Reply to anonymous Referee No.2: Multipoint Reconstruction of Wind Speeds

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NOTE: All figure and equation numbers refer to the original submitted manuscript and may differ from the ones in the revised version.

L13. I would argue that hydro is more represented in decarbonized energy sources than wind and solar; at least in some parts of the world. It should be included in this list.

We added it to the list in the revised manuscript.

L15. Why wind(solar) is capital in the manuscript?
L25. The citation style is incorrect. Please revise accordingly.
L28. Should be "...known to be..."
L33. There should be a space between 10 and min. Please apply the same correction everywhere else (number and unit separation). Also, the citation style is incorrect. Please revise this issue everywhere in the manuscript.

All changed accordingly.

The exact definition of intermittency (for the context of this study) should be provided in the Introduction. The authors talk a lot about intermittency, but the exact definition is not provided.

We added following definition in the introduction:
"Within this context the term intermittency is used in the spirit of Kolmogorov 62 to describe the characteristic heavy-tailed shape of pdfs often found at small scales in time series of turbulent systems (Frisch, 2004)."

L46. Remove one "and" at the end of this line.

L60. It should be specified that t is the time.

L61–62. Please revise the sentence for proper English.

All changed accordingly.

L60 and L65. Please clarify the difference between u(t) and U(t).

We clarified it in the revised manuscript in the beginning of the method section: "With $U(t)$ we refer to the resulting wind speed from the horizontal components. The quasi-stationary wind speed $u(t)$ is then obtianed from $U(t)$ by respectively normalizing it with the mean $\overline{U}$ and standard deviation σ_U within blocks of 1 min length."

The abbreviation pdf is sometimes italicized and sometimes not. Please be consistent.

L110. There should be a comma after the Pawula theorem. Also, please provide a reference for this claim on L110 and L111.

All changed accordingly.

The reference for the Pawula theorem is Risken 1996. Regarding our claim in L110 und L111, we added following plot to the manuscript: "As one can see in fig. (1), the fourth Kramery-Moyal coefficient is slightly larger than zero, but negligible compared to the magnitude of the diffusion function $D^{(2)}$"

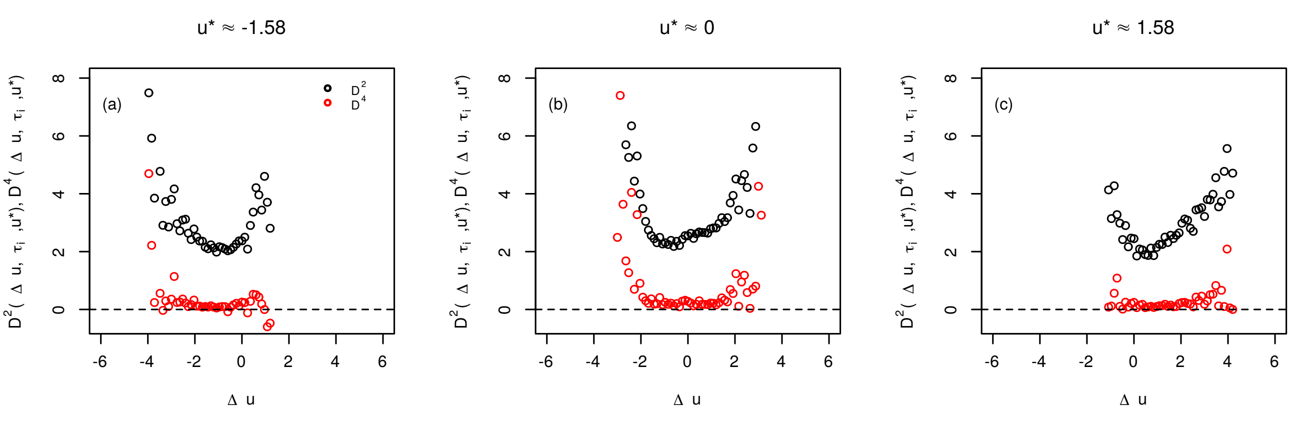

Figure 1. Exemplary estimations of the second and fourth Kramers-Moyal coefficient $D^{(2)}$ and $D^{(4)}$ for $\tau = 65\ s$

L155–156. Why the order of the polynomial of 3 and 2. Is this the lowest polynomial order that properly fits the data?

