Response to the RC1

(egusphere-2024-1532)

We sincerely appreciate the referee for these valuable comments on our manuscript entitled "Simulation performance of different planetary boundary layer schemes in WRF V4.3.1 on wind field over Sichuan Basin within "Gray zone" resolution"(egusphere-2024-1532). These comments are all valuable and helpful for improving our article. According to the referee's comments, we have made extensive modifications to our manuscript to make our results convincing. In the following point-to-point response, the comments from RC1 are in black font, and our replies are in blue font. We hope our answers clarify the referee's concerns.

General comments:

The manuscripts describes the results from WRF simulations over the Sichuan Basin, because the wind modelling is poor over this area. This is a interesting topic that warrants further investigations. My main concern with the paper is that the description of the measurements is missing. Effects of flow distortion on wind speed can be significant and should be described thoroughly. Technical specifications of the cup anemometer are not given. What is the observational uncertainty of the measurements? Are they regularly calibrated in a wind tunnel? Is a calm threshold applied for the wind vane? Furthermore, the local terrain effects will usually dominate wind speeds and direction measured at 10 m, which are not described at all in the manuscript (what is the local surface roughness etc?). In addition, there is unclear descriptions (see for example comment related to classification of "cold air" and "deep convection") and smaller technical issues. The authors have to convince the reader that the measurements are suitable for addressing a certain scientific question and relate the simulations to the specific research question. Finding the PBL scheme that can 'best' represent the wind distribution at one mast, is not so useful if a mast a kilometer away would lead to completely different results.

Response:

Thank you for your valuable comments and insightful suggestions on our manuscript. The wind direction and speed instruments installed at Guanghan Airport are primarily used to collect wind field data in support of flight operations. The International Civil Aviation Organization (ICAO) imposes stringent requirements on the collection, calibration, and quality control of meteorological data to ensure the accuracy and precision of wind measurements. Detailed information regarding wind measurement can be found in the following response to "Specific Issues". With regard to the question the referee mentioned at the end, we acknowledge the limitations highlighted by the referee, particularly the potential variations in results across different locations, which is a crucial consideration in wind speed simulation research. Nevertheless, we would like to elucidate the rationale and significance of our study from the following perspectives.

1. Simulations of wind speed at single site are frequently utilized to validate the performance

of numerical models in numerous scenarios(Denis et al.,2020). By selecting representative sites with high-quality observational data, valuable references can be provided for enhancing and optimizing PBL schemes. Besides, the observations from a number of stations are compared to the model output of wind speed and direction at the nearest grid point to each station (Gómez et al.,2015).

2. In practical applications, single-site wind speed simulations are frequently employed to fulfill specific engineering requirements. In such contexts, accurately simulating the wind speed distribution at a critical location holds direct practical significance.

3. Our objective in this study is not to ascertain a universally optimal PBL scheme applicable across all regions, but rather to assess the efficacy of different PBL schemes in specific locations within distinct geographic and climatic contexts, for instance, the western Sichuan Basin, and strong wind processes. This approach not only facilitates a more profound comprehension of the constraints and benefits associated with particular schemes, but also furnishes essential foundational data for subsequent multi-site or regional investigations.

References:

Gómez-Navarro, J. J., Raible, C. C., and Dierer, S.: Sensitivity of the WRF model to PBL parametrisations and nesting techniques: evaluation of wind storms over complex terrain, Geosci. Model Dev., 8, 3349–3363, https://doi.org/10.5194/gmd-8-3349-2015, 2015.

Denis, E.K., Muyiwa, S. A.:A preliminary sensitivity study of Planetary Boundary Layer parameterisation schemes in the weather research and forecasting model to surface winds in coastal Ghana,Renewable Energy, 146, 66-86,https://doi.org/10.1016/j.renene.2019.06.133, 2020.

Specific issues:

L72-L88: For each case study one can find a PBL scheme that does better than the rest. This section should also describe the physical process that cause a certain PBL scheme to do better and should be related the research question in this study.

