Reply to anonymous Referee No.1: 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.

Page 1, line 2: The authors introduce the abbreviation ‘cpdfs’, but inconsistently use ‘conditional pdf’ (e.g. Page 3, line 86) throughout the paper. This should be consistent.

Changed in the revised manuscript.

Page 2, line 28: Define what ‘short time scales’ means and how it relates to intermit- tency.

We added following explanation to the revised manuscript:

“With short time scales we refer to time scales in the range of seconds to minutes. As it can be seen in (Boettcher et al., 2003). The effect of intermittency is most prominent at time scales < 1 s, but as the time scales increase, the pdfs broaden.”

Page 2, line 35: How is the complexity of the wind energy conversion process con- nected to the desire of finding 3D velocity fields?

To avoid misunderstanding we reformulated our sentence in the manuscript:

“The missing of the basic understanding, the impracticability of handling such huge data sets as well as the complexity of the wind energy conversion process leads often to the demand of simplified models for wind speed.”

in a new way:

“The impracticability of having all details of a turbulent wind field leads to the demand of the praxis to have access to simplified models for wind speed.”

Page 3, line 60: ‘1 min’ should read ‘1 minute long’.

Changed accordingly.

Page 3, line 60: Even though U(t) is an obvious reference to a velocity, it should be defined in the text (especially as to see whether it is the mean of the full 3D velocity vector or of one component).

We added a definition in the revised manuscript: “With $U(t)$ we refer to the resulting wind speed from the horizontal components.”

Page 3, line 60ff: How much data was used?

We added the information in the manuscript: “The data were recorded at a sampling frequency of 1 Hz between calender week 1 to 10 in 2007 with an ultrasonic anemometer, mounted at 80 m height, resulting in approximately $6 \cdot 10^6$ samples.”

Page 3, line 64f: It should be spelled out what these abbreviations are meant to abbreviate.

Changed accordingly.

Page 3, line 65f: Is this normalization justified? There could potentially a strong coupling between the mean and the standard deviation that erroneously gets averaged by this procedure. The authors should provide a plot of standard deviation vs mean for their dataset to build confidence in this grouping of blocks.

Indeed there is a significant coupling between mean wind speeds and fluctuations. Measuring the strength of the fluctuations by means of the standard derivation, we detect a quadratic scaling between both quantities fig. 1). However 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: $U^* = (\sigma_U \cdot u^*) + U$.

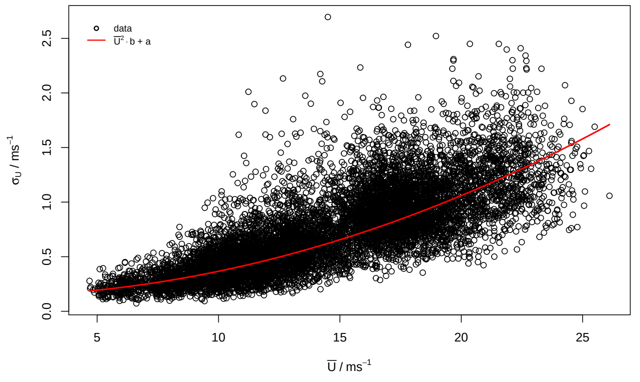

Figure 1. dependency of standard derivation σ_U on mean flow $\overline{U}$

Page 3, line 68: Explain how the wind speeds emerge from a turbulent cascade.

It is well known that the turbulent behavior of wind speed below 10 min becomes similar to the ideal homogeneous isotropic turbulence involving a turbulent cascades from large to small scales. The common arguments are that below 10 min the wind turbulence becomes three dimensional whereas for larger time scales the turbulence is more like a two dimensional turbulence with a cascade to larger scales. This transition between two and three dimensional turbulences with different directions of the cascade, in which the energy is transported, supports the idea of the similarity between three dimensional homogeneous isotropic turbulence and wind turbulence on smaller scales. A statistical analysis of wind data supporting this results are given in (Morales et al., 2012).

Page 3, line 78: Taylors frozen flow hypothesis only holds if the fluctuations are small compared to the mean flow. Is this the case here? Provide numbers.