Indeed third and second order polynomials for the drift $D^{(1)}$ and diffusion $D^{(2)}$ functions are the polynomials of lowest that are properly fitting the data. Emperical studies ((Renner et al., 2001), (Reinke et al., 2018)) suggests that these polynomials are well suited to problems in fluid mechanics. Choosing higher order polynomials is possible as well, but the parametrization might be suffering from overfitting then. Furthermore, up to now we did not see any fundamental changes in the results using higher order polynomials – see a rigorous approach to support these findings by the use of the integral fluctuation theorem for ideal turbulent data (Reinke et al., 2018).

Figure 2. The two labels in the legend are identical, but the different notation is used in the figure caption. Please correct this before this figure can be reviewed properly.
L167. Please correct the English.
All figures. Please add (a), (b), (c), etc. labels for subplots.
L179. I belie that "an" should be "a".
Equation 19. The function exp should not be italicized. The same holds for any other function in the manuscript.
L223. The word min should not be italicized.

All changed accordingly.

MAJOR COMMENT: L226. In non-stationary wind speed records, the fluctuations are dependent on wind speed. Reading this section (and this particular line), this reviewer concludes that the presented methodology does not account for this relationship. For instance, in the case of non-stationary thunderstorm winds, Chen and Letchford (2004) (doi: 10.1016/j.engstruct.2003.12.009) modulated the fluctuations based on the moving-mean wind speed. A similar approach was used by Chay et al. (2004) (doi: 10.1016/j.engstruct.2005.07.007). This has been shown on the example of full-scale data of thunderstorm winds in Burlando et al. (2017) (doi:10.1175/MWR-D-17-0018.1) and Zhang et al. (2018) (doi: 10.1016/j.probengmech.2017.06.003). Notice that in these papers the moving-mean turbulence intensity in the transient (thunderstorm) wind record is not changing in time. This confirms that the fluctuations increase as the mean wind speed increases. Please clarify this issue because it is particularly important for transient wind speed records. This change (previous comment) would perhaps correct for the discrepancies between the measurements and the reconstruction in Figure 8 (pdfs).

This comment of the referee addresses several points to which we want to answer:

Comment on Chen and Letchford (2004) (doi: 10.1016/j.engstruct.2003.12.009): In this paper special wind situations of thunderstorm downbursts are grasped by a deterministic–stochastic hybrid model. The fluctuation is modeled as a uniformly modulated evolutionary vector stochastic process. In our paper we focus on this stochastic part and not on the larger scale deterministic part as (Chen and Letchford, 2004). In contrast to (Chen and Letchford, 2004) we do not model the fluctuations by a stochastic process in time, but we show that a new class of a sstochastic process in scale can be used. Common stochastic processes are Markovian in time and thus are not able to grasp general aspects of multi-point statistics. Turbulent wind signals are in general not Markovian in time, but it is the novelty that we show in our paper that these turbulent wind fluctuations are Markovian with respect to a special scale process (see fig. 2), which enables us to set up a stochastic process in scale. This scale process is more complicated, but statistically more complete.

Cases of rapidly changing wind conditions like thunderstorm events or other transient wind speed changes are not in the focus of our work. We aim at modelling the quasi-stationary wind speed fluctuations $u^*(t)$, which are obtained by a blockwise normalization (of 1 min length) with respect to the mean and standard derivation of wind speeds $U(t)$. This way we decouple the fluctuations from the magnitude of the mean flow. As stated later on, a rescaling of the fluctuations is achieved when we transform the modelled fluctions $u^*(t)$ back to real wind speeds U^*, by multiplying it with the standard derivation: $U^* = (\sigma_U \cdot u^*) + U$.

