This paper presents a methodology for generating wind speed fluctuations. The methodology is well described, and the paper is overall well written. However, this reviewer has a list of specific comments, including some major comments, that should be addressed before this manuscript can be accepted for publishing.

Specific comments:

1. 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.
2. L15. Why wind(solar) is capital in the manuscript?
3. L25. The citation style is incorrect. Please revise accordingly.
4. L28. Should be "…known to be…"
5. 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.
6. 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.
7. L46. Remove one "and" at the end of this line.
8. L60. It should be specified that t is the time.
9. L61–62. Please revise the sentence for proper English.
10. L60 and L65. Please clarify the difference between u(t) and U(t).
11. The abbreviation pdf is sometimes italicized and sometimes not. Please be consistent.
12. L110. There should be a comma after the Pawula theorem. Also, please provide a reference for this claim on L110 and L111.
13. L155–156. Why the order of the polynomial of 3 and 2. Is this the lowest polynomial order that properly fits the data?
14. 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.
15. L167. Please correct the English.
16. All figures. Please add (a), (b), (c), etc. labels for subplots.
17. L179. I belie that "an" should be "a".
18. Equation 19. The function exp should not be italicized. The same holds for any other function in the manuscript.
19. L223. The word min should not be italicized.
20. 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.

21. This change (previous comment) would perhaps correct for the discrepancies between the measurements and the reconstruction in Figure 8 (pdfs).
22. 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?
23. 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?
24. 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.
25. 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?
26. L238. The phrase “a fairly nice match” is not scientific. Please be specific.
27. 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?
28. L252–L253. Not necessarily until the method accounts for the spatially coherent fluctuations.
29. 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.
30. 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?