness remains zero in the modified field although this is the property that is known to be non-zero according to Kolmogorov 1941. The method modifies the intermittency in the x1 direction but it remains perfectly Gaussian in the transversal direction, so the procedure introduces a small-scale anisotropy that there is no experimental evidence for. The resulting field becomes compressible (maybe it doesn't matter to much for loads, but it is a bit unphysical).

It is correct, that the combination of the Mann model and the time-mapping from the CTRW model does not yield all the desired statistics and that there is only temporal intermittency introduced. However, we wanted to go one step further from the idea of the CTRW model as used e.g. in (Schwarz: Wind turbine load dynamics in the context of intermittent atmospheric turbulence, Ph.D. thesis, Oldenburg, 2020) where the different time series are spatially uncorrelated - or fully correlated when the same time series is used many times in a certain area.

An important factor is to compare Gaussian and non-Gaussian wind fields and their effect on loads. Ehrich (Ehrich: Analysis of the effect of intermittent wind on wind turbines by means of CFD, Ph.D. thesis, Oldenburg, 2022) worked with CTRW wind fields which he time-shifted in order to create intermittent wind fields and compared to their Gaussian counterparts. He found an effect of intermittency on turbine loads.

Berg et al. (Berg et al.: Gaussian vs non-Gaussian turbulence: impact on wind turbine loads, Wind Energy 19, 2016) used LES simulations which naturally show non-Gaussian characteristics as input to wind turbine load calculations and performed POD in order to create a Gaussian counterpart from that. It is correct, that their outcome was that the effect of intermittency on loads is not very pronounced. However, we know from literature, that there is still a lot unknown concerning the effect of intermittency on loads, since different studies come to different conclusions. Our intention was to create a non-Gaussian wind field which shares commonalities with a Gaussian wind field in order to isolate the effect of intermittency based on a synthetic wind field model such as the Mann model.

From general turbulence theory, there are certain attempts to generate fields with non-Gaussian increment statistics from a proper superposition of Gaussian fields.

Rosales and Meneveau (A minimal multiscale Lagrangian map approach to synthesize non-Gaussian turbulent vector fields, Physics of Fluids 18, 2006) perform a superposition of Gaussian Fourier modes and rescaled in order to receive a desired spectrum. This points to the superposition of Gaussian pdfs with varying variances as done by Castaing (Castaing et al.: Velocity probability density functions of high Reynolds number turbulence, Physica D 46, 1990), which has also been applied by Wilczek (Wilczek: Non-Gaussianity and intermittency in an ensemble of Gaussian fields, New Journal of Physics 18, 2016) who did superpositions of entire Gaussian fields. A similar approach has also recently been applied to the Mann model by a group from Oldenburg where the small-scale Gaussian statistics of Mann wind fields with varying covariances were superposed to yield small-scale intermittency (Friedrich, J., et al. "Superstatistical Wind Fields from Pointwise Atmospheric Turbulence Measurements." PRX Energy, vol. 1, no. 2, 2022, https://doi.org/10.1103/prxenergy.1.023006.)

However, our intention was to combine well established models in wind energy which we did by combining the Mann model with the time-mapping procedure from the CTRW model. It is always a matter of what is investigated, our model shows spatial correlations in the x_2-x_3-plane similar to the Mann model and allows to investigate the influence of temporal intermittency. to address your comment, lines 356-367 of the marked version of the manuscript were modified.

The authors are only using isotropic turbulence which is far from what is observed in the atmosphere where Γ is usually between 3 and 4. The explanation for this on page 26 is not convincing. Is it difficult to generate a $\Gamma \neq 0$ field and then apply the time mapping?