If and how our approach may be adapted to situations like thunderstorms is out of the scope of our paper, may be just a shortening of our decompositioning in 1 min - blocks is already helpful. We would agree to add this point as a fotenote in our paper or add it to the discussion.

Concerning the discrepancy of Fig 8:

In our data there was no thunderstorm like behavior. The discrepancy is mainly statistical nature. We have two comments to the discrepancies in the plots of fig. 8:

a) Discrepancy in the timeseries: The mean wind speeds $\overline{U}(t)$ were generated by simple stochastic model and thus there will be deviations from the corresponding mean wind speeds from the measurements to the very same timestamp. If we would have used the historic mean wind speeds, there would be only minor deviations.

b) Discrepancy in the increment pdfs: The main deviations in the incrementd pdfs are found at the tails of the pdf on larger scales τ_i (note also the logarithmic y-scale). As these are probabilities our model is virtually completely correct for all scales, especially for small scales τ_i.

MAJOR COMMENT: Related to my previous comment, non-stationary velocity records are often non-Gaussian too. Can you please clarify how is this accounted for in your methodology?

This is correct. The central point for our approach here is the validity of the Markov property. If this is fulfilled, the other parts, like the shape of the probability distributions, are mathematically rigorous consequences. As mentioned above, in a careful investigation one may find wind conidiations for which our approach is not valid. The importance of such cases are out of the scope of our work presented here.

MAJOR COMMENT: The purpose of this methodology is to generate fluctuating wind records. This topic addressed in the seminal paper by Shinozuka (1972) (doi: https://doi.org/10.1016/0045-7949(72)90043-0). Without going into mathematical rigor in this review, the basis of his method is to generate random numbers (through Monte Carlo) that follow the prescribed power spectral density of wind fluctuation (e.g., Kamal spectra, Davenport spectra, von Karman spectra, Mann spectra, etc.). This method is later implemented in some of the studies provided in my comment 20 and references therein. So, my question is how the method proposed in your study extends beyond this well-established methodology of generating wind fluctuations? What are the benefits of using the presented method in your study?

Methods relying on a prescribed power spectral density (PSD) to generate time series of wind speed fluctuations do have the benefit of being computationally fast and applicable without posing much requirements on the data. Nevertheless such methods only provide time series of a predefined length as shown for the amplitude-modulation sheme in (Chen and Letchford, 2004). The benefit of our method is that a time series can be continued in-situ for an arbitrary amount of iterations. Due to the stochastic nature of our algorithm an ensemble of possible scenarios for the evolution of the wind speed fluctuations, starting from a specific situation, can be assembled.
Furthermore, the main point of our paper is that we are mathematically much more general as Kamal spectra, Davenport spectra, von Karman spectra and Mann spectra, which are all low order two point (two time quantities, (see (Peinke et al., 2019)). Thus intermittency (higher order two point quantity) like fig. 8 and more complex multipoint structures (like gusts, see fig. 7) are now grasped by our approach. Our paper will open a new way to investigate such data (see (Fuchs et al., 2020) and (Hadjihosseini et al., 2016)). Note that the knowledge of the Fokker-Planck equation describes in a very compact way all the changes in statistics of two point (time) quantities as shown in fig 8.

MAJOR COMMENT: Can the authors plot spectra of the two velocity time series in Figure 5? Please also include the reference –5/3 slope for benchmarking.

We see a good agreement between the spectra from the mesaurements and the reconstruction with the -5/3 spectra within the internal subrange between $f > 0.1\ Hz$ and $f < 1\ Hz$ (see fig. 2). The flattening of the spectra observed at low frequencies ($f < 0.1\ Hz$), was also noted by (Morales et al., 2012) for wind speeds in a similar range. But as this observation is not of interest for our work, we do not discuss further details here.
Furthermore we would like to stress that the spectrum from the reconstructed time series matches the one from the measurement very well, disregarding the deviations at high frequencies, where we are in the range of measurement noise of the ultrasonic anemometer. This shows that our method is able to capture two-point statistics like the power spectral density, but we would like to note, that we are going beyond, as the generated wind speed fluctuations $p(u^*, t^* | u_1, t^* - \tau_1; ...; u_N, t^* - \tau_N)$ are based

on N-point statistics.