Response:

Thank you very much for your valuable suggestion. Accordingly, we have revised this part in our manuscript as follows:

"In China, Ma et al. (2014) conducted a series of sensitivity simulations on spring strong wind events in Xinjiang Province using the YSU, MYJ, and ACM2 schemes. The results indicated that the YSU scheme exhibited greater downward transport of high-level momentum, attributed to enhanced turbulent mixing effects. This improvement helps simulate temperature and moisture profiles more accurately during the daytime when convection is dominant (Hong et al., 2006). The YSU scheme has also been shown to be the optimal PBL scheme for simulating 10-meter wind speeds in other regions (Cui et al., 2018; Li et al., 2018). However, in coastal areas like Fujian Province (Yang et al., 2014), studies have demonstrated that the MYJ scheme is the best choice for simulating near-surface wind speeds due to its advancements in calculating turbulent kinetic energy (TKE). The MYJ scheme computes TKE at each level, allowing for a more precise representation of turbulence within the boundary layer, which enhances its ability to model the generation, dissipation, and transport of turbulence (Janjié, 1990; Jaydeep et al., 2024). In the mountainous terrain of Huanghan and Guizhou, ACM2 has demonstrated superior performance in simulating near-surface wind speeds (Zhang and Yin, 2013; Mu et al., 2017). From these studies, it is evident that the performance of a PBL scheme is highly dependent on its ability to accurately represent the key physical processes within the boundary layer across different topographical contexts, leading to significant regional variations in the performance of PBL schemes in WRF."

L102: So the aim of the study is diffusion in stable cases: that should be moved earlier in the introduction and the discussion about the different PBL schemes should be related to it.

Response:

Thanks for your comment, and we apologize for the confusion caused by the sentences here.

In the matter of fact, we bring up the issue of pollutant dispersion here, aiming to emphasize that numerous studies hitherto have concentrated on the pollutant dispersion under stable and weak wind conditions in the Sichuan Basin, but less attention paid to unstable or strong wind events.

We have rewritten this sentence as below:

"Therefore, wind is not still as wildly studied as temperature and precipitation in Sichuan Basin, and numerous studies hitherto have concentrated on the pollutant dispersion under stable and weak wind conditions here, and less attention paid to unstable or strong wind events."

L114: add reference for "grey zone", e.g. https://journals.ametsoc.org/view/journals/atsc/61/14/1520-0469_2004_061_1816_tnmitt_2.0.co_2.xml

Response:

We kindly thanks for your suggestion, we have added this reference in the revised manuscript.

L218: I have never seen the formulas before so at least a reference should be provided. In general, the Weibull A and k should be found by fitting the Weibull distribution to the observed frequency histogram of the wind speeds.

Response:

Thanks for pointing out this.

Indeed, the probability density function (PDF) of the Weibull distribution can be expressed in various forms (Lai et al., 2006). In our manuscript, we calculated the PDF of the Weibull distribution following the approach of Jiang et al. (2015). In response to your comments, we have added two additional references to the revised manuscript.

References:

Lai, C. D., Murthy, D., and Xie, M. : Weibull Distributions and Their Applications, Springer Handbook of Engineering Statistics, Chapter 3. 63-78, 10.1007/978-1-84628-288-1_3, 2006.

Jiang, H., Wang, J. Z. , Dong, Y., Lu, H. :Comprehensive assessment of wind resources and the low-carbon economy: An empirical study in the Alxa and Xilin Gol Leagues of inner Mongolia, China, Renewable and Sustainable Energy Reviews, 50, 1304-1319, https://doi.org/10.1016/j.rser.2015.05.082, 2015.

Section 3: I am missing description of the measurements: what kind of cup anemometer was being used? What kind of wind vane? Was any quality assurance done to make sure the data were adequate for this study. If you are measuring at 10 m the wind speed is totally dominated by the roughness length at the site, so that should be thorougly described and assessed.

Response:

Thank you for your comment.

We fully understand your concern. In response, we have provided detailed information about the cup anemometer in Section 2 of our manuscript.

The wind direction and speed measurements were conducted using the FIRST CLASS three-cup anemometer and wind vane (Figure 1), manufactured by Thies Clima in Germany. The anemometer consists of three cups made from carbon fiber-reinforced plastic, which rotate in response to wind flow. This rotation is photoelectrically scanned and converted into a square wave signal, with the frequency of the signal being directly proportional to the rotation speed.

The wind vane's dynamic characteristics are optimized by its lightweight aluminum structure. The combined action of the wind vane and its counterweight results in a high damping coefficient and minimal delay distance, both of which contribute to the vane's excellent overall performance. The relevant technical specifications are provided in the following table.

Table 1 Technical specifications for cup anemometer and wind vane

Technical Specifications	Description

	Cup Anemometer	Wind Vane
Ranege	0.3-75m/s	0-360°
Starting threshold	<0.3m/s	< 0.5 m/s at 10° amplitude (in accordance with ASTM D 5366-96) < 0.2 m/s at 90° amplitude (in accordance with VDI 3786 part 2)
Accuracy	1% of the measured value or < 0.2 m/s	0.5°
Resolution	0.05 m/s	0.35°

Figure 1 FIRST CLASS three-cup anemometer(left) and wind vane(right)

The anemometer and wind vane have undergone calibration twice a year. Quality control measures such as outlier removal are applied to the collected wind data. Statistical methods to detect outliers or unusual patterns in the data are applied in our study.

