Overview

This paper introduces Gaussian mixture model for extreme wind turbulence estimation and applies it to a 15-year period of measurements. A discussion related to IEC standard turbulence is also included. Overall, I think the manuscript is well written and an interesting study. I have several suggestions and comments that I expect to be addressed.

Specific comments

1.   The Abstract should include the main findings of this study. You explain the research question and state that you carried out different comparative analyses between different statistical models, but it is never mentioned in the Abstract what the main finding of these analyses is. That should be added in the revised manuscript.

2.   Lines 42–43. It's common to cite papers as *Monahan (2018) model the joint*… instead of *(Monahan, 2018) model the joint*… The latter formatting is used when the references are listed to support a statement. This inconsistency is observed throughout the manuscript.

3.   Line 52: Is the provided order of $10^{(-7)}$ authors' assessment of there is a support for this value elsewhere in the literature?

4.   Line 61: Can the authors provide one more paragraph that will summarize the outcome of other studies that used GMM? Since the claim in Line 61 is that only few studies exist that used this method, it would be interesting to what were their findings and is it relevant to the present work?

5.   Line 74: The should be the (without capital t).

6.   Line 83: So, what is the value of k in this study? The authors proceed with explaining the theoretical framework of this model, but the reader is not provided with the information about k. And how is that value of k justified or determined? Update: I see now that the value is provided after Table 1, but this should be discussed even earlier so that the reader is not confused by the time it gets to Table 1.

7.   Line 86: Can you provide a reference (book or paper) for the 3-step estimation of model parameters that is described after Line 86?

8.   Section 3.1. Is there any study on homogeneity of wind data from this tower? One would assume that anemometers were re-calibrated and/or replaced and the environment around the anemometers has changed over the 15-year interval. This can introduce systematic biases to wind data. Can the authors comment on this?

9.   Lines 142, 144 and elsewhere: Use power notation for m/s and other units.

10.   Is it possible to further explore this method to separate turbulence associated with thunderstorm winds (downbursts and various gust front outflows) vs. severe non-thunderstorm winds. The former class of winds is characterized by non-Gaussian distribution of fluctuations as well as mean wind (i.e., mean wind is not constant over a 10-min period). See Hangan et al. (2019; J. Fluid Struct. Doi: 10.1016/j.jfluidstructs.2019.01.024) for further discussion.

11.   While I am not sure how to provide a sudden to improve what I am about to say, after reading this manuscript several times I have a feeling that the authors could have made better job of connecting the observational data and the proposed methodology. An example is Section 3.2., i.e., what is the main message of this section? Why randomly sampling those data when the authors later present real observations?

12.   One can perform a similar analysis to what is done in this paper by using Monte Carlo simulations where the random numbers are generated from (observed or assumed) wind distributions with the constraint that the generated numbers (fluctuations) need to obey turbulence

energy spectra. Then, one can estimate turbulence and other statistical parameters from the generated data. What are some of the positives (and perhaps negatives) of the method proposed herein in respect to the simpler Monte Carlo simulations?

13.   Line 253: How computationally expensive is this method? How much computational time was required to perform this analysis?