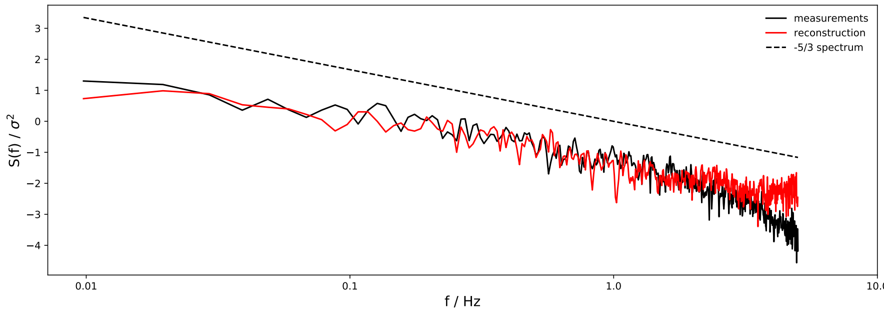

Figure 2. power spectral density of measured and reconstructed wind speed fluctuation

MAJOR COMMENT: Going back to L36 in the Introduction. The authors correctly talk about the spatial dependency of fluctuations and coherence. How is the current model generating fluctuations in space? The presented results are for a point measurement, but the implementation for wind energy (i.e., wind turbine) analysis requires the spatially dependent profile. How a coherence function can be implemented in the method?

The main scope of our paper is to present a new method to generate realistic time series of wind speed fluctuations. An extension to higher dimensions, enabling one to generate wind fields in time and space is of course desirable, but the authors would consider this to be the next step, as this will not be straightforward to do. We have three further remarks on our approach:

a) The one-point time signal corresponds in the common approach of Taylor's frozen turbulence hypothesis to spatial structures in the flow direction, regardless some necessary correction to Taylor's hypothesis.

b) Our approach is statistically complete for one direction (in the sense of grasping any n-point statistics), thus the question to extend this to the full three-dimensional space would run into a statistical solution of the turbulence problem, which is still our dream to pave the way.

c) The knowledge of a one point-time series already provides a better prediction of loads and power outputs as shown by (Wächter et al., 2010)

L238. The phrase "a fairly nice match" is not scientific. Please be specific.

We reformulated our comparison in the revised manuscript.

How computationally efficient is your method? How much computational time is required to generate a fluctuation time series of different lengths? Can you please comment on this?

One step in scripting languages like Python or R takes on average about 0.0005 s at generating a time series of 10^4 length on an ordinary PC. Utilizing langues like C/C++ or Fortran the computation should be boosted at least with a factor 10-100. So the decline in processor power upon generating very large time series will not be of much impact for the practicability of our method. It is also not the computational efficiency which we emphasize here, but the new quality (mutli-point statistics) we give access to by this apporach.

L252–L253. Not necessarily until the method accounts for the spatially coherent fluctuations.

We agree that this is an important aspect. Thus we suggest to add a footnote in our article to clarify this point: "Note, here we do not include the aspect of spatial coherence. To affect a big WEC such temporal fluctuations must have a sufficient large spatial structure."

References. Some citations include article titles while the others do not. In addition, some journal names are abbreviated whereas the others are not. Please be consistent.

Changed accordingly.

Title: what exactly the authors mean by "multipoint?" This reviewer assumes this word signifies the time dependency of the methodology. If yes, isn't this redundant because fluctuations have to be time dependent?

We agree that the term "multipoint" needs to be specified. We will add following explanation in the introduction: "While commonly applied methods, like spectral analysis and two-point correlations, limit themselves to two-point statistics, here we extend the methodology to more than two points in time. We obtain generalized correlations between multiple points in time, in terms of probabiliy density functions (pdfs) for the occurrence of a whole sequence of wind speeds. Those pdfs we denote multipoint pdfs, and they constitute the basic concept of our approach."

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