As one can see from the provided histogram of the fractions of the fluctuations u^* and the mean flow $\overline{U}$ (cf. fig. (2)), in most of the samples the fluctions are at least one magnitude smaller than the mean flow. In accordance we could add to our paper the comment that we use Taylors frozen flow hypothesis as an approximation, which might be improved in future.
However our statement about Taylos frozen flow hypothesis was just supposed to be a general remark regarding the term 'multi-point' in the context of time series. As we are dealing with time series here, there is no check the validity Taylors hypothesis in our paper.

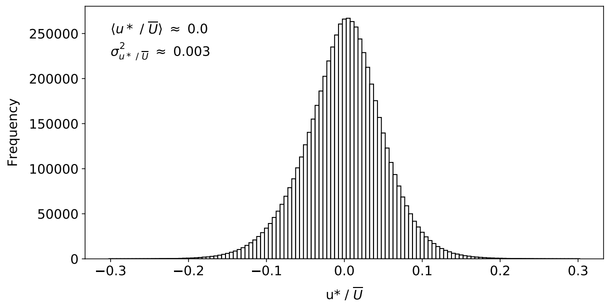

Figure 2. dependency of standard derivation σ_U on mean flow $\overline{U}$

Page 3, line 86: The abbreviation ‘lhs’ (and ‘rhs’) should be defined, even if it is commonly used.

Changed accordingly.

Page 4: The derivation appears unwieldy (and is a reproduction of previous work) and might be better suited for supplemental materials.

We removed this part of the method section accordingly and shifted it to the appendix.

Page 4, line 102: The N. Reinke reference is missing a year.

Changed accordingly.

Page 5, line 105: The citation style for Risken 1996 is different from other references.

Changed accordingly.

Page 5, line 109: The argument on the minus signseems handwaving. Even though it is commonly repeated, a more rigorous, mathematical explanation would be desirable.

The minus (eq. (8)) is motivated by the fact the we consider the turbulent cascade going from large to small scales. With loss of generality a positive sign may be used (see (Peinke et al., 2019)).

Page 5, 110: Provide a reference for the Pawula theorem.

We added the reference in the revised manuscript (Risken 1996).

Page 5, line 111: Provide a plot in the manuscript to show that this approximation is valid for the data at hand.

We added a plot in the the revised manuscript: "As one can see in fig. (3), the fourth Kramery-Moyal coefficient is slightly larger than zero, but negligible compared to the magnitude of the diffusion function $D^{(2)}$"

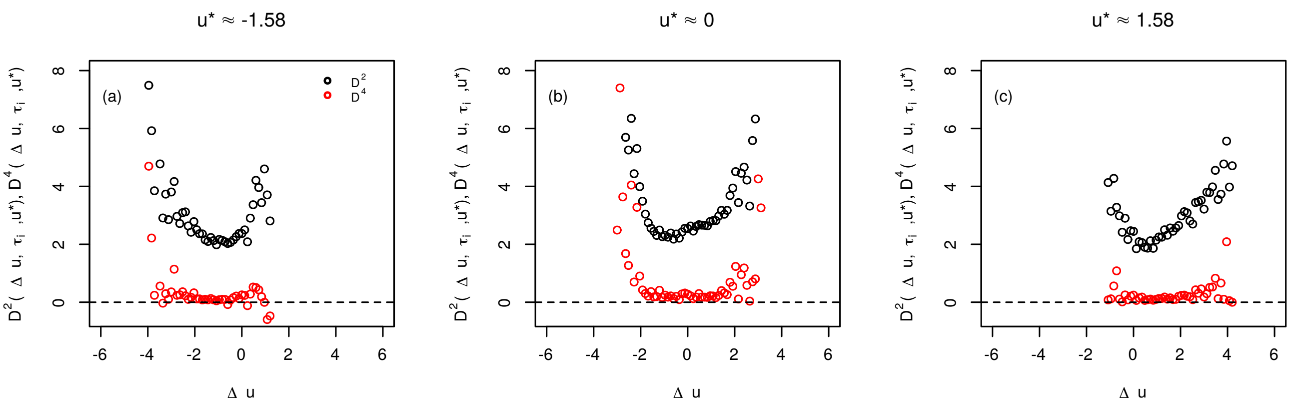

Figure 3. Exemplary estimations of the second and fourth Kramers-Moyal coefficient $D^{(2)}$ and $D^{(4)}$ for $\tau = 65\ s$

Page 5, line 118: I disagree with the statement that 'it can be easily seen'. Provide more details.

We added are more detailed explanation in the revised manuscript.