Regarding the roughness length mentioned by the referee, we acknowledge the significance of ground roughness length in wind speed research, particularly in weak wind conditions. However, our study primarily focuses on strong wind processes in the Sichuan Basin, where the impact of ground roughness is relatively minimal. Furthermore, we utilized consistent wind direction and speed observation data to assess various PBL schemes, ensuring uniformity and mitigating variable discrepancies attributed to ground roughness. This approach centers on evaluating the performance of PBL schemes themselves(Yu et al., 2023). We believe this methodology can effectively gauge PBL scheme performance during strong wind processes while maintaining analytical simplicity and efficacy. However, it is worth noting that further exploration into ground roughness as a factor holds merit, especially when studying diverse wind speed conditions. Should future research necessitate an examination of PBL scheme performance across varying wind speeds, inclusion of ground roughness analysis will be considered.

According to the referee's comments, we have added the sentence in section 2.1 line 172 as follows:

"The terrain here is flat and homogeneous, and prevailing wind direction are north and northeast in climatology."

L239: A classification should classify a certain variable or process. But cold air is a property of the air, whereas deep convection is related to atmospheric stability. For example, you can have deep convection in very cold air. So this classification does not make sense.

Response:

Well, we really appreciate the referee's insightful comments.

We totally agree with you, that the deep convection occurs in very cold air in some region, and even if the thunderstorm gale processes still have the participation of cold air too. So, we are very sorry for our inaccurate expression. What we intend to clarify here is that the strong winds is caused mainly by convective weather system or non-convective weather system.

Therefore, the term 'cold air' as used in Table 3 denotes the case which is generated by incursion of cold air from northern regions like Siberia or Mongolia in Sichuan Basin, in such process, the cold air forces the warmer air ahead of it to rise rapidly, meanwhile, sharp temperature drop and changes in humidity can be observed. The term 'deep convection' specifically denotes the strong wind cases primarily caused by convective weather systems, often accompanied by thunderstorm. In such cases, the vertical motion or convection is the dominant.

Since the main focus of this paper is not on the meteorological cause of strong wind events in the Sichuan basin, we provide a simplified classification method here to help understand the differences of the performance between various planetary boundary layer (PBL) schemes in simulating strong near-surface winds caused by different meteorological processes. We have been aware of this problem here, accordingly, We clarified the two terms in the manuscript.

<u>Technical corrections</u>

L9: remove unique or specify what you mean with unique and why it is unique.

Response:

We kindly thank you for your suggestion, the points are well taken, we removed the word "unique".

L11: change to "In this study, the Weather Research ..."

Response:

Thanks for pointing out this typo, we have made a correction in the revised manuscript.

L13: I would change to near-surface wind fields because at the surface there is by definition no wind.

Response:

You are right, many thanks for pointing out this incorrect expression, we corrected it in our manuscript.

L15: You mean multiple case studies? Not sure what a multi-case study is.

Response:

Sorry for the error. What we're trying to say is that the study was based on multiple case studies (28 cases), rather than just one case. We have made a correction in our manuscript.

L19: You mean that the wind speed is sensitive to the choice of PBL scheme? In that case rewrite this sentence to make this more clear.

Response:

We really appreciate the referee's comment. We checked this sentence carefully, and rewrote it in our revised manuscript as follows:

"The results demonstrate that the wind direction can be well reproduced, yet it is not as sensitive to the PBL scheme as the near-surface wind speed."

L26: tiny -> small

Response:

Thanks for pointing out this incorrect expression, we corrected it in the manuscript.

L26-29: "However, ... were opposite". This line is unclear, do you mean that the case studies where chosen to be mostly around the morning? If yes, why? What is a evening to evening process? The opposite of what?

Response:

We apologize for the confusion caused by the sentence in the abstract. There are a few key moments that we didn't express clearly. First, we want to clarify that the time appeared in this sentence means the time when observed wind speed exceeded 6 m s^{-1}. Because the 28 near-surface wind events were chosen with a criteria of the maximum wind speed greater than 6 m s^{-1} (L130).