Considering the Γ-parameter, we wanted to investigate the isolated effect of the time-mapping procedure without additional effects which influence the spatial correlations. This way, all discrepancies certainly come through the time-mapping procedure and the corresponding necessary interpolation procedure. Of course, a $\Gamma \neq 0$ Mann field could be time-mapped. However, there remain open questions. Of course, the shear rate is equal over the whole domain, but the mean velocity is changing. An open question is if the time-mapping should be equal over the whole x_2-x_3-plane or if it should be enhanced in areas with a supposed larger mean velocity. Such considerations are definitely of high value, however, since we aimed at investigating the general characteristics of the Time-mapped Mann field, this would have exceeded the scope of the paper.

We clarified more on that starting in line 486-493 in the marked version of the manuscript.

Point 3:

Coherence (figure 7) is a second-order statistics and it certainly changes a lot. (Please don't show all the irrelevant scatter in the plots, just the smoothed coherences.)

You are right, the scatter should not be shown here. Fig. 7 was modified accordingly.

I don't think this large change in coherence towards a much more pointed shape has been observed anywhere in atmospheric measurements while the theoretical von Karman coherences have been verified for small separations at several occasions. Please comment on this. Since the two-point cross spectra changes so drastically you should also expect the auto-spectra to change (figure 6).

Figure 7 shows the coherence in κ_1-direction of the three velocity components separated by a spatial distance $\triangle x_2$ in x_2-direction in comparison to the Mann model. The Mann model is based on the von Kármán energy spectrum, which is reflected by the coherence of the Mann-modelled wind field. For coherence in x_2-direction with respect to spatial steps in x_3-direction, the coherences of the Mann model and the Time-mapped Mann model fit well, as shown in the appendix in figure A.2. However, for the coherence in x_1-direction, ,you are right, the spectrum shows significant differences to the Mann model. That shows, the time-mapping goes at the cost of the spatial correlations in x_1-direction which is expected since the x_1-axis is stretched and compressed which is also reflected by the spectrum in x_1-direction in figure 6(a). Lines 384-385 and lines 388-390 were changed.

This is obscured in figure 6 in the way the spectra are treated and plotted. First of all, it is customary, and with good reason, to plot the pre-multiplied spectrum ($\kappa_1 F_1(\kappa_1)$) because it makes it easier to see how the variance is distributed on frequency. Secondly, please do bin averaging so you plot an equal number of power spectral densities per decade. In this way it is possible to see the differences between the conventional and time-mapped spectra.

Thank you for the comment. Fig. 6 is re-poltted with ($\kappa_1 F_1(\kappa_1)$) on the y-axis and the noise in the plot was removed as advised.

Also regarding figure 6, I think it is totally unphysical to assume uncorrelated time mapping at every point and it obviously give nonsensical $F_2(\kappa_2)$ and $F_3(\kappa_3)$ spectrum. There are no reason to show these.

Concerning the uncorrelated Time-mapped Mann field, there seems to be a misunderstanding. It is not uncorrelated time-mapping in each point of the slice. The slices are mixed up so that the spatial correlation in the plane is destroyed, but still all points in the x_2-x_3-plane are shifted by the same time step in the temporal direction. This explained in lines 336-337 of the marked version of the manuscript.
But you are right, that at least in the new versions of figure 6 (b) and (c) plotting the uncorrelated version is not reasonable anymore, so we removed it from there.

Point 4:
There seems to be no good explanation on the very non-intermittent behavior of blade root flapwise moment in figure 10. It is also hard to understand the behavior of the kurtosis in figure 11. Why do you see very regular peaks at τ equal to interger seconds?

Thank you for this remark. The plot in figure 10a does not reveal intermittency which is actually there but with another frequency than for the other loads. This goes back to the frequency of the loads. The energy spectra of the thrust, torque, and tower fore-aft bending moment show peaks at the 3P frequencies and higher whereas the blade root flapwise bending moment also shows a peak for the 1P frequency. This carries over to the kurtosis spectrum shown in figure 11, which shows also the 3P frequency for the thrust. For the blade root flapwise bending moment, the frequency of fluctuations relates to the 1P frequency of the load energy spectrum. A non-monotonically decreasing kurtosis is also visible in Schwarz's dissertation (figure 5.8), which confirms our result that the intermittency is not strictly descreasing. However, the results in Schwarz dissertation are not completely comparable because the CTRW time series in there are either fully correlated or not correlated at all. The dependence of the oscillations in the kurtosis plots on the intensity of the spatial correlations in the x_2-x_3-plane is subject to a follow-up paper that we are currently working on. That means there is intermittency, it was not visible with the specific choice of the time step.