Page 6, line 139: The authors only check the validity of equation 15 for a single choice of the difference time tau = 1s. Given the potential of long-range correlations or eddies in the flow, this expression should be checked for several choices of tau.

The validity of equation 15 has been checked for a time scale separation $\Delta\tau_{EM} = 0.1\ s$, being the Einstein-Markov length for this data set, and as well for $\Delta\tau > 0.1\ s$, namely $\Delta\tau = 10 \cdot \Delta\tau_{EM} = 1.0\ s$ (see fig. 2) shown within our paper. As described in the beginning of the method section, the wind speeds $U(t)$ are transformed to quasi-stationary wind speed fluctuations $u(t)$, thus long-range correlations (longer that 1 min) will be eliminated by that procudure.

Page 7, figure 1: The isoline heights are hard to read.

Changed accordingly.

Page 7, line 156: Introduce abbreviations.

Added accordingly.

Page 8, line 160: What is d_{10}, etc?

The polynomial coefficients $d_{ij}(\tau_i, u^*)$ are themselves higher-order polynomial obtained from fitting the the third and second order polynomial coeffients over all considered τ_i and u^*. We explicitly obtained following polynomials (and added them to the appendix of the revised manuscript):

$$d_{10} = c_{0,d_{10}} \cdot \tau_i + c_{1,d_{10}} \cdot u^* \cdot \tau_i^{\tilde{c}_{1,d_{10}}} + c_{2,d_{10}} \cdot \tau_i^{\tilde{c}_{2,d_{10}}} \cdot u^{*2} + c_{3,d_{10}} \cdot u^{*3} \quad (1)$$

$$d_{11} = c_{0,d_{11}} \cdot \tau_i + c_{1,d_{11}} \cdot \tau_i^{\tilde{c}_{1,d_{11}}} + c_{2,d_{11}} \cdot \tau_i^{\tilde{c}_{2,d_{11}}} \cdot u^{*2} \quad (2)$$

$$d_{13} = c_{0,d_{13}} \cdot \tau_i^{\tilde{c}_{0,d_{13}}} + c_{1,d_{13}} \cdot u^* \quad (3)$$

$$\gamma_{D^{(1)}} = c_{1,\gamma_{D^{(1)}}} \cdot \tau_i^{\tilde{c}_{1,\gamma_{D^{(1)}}}} \cdot u^* \quad (4)$$

with $c_{0,d_{10}} = -0.006$, $c_{1,d_{10}} = -0.888$, $\tilde{c}_{1,d_{10}} = 0.098$, $c_{2,d_{10}} = 0.137$, $\tilde{c}_{2,d_{10}} = 0.019$, $c_{3,d_{10}} = -10.566$, $c_{0,d_{11}} = -1.656$, $c_{1,d_{11}} = -0.018$, $\tilde{c}_{1,d_{11}} = -8.853e-05$, $c_{2,d_{11}} = -0.268$, $\tilde{c}_{2,d_{11}} = 1.671$, $c_{0,d_{13}} = -0.005$, $\tilde{c}_{0,d_{13}} = 0.012$, $c_{1,d_{13}} = 1.023$, $c_{1,\gamma_{D^{(1)}}} = 0.341$, $\tilde{c}_{1,\gamma_{D^{(1)}}} = 0.247$.

And for the diffusion function:

$$d_{20} = c_{0,d_{20}} \cdot \tau_i^{\tilde{c}_{0,d_{20}}} + c_{1,d_{20}} \cdot \tau_i^{\tilde{c}_{1,d_{20}}} \cdot u^* + c_{2,d_{20}} \cdot \tau_i^{\tilde{c}_{2,d_{20}}} \cdot u^{*2} \quad (5)$$

$$d_{21} = c_{0,d_{21}} \cdot \tau_i^{\tilde{c}_{0,d_{21}}} + c_{1,d_{21}} \cdot \tau_i^{\tilde{c}_{1,d_{21}}} \cdot u^* \quad (6)$$

$$d_{22} = c_{0,d_{22}} \cdot \tau_i^{\tilde{c}_{0,d_{22}}} + c_{1,d_{22}} \cdot \tau_i^{\tilde{c}_{1,d_{22}}} \cdot u^* \quad (7)$$

with $c_{0,d_{20}} = 0.024$, $\tilde{c}_{0,d_{20}} = -0.0001$, $c_{1,d_{20}} = 0.0002$, $\tilde{c}_{1,d_{20}} = 1.076$, $c_{2,d_{20}} = 1.573$, $\tilde{c}_{2,d_{20}} = 1.622$, $c_{0,d_{21}} = 0.002$, $\tilde{c}_{0,d_{21}} = -0.001$, $c_{1,d_{21}} = 1.104$, $\tilde{c}_{1,d_{21}} = 1.395$, $c_{0,d_{22}} = 0.042$, $\tilde{c}_{0,d_{22}} = 0.002$, $c_{1,d_{22}} = 0.555$, $\tilde{c}_{1,d_{22}} = 0.364$.

