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Interactive comment on "Multipoint Reconstruction of Wind Speeds" *by* Christian Behnken et al.

Anonymous Referee #1

Received and published: 6 March 2020

In their manuscript 'Multipoint Reconstruction of Wind Speeds', the authors develop a multi-point stochastic algorithm that is capable to generate synthetic wind speed time series. This is similarly done via the use of field data and the support of a Fokker-Planck framework alike. The method presented is sound, and applications are wide-ranging in the design and analysis of wind farms and fields. As an extension, the authors also deal with the non-stationarity that is typical for real-world wind speed fluctuations. Overall the paper is a welcome addition to the literature and shows promising results. However, its technical presentation doesn't give the promising results enough credit and is dragging the quality of the manuscript down. I would not suggest publication in its current state, but with a bit of additional effort, this should be redeemable. Concerning this, I do have a series of comments on the manuscript that should be ad-

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Discussion paper

dressed. Once they have been properly answered, I would suggest the manuscript for publication.

General Comments:

Overall language and grammar: The comma placement, hyphenation, and grammar need improvements. The authors should give the text another round of fixes to ensure the proper use of language.

Section 1 through 2.1: In the introduction and methods section, the authors build upon the senior's authors work in Nawroth et al (2006) for turbulent flows. The authors do give the reference, but it should be made clearer which parts are new and which parts are a simple reproduction.

Font sizes and font styles are inconsistent between the figures and the text. This should be fixed.

Specific Comments: 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.

Page 2, line 28: Define what 'short time scales' means and how it relates to intermittency.

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

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

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).

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

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

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.

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

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.

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

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

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

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

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

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

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

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

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.

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

Page 7, line 156: Introduce abbreviations.

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

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.

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

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

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

Page 13, line 227: Typo: replace sigma with \sigma.

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

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.

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

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

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