Review of "Super Typhoons Mangkhut (2018) and Saola (2023) during landfall: comparison and insights for wind engineering practice" by Liu et al.

This paper is an analysis of wind characteristics of two super typhoons Mangkhut and Saola, aiming to draw relevant insights for wind engineering. This paper is motivated by the absence of super typhoons characteristics in current engineering models, as these models are primarily based on common typhoons. The motivation is good, but the authors fail to address the question posed in the introduction. The emphasis of the paper is on the wind profiles and the gradient winds. The authors claim that low-level jets (LLJs) are identified, but LLJs are a well-documented and common feature in typhoons. The authors suggest that the standardization model can be used to calculate the wind speed of super typhoons, but this merely validates the performance of existing engineering models rather than provide new insights. The paper is also not well written. The lack of key details/definitions and enormous typos make it difficult for the reviewer to follow the study.

The reviewer cannot recommend publication in its current form, as it needs substantial revisions. The authors have to think carefully what they can offer, e.g., which aspects of super typhoons are absent in current engineering practice. Below some suggestions are listed that may help improve the manuscript.

Major comments:
1. There are lots of studies on wind characteristics of these two super typhoons. The authors should cite them.
2. Line 161: Why the Dvorak technique is mentioned but not used in the manuscript? Why no details of the DL method are presented? Why Figure 4 is needed as intensity is already provided in Figure 1? I suggest to remove Figure 4 as it does not provide extra information. Are there any other variables can be provided using the authors' DL method?
3. What do the yellow and blue areas in Figure 6 indicate for?
4. What is $\Delta p_r / \Delta p_c$ in Figure 7? The authors should define these two variables in the main text.
5. Lines 242-243 & 248-249: The authors should cite references to support these lines.
6. Line 252: this paragraph is on Mangkhut, instead of Saola, and there is no second landfall of Mangkhut.
7. Figure 9a needs a colorbar. Why is U-r panel in Figure 9a different from 9b? Can authors provide a U-r panel with observed data for Figure 9a?
8. The three portions of Figure 12 are divided somehow arbitrarily. Can authors provide reasons for this choice?
9. Lines 341-343: A substantial body of literature has already documented the presence of LLJs in typhoons. The authors should cite some of them.
10. Can authors provide a detailed derivation for Eq. (2).
11. Line 360: can authors provide details of the standardization method?
12. Eq. (2): friction due to turbulence should be considered when calculating gradient winds in boundary layers.
13. Line 377: the large gradient winds should be related to pressure gradients instead of LLJ.
14. "modelled wind" is not accurate in section 4.2 as the standardization method is also essentially modelled wind.

15. Lines 376-397: What is super-gradient wind, how does this relate to LLJ, why is gradient wind suddenly related to vertical velocity? The reviewer does not see the logic in these lines.
16. There are a fair number of typos in the manuscript. Suggest the authors have an editor go over the paper. I list some of them below.

Technical comments:

1. Line 29: The first letter of "the" should be capitalized.
2. Line 36: "there conditions" must be grammatically wrong.
3. Line 40: The first letter of "research" should be capitalized.
4. Line 91: What do the authors mean by "Saola got much closer to the Southeastern coastline of China than Mangkhut before making landfall,"? Is the track of Saola closer?
5. Lines 157-159: there must be some typos, given the authors said Saola has an eye but no eye is observed.