Review of "Applying a Random Time Mapping to Mann modelled turbulence for the generation of intermitten wind field" by K. Yassin *et al* submitted to Wind Energy Science

Jakob Mann

August 2022

General remarks

The authors propose and investigate a modification to a three-dimensional turbulence inflow model, the so-called Mann model. The modification the authors propose is motivated by an analysis that shows that velocity increments (differences over relatively short time scales) appear not to be Gaussian as it is implied in the Mann model. This is called intermittency. The way to authors do the modification is to randomly stretch and compress the time axis without changing the overall or average progression of time. This has been done previously for a one-dimensional field but never, to my knowledge, to a three-dimensional field. The authors finally test the impact of their modified fields (Time-mapped Mann fields) relative to unmodified fields on wind turbine loads. They show the standard deviations of four different loads are essentially unchanged while the increment statistics differs. The kurtosis of the increments of three out of four loads are increased albeit not as much as the velocity field itself.

Although the research as such is original, the are several severe issues with the paper. Let me summarize those in the following points:

1. The motivation for intermittency (figure 1) is misleading.
2. The modification to the Mann model is quite nonphysical.
3. The conclusion that the spatial structure of the turbulence (understood as the second-order statistics) is unchanged by the modification is flawed.
4. Some of the simulation results are hard to understand and are not well explained.

Point 1

I might misunderstand the background for figure 1, so I'll summarize it here. The plot is compiled from data where the 10-minute average wind speeds ranges from 5 to 15 m/s and the turbulence intensity from 5% to 25%. Imaging that you only have one wind speed but that the turbulence intensity is 5% at night and 25% during the day, and during these periods the turbulence is perfectly Gaussian. Now you calculate the increment pdf which for both night and day are Gaussian with the night pdf being much narrower than the day pdf. But when you add them, they total pdf will become non-Gaussian with a positive kurtosis excess. The situation become more complicated with you included varying mean wind speed, but the example illustrates that you can get small-scale intermittency simply by combining Gaussian distributions with different widths. This is surely not what we are after. I wonder if one takes a long, stationary chunck of the data and do the same analysis whether you get strong kurtosis at all.

Another more puzzling point is the equation for the increment

$$v_{meas} = \sqrt{u_1^2(\boldsymbol{x}, t+\tau) + u_2^2(\boldsymbol{x}, t+\tau)} - \sqrt{u_1^2(\boldsymbol{x}, t) + u_2^2(\boldsymbol{x}, t+\tau)} \qquad (1)$$

appearing in the text on page 6. So according to the definition the authors take the length of the fluctuating part of the horizontal vector and subtract that at times separated by τ. This is a very strange procedure. Once could understand if one took the length of the total vector in which case the square root would be roughly equal to $U_1 + u_1$ (see L. Kristensen, J. Atm. Oc. Tech. 1998). As it stands now, any perfectly joint Gaussian u_1 and u_2 process would give a kurtosis excess of v_{meas}.

I think the motivation section should be improved answering these critical questions.

Point 2

The mathematics of the modification of the Mann model is quite understandable. However, it is not very physical. For example, why it the focus only on the kurtosis? The skewness remains zero in the modified field although this is the property that is known to be non-zero according to Kolmogorov 1941. The method modifies the intermittency in the x_1 direction but it remains perfectly Gaussian in the transversal direction, so the procedure introduces a small-scale anisotropy that there is no experimental evidence for. The resulting field becomes compressible (maybe it doesn't matter to much for loads, but it is a bit unphysical). The authors are only using isotropic turbulence which is far from what is observed in the atmosphere where Γ is usually between 3 and 4. The explanation for this on page 26 is not convincing. Is it difficult to generate a $\Gamma \neq 0$ field and then apply the time mapping? A relatively simple and more physical method to generate fields with non-Gaussian increment was presented by Rosales and Meneveau, Phys. Fluids 2006 which I think should be discussed as well. The method of Berg et al (2016) is also more physical and they see small effect on the damage equivalent loads. Please discuss what is physical and what is not.

Point 3

Coherence (figure 7) is a second-order statistics and it certainly changes a lot. (Please don't show all the irrelevant scatter in the plots, just the smoothened coherences.) I don't think this large change in coherence towards a much more pointed shape has been observed anywhere in atmospheric measurements while the theoretical von Karman coherences have been verified for small separations at several occasions. Please comment on this. Since the two-point cross spectra changes so drastically you should also expect the auto-spectra to change (figure 6). This is obscured in figure 6 in the way the spectra are treated and plotted. First of all, it is costumary, and with good reason, to plot the pre-multiplied spectrum ($\kappa_1 F_1(\kappa_1)$) because it makes it easier to see how the variance is distributed on frequency. Secondly, please do bin averaging so you plot an equal number of power spectral densities per decade. In this way it is possible to see the differences between the conventional and time-mapped spectra. Also regarding figure 6, I think it is totally unphysical to assume uncorrelated time mapping at every point and it obviously give nonsensical $F_2(\kappa_2)$ and $F_3(\kappa_3)$ spectrum. There are no reason to show these.

Point 4

There seems to be no good explanation on the very non-intermittent behavior of root-flap moment in figure 10. It is also hard to understand the behavior of the kurtosis in figure 11. Why do you see very regular peaks at τ equal to interger seconds?

Some specific remarks

1. In the introduction, which is nice, it would be great to be more specific on what the different studies show. Please state how large are the difference in percent instead of stating "very close", "agree quite well", "are different from" wherever it is possible.

2. Is there any physical reason for the choice the distribution of time increment maybe related to the fact that it is α-stable? Eq (17) seems to miss "p()" on the left hand side.

3. Smaller language issues. l 37 "realistic" → "realistically", l 110, I think it is more correct to use "componennt" instead of "direction", l 112 "statistics are" → "statistics is" (also l 319), l 115 "independent from" → "independent of", l 127 "*coh*" should be "coh", l 225 A distribution is not delta correlated but it can have a delta destribution.

4. Eq (13) A square is missing on the κ after the Kronecker delta symbol.

My conclusion

This paper contains new and original research. However, many changes are needed in order to get it up to the required scientific standard.