

Review comments to the manuscript WES-2025-7 “Can the Mann model describe the typhoon turbulence?”

This study tries to investigate the turbulence characteristics during tropical cyclones by analyzing the measurement data. The analysis of the measurement data is detailed and there seems no problems in the analysis. However, there is a big jump in logic between the facts that the authors show and the conclusions. The authors conclude that the gap between the measurement data and Mann model is caused by the characteristics of tropical cyclones but the authors does not show enough facts to justify this conclusion. The authors should show the difference of the spectrum from measurement data during tropical cyclone and other strong wind causing events to discuss the differences between the tropical cyclones and other strong wind causing events (e.g., extratropical cyclones), If not, the discussion in this paper seems almost meaningless and it is difficult to accept this manuscript for publication. The reviewer would suggest the authors to rewrite the manuscript considering the following comments.

Followings are more specific questions. Some of them are related to the point mentioned above and the other comments are smaller points.

1. Line 110. Is it typical that de-spiking is needed for sonic anemometer measurement? Is it only for extreme wind events? Anyway, in this case the authors should show some examples of de-spiking by showing time history of the original data and filtered data to justify their filtering.
2. Line 135. How was the value of U in the Taylor hypothesis calculated? Is it the average wind speed for each 30 minutes?? However, can it be justified for the case when the wind speed changes a lot, which is typical when tropical cyclone is approaching?
3. Fig 4-7. What is the source of the paths of the typhoon? I would like authors to provide the source of these paths.
4. Fig 4-7. The discussion on the low wave number side of the spectrum is little unclear. If It seems that some of the spectrum (u or v component) shows fairly good agreement with Mann model, and it would be difficult to say that Mann model does not work for tropical cyclones. Even at the inner part (e.g., front inner of Hagpit), the fit to Mann model is not bad. If the authors would like to say that these low wave number characteristics are

specific to tropical cyclones, then the comparison with non-tropical cyclone events are needed, which is missing in the current manuscript.

5. Line 388- The connection between the measurement data analysis in section 3 and the analysis of WRF simulation is little unclear. The wave number range in Figure 11 and Figures 4-7 are different and the reviewer have difficulty to understand the connection between them. More clarifications are needed.
6. Figure 11, What are the cause of the “roll”? Obviously, it cannot be concluded from the facts provided in this paper that this is specific to tropical cyclones. As is clear from figure 11 (a), the roll is visible on the left side of the tropical cyclone, where wind is northerly, and maybe the land affects the forming of this roll. On the other hand, on the right side of the tropical cyclone, the roll is not visible, maybe because of the southerly wind from the sea. But if so, this phenomenon has little connection with tropical cyclones and similar phenomena could also be visible in the other strong wind events. If authors would like to point out that the roll and associated large power in low wave number regions, the comparison with non-tropical cyclone events are needed.
7. Figure 11 and figure 5. The centre of the tropical cyclone seems to be at 22 degrees N and 115 degrees E at 0300UTC on August 22nd in figure 11. However, in figure 5, the path does not pass this point. Is it due to the error in the WRF simulation? or is it the error in the tropical cyclone path analysis? Anyway, comments from authors are needed.