We revised the abstract sentence in our manuscript as follows:

"However, the simulation results for strong winds occurring during the mid-night to early morning hours exhibit poor root mean square errors but high correlation coefficients, whereas for strong wind processes happening in the early to late evening hours and for southwesterly wind processes demonstrate the opposite pattern."

L28: COR, if this is correlation coefficient just write it out. In general, abbrevations should be minimized in the abstract, because the reader does not know what they are at this point.

Response:

Thank you for pointing out this, we have made correction in the manuscript.

L35: "as the most" -> why is it the most fundamental? Temperature or humidity can also be fundamental, rewrite.

Response:

Thank you for your comment, we totally accept and have rewritten this sentence as "Wind, as the one of fundamental natural phenomenon in the atmosphere,......" in our manuscript.

L49: I would say topographic and underlying surface are referring to the same concept. And thermal effects are also dynamic?

Response:

Thank you for your comment.

The terms "topography" and "underlying surface" both refer to aspects of the Earth's surface in many fields, but have distinct meanings and applications in simulation of meteorology.

<u>Topography</u> refers to the physical appearance of the natural features of an area of land, especially the shape of its surface, such as mountains, valleys, rivers, or craters

on the surface. We know that mountains, valleys, and other topographical features can channel and redirect winds, creating localized variations in wind speed and direction.

The underlying surface refers to the physical material and properties present beneath a specific area, including soil type, bedrock, and other substrata.For instance, the type and density of vegetation can affect wind flow near the surface by increasing friction and altering turbulence.

As for the second question, what we intend to express is that, the near-surface wind fields can be affected by the dynamic process such as atmospheric pressure gradients, thermodynamic process including temperature gradients from the aspect of meteorology.

Thererfore, considering your suggestion, we have rewritten this part in our manuscript as follows:

“Near-surface wind fields are influenced by a combination of various factors, including atmospheric dynamic and thermodynamic processes (such as pressure gradient force, temperature gradients, and so on), topography (such as geographical features, elevation), and underlying surface (such as vegetation, land use) (Zhang et al., 2021).”

L116: vortices -> eddies

Response:

Suggestion taken, thank you for your comment.

L132: resolution -> horizontal resolution.

Response:

Suggestion taken, thank you for your comment.

L133: grids -> grid cells

Response:

Thanks for pointing out this typo, we have made correction in the manuscript.

Ll134: 45 what?

Response:

We are sorry for the mistake, it should be 45 levels, and we have made correction in the manuscript.

L138: upstream? The prevailing wind direction hasn't been introduced so the reader doesn't know what is upstream or downstream.

Response:

Many thanks for this comment, we have added the prevailing wind direction information in the manuscript (section 2.1).

L152: reference missing for SRTM3

Response:

Thank you for the comment. We have added the reference below in our manuscript.

Farr, T.G., Rosen, P. A., Caro, E. R., Crippen, R., Duren, R.M., Hensley, S., Kobrick, M., Paller, M., Rodríguez, E., Roth, L., Seal, D.A., Shaffer, S.J., Shimada, J., Umland, J.W., Werner, M., Oskin, M.E., Burbank, D.W., and Alsdorf, D.E.: The Shuttle Radar Topography Mission, Reviews of Geophysics, 45, https://doi.org/10.1029/2005RG000183, 2007.

L160: high -> height

Response:

Thanks for pointing out this typo, we have made correction in the manuscript.

L198: surface -> near surface, see earlier comment

Response:

Thanks you for your comment, we have made correction in the manuscript.

L206: I miss the definition of the overbar/overline.

Response:

Thanks you for your comment , we have made correction in the manuscript.

Fig 2: seies -> series

Response:

Thanks for pointing out this typo, we have made correction in the manuscript.

L265: Using overestimate and underestimate in relation with wind direction is confusing. Rewrite to use a directional metric.

Response:

Thank you for your comment, we have rewritten this part according to your suggestion:

"Besides, it is also shown that the occurrence frequencies of the wind fields simulated by four PBL schemes in the NNE and NE directions are all relatively higher than observation, but for wind in NNW direction, the simulated frequencies are significantly lower..."

Moreover, we added the directional statistical metrics (ME, RMSE and circular COR) for simulated 10-m wind (Table 4).

Table 4. Statistical metrics for simulated 10-m wind direction.

	Average Wind Direction (degrees)	ME(degrees)	RMSE(degrees)	Circular COR
Observations	22.2			
YSU	33.3	12.1	57.8	0.37
MYJ	32.1	12.5	58.9	0.36
MYNN2	36.9	14.2	61.3	0.33
QNSE	31.0	9.8	62.1	0.30