Firstly, the research on "the spatiotemporal distribution map of the power-law exponent of the inertial sub range" proposed by the author team in the manuscript is worthy of recognition. Secondly, the author team has provided good solutions for the review comments. Here, I think there are some modifications and improvements that can better present this article to everyone. So my suggestion is that after minor revision, it can be published. The specific suggestions are as follows:

1. In the abstract section: I think these statements are worth discussing, "Here, we propose a method for …proving the superiority of our method.". Firstly, based on facts, explain what methods were proposed, what actions were taken, and what problems were solved. Please do not exaggerate excessively; Secondly, these few sentences are relatively long and can be difficult to understand, leading to confusion.
2. In the Instruments and Data Quality Control section: We all know that the definition of the boundary layer is the part of the troposphere directly affected by the ground, with a time scale of 1 hour. If you set the comparison sample time to 30 minutes here, I think many turbulence effects will be averaged out severely. The implication is that beyond the freezing time of atmospheric turbulence, we know that turbulence has a time period of 10 seconds to 10 minutes. The fluctuation of wind speed is also like this, which is described in detail in the reference you mentioned (Stull, R. B.1988.). I think the author's description here may still be a bit confusing. Although you compared the fluctuation between 30 minutes and 5 minutes with a difference of less than 0.3, is this fluctuation too large for turbulence? Implicitly, you don't have convincing evidence to suggest that this 0.3 has little effect on turbulence, and these are only under clear sky turbulence conditions. Also, will a 30-minutes average result in relatively small changes in turbulence? These require careful and rigorous thinking from the author and their team.
3. I don't quite agree with the analysis of Figures 9 and 10, mainly because the author directly summarizes this difference as: " This is because under different horizontal wind directions, the results of the U and V components are more susceptible to interference from the gradient tower itself, while the vertical wind speed (W) is not affected by this, thus yielding better results." I think this is not rigorous. Based on the comparison provided by the author (Figures 7 and 9), the turbulent kinetic energy is not 100% consistent. Does this mean that the method can also lead to errors? Is there any other possibility?
4. The author has an error in the abstract and conclusion that needs to be corrected. The long-term comparative observation in Figure 10 is no longer a month.
5. I have carefully reviewed the manuscript of the author team and I would like to raise a question here. When introducing the method, the author mistakenly treated the kinetic energy dissipation rate ε as a fixed constant, which is incorrect. Usually, this method introduces errors, which are not easy to evaluate. This is when I see significant differences in turbulent flow energy in Figures 3, 7, and 9, the power-law exponent does not follow at this moment. Of course, this viewpoint does not affect the idea that this article is a good one, but we cannot ignore this issue. Please reflect the answer to this question in the theory, results and discussion, which will better reflect the author's scientific research attitude.
6. In addition, there are some normative technical issues: for example, the legend in Figure 9c only has wind lidar; For example, in Figure 10, the vertical coordinate units should be

kept as consistent as possible with the previous ones; The y-axis in Figures 3, 7, and 9 should be changed to “Power-law exponent”.