In order to clarify this more in the manuscript, we have added plots for the energy spectra of the blade root flapwise moment and the thrust in the appendix. Further, we have adapted the choice of the time step for the plotting in figure 10a in order to show the intermittency more clearly. To clarify this, we have modified the manuscript in lines 441-447

Specific remarks

1.) In the introduction, which is nice, it would be great to be more specific on what the different studies show. Please state how large are the difference in percent instead of stating "very close", "agree quite well", "are different from" wherever it is possible.

Thank you for the remarks. The mentioned sentences are now modified to give a better description of the results of the reviewed researches in the introduction section. The lines 46-47 and 55-57 in the marked version of the manuscript were modified accordingly.

2.) Is there any physical reason for the choice the distribution of time increment maybe related to the fact that it is α-stable? Eq (17) seems to miss "p()" on the left hand side.

Thank you for the notification. Yes, equation (17) indeed describes a probability density function (pdf), $p(\tau_\alpha)$.

An important reason for the special choice Fig.10(a) of the distribution is that for the time shift in the original CTRW model, it has to be ensured that the time step does not become negative. This means according to Kleinhans & Friedrich (Continuous-time random walks: Simulation of continuous trajectories, Phys. Rev. E 76, 2007) that the distribution for the temporal increments has to be a "fully skewed stable distribution".

The pdf for τ_α in our equation (17), which we adapted from Kleinhans & Friedrich, is given in general form in (Metzler & Klafter: The random walk's guide to anomalous diffusion: A fractional dynamics approach, Physics Reports 339, 2000) in their equation (C.11) in *proposition C.3* (adapted to our notation):

$$p_{\alpha,\beta}(\tau_\alpha) = \frac{1}{\pi}\mathrm{Re}\int_0^\infty \mathrm{d}z \exp\left(-\mathrm{i}\tau_\alpha z - z^\alpha \exp\left(\mathrm{i}\frac{\pi\beta}{2}\right)\right). \tag{1}$$

Kleinhans & Friedrich use this equation for the case $0 < \alpha \leq 1$ and $\beta = -\alpha$ (for $\beta \neq 0$, this distribution is skewed).

Metzler & Klafter state in their *proposition C.11* that $p_{\alpha,-\alpha}(\tau_\alpha) = 0$, for all $\tau_\alpha < 0$. This ensures that there are no negative time steps. Further, the asymptotic behavior of this distribution for large τ_α is determined by α, $p_{\alpha,-\alpha}(\tau_\alpha) \sim \tau_\alpha^{-(1+\alpha)}$ (proposition C.5).

In order to enable the reader of our manuscript to look deeper into the derivation of the used form of the pdf of τ_α, we have also added the source for the general derivation of the pdfs to the manuscript in lines 221-222 and line 228.

3.) Smaller language issues. l 37 "realistic" → "realistically", l 110, I think it is more correct to use "component" instead of "direction", l 112 "statistics are" → "statistics is" (also l 319), l 115 "independent from" → "independent of", l 127 "coh" should be "coh", l 225 A distribution is not delta correlated but it can have a delta destribution.

Thank you for the comments, all language issues are now corrected. Regarding the delta-correlated fields, since the idea of the completely uncorrelated field follows the work of Schwarz et al., we have followed the exact wording they have used in their work. However, we have changed this confusing wording to "uncorrelated field" in the manuscript.

4.) Eq (13) A square is missing on the κ after the Kronecker delta symbol.
Thank you. Equation (13) is now corrected according to your comment.