Page 9, figure 3: On Page 7, line 156 the authors write that the drift and diffusion terms should be polynomials of order 2 and 3. Provide fits in figure 3 to illustrate this.

Added accordingly.

Page 9, equation 19: Do not put ‘exp’ in italics.

Changed accordingly.

Page 10, figure 4: The black-on-black isoline notations are virtually impossible to read.

Changed accordingly.

Page 10, figure 4: The math in the right panel does not look convincing. Can the authors comment on this?

As the estimation of the conditional pdfs is purely based on measurement data, sparse regions in the phase space will naturally result in poor estimations of pdfs. This is of course a major problem for our method, as it relies on those pdfs. With the plot in the right panel we wanted to show that we now can extrapolate conditional pdfs to sparse regions via numerical solutions of the Fokker-Planck equations. Still, even though the estimated conditional pdf suffers from a low availability of data, our numerical solution appears to be a reasonable representation of the true pdf. To make our approach clear, we added in the manuscript the comment: ’Note that due the use of the FPE, the obtained pdfs are less noisy and extend to large values as seen in fig. 4’

Page 13, line 227: Typo: replace sigma with “
sigma.”

Changed accordingly.

Page 13, line 227: Why can the coefficients D be considered slowly changing functions? Figure 7 seems to show the contrary.

In Figure 7 we show the timeseries of the wind speed fluctuations $u*$, which are indeed fast-changing. Also the underlying conditional pdfs $p(u^*|u_1;...;u_N)$, shown to the right, are fast-changing, due to different values of u_1 , ..., u_N. It is a central point of our multipoint approach that the underlying Fokker-Planck equation, defined by its coefficients $D^{(i)}$, is not changing for this interval, or, respectively, is slowly changing due to the non-stationary nature of windspeeds. This slow change takes place on large time scales for which the mean values like $\overline{U}$ are defined. In the same sense the joint pdfs $p(u^*;u_1,...;u_N)$ are changing only slowly, while the fast-fluctuating conditional pdfs $p(u^*|u_1;...;u_N)$ are generated by the fast-fluctuating arguments in the joint pdf. The basic difference is based on the aspect one is looking at. The signal is fast changing, but the underlying equation is fixed. Fast changes are generated by fast-fluctuating arguments of a slow-changing function.

Page 14, figure 8: Consider splitting the top panel into two. Through the overlapping curves, a lot of detailed information is getting lost.
Page 15, line 248: RANS should be spelled out.
Page 15, line 249: ‘Great’ is an odd choice of word.
Page 15, line 255: Consider rephrasing the sentence. Not all multipoint-based models automatically capture small-scale intermittency.
Page 15, line 265: Do not use capitals in the reference. Page 15, line 267: Capital ‘A’ in acknowledgments.

All changed accordingly.

Page 15, line 269: Is the first name of the person Andeé?

No, thank you for pointing this out.

Page 16 References: A lot of the references are inconsistent when it comes to providing doi, placement of first name letters and abbreviation dots.

Changed accordingly.

References

Boettcher, F., Renner, C., Waldl, H.-P., and Peinke, J.: On the statistics of wind gusts, Boundary-Layer Meteorology, 108, 163–173, https://doi.org/10.1023/A:1023009722736, 2003.

Morales, A., Waechter, M., and Peinke, J.: Characterization of wind turbulence by higher-order statistics, Wind Energy, 15, 391–406, https://doi.org/10.1002/we.478, 2012.

Peinke, J., Tabar, M. R., and Wächter, M.: The Fokker–Planck Approach to Complex Spatiotemporal Disordered Systems, Annual Review of Condensed Matter Physics, 10, 107–132, https://doi.org/10.1146/annurev-conmatphys-033117-054252, 2019.