Responses to the Reviewers

We would like to express our sincere gratitude to all reviewers for their insightful and constructive comments. Their valuable suggestions have greatly contributed to improving the clarity and overall quality of our manuscript. Based on their feedback, we have carefully revised and supplemented the manuscript accordingly.

During revision, we realized that several concerns from all three reviewers stemmed from our use of the term “built-up areas” to describe the study area, which may have been misleading. In the original manuscript, this term referred to the following categories from the urban-rural classification dataset of Li et al. (2023): Urban center, Urban landscape, Densely clustered towns, Sparsely clustered towns, Dense villages, Sparse villages, Isolated villages, Residential croplands, Populated croplands, Residential rangelands, and Residential woodlands. However, we realized that “built-up areas” is commonly understood as regions with intensive infrastructure, such as urban, suburban, or residential zones, which does not fully reflect our definition, since some “residential” classes include croplands and woodlands. To more accurately capture the physical characteristics of these surfaces, we have replaced “built-up areas” with “**high-roughness surface areas**” throughout the manuscript, except where the term specifically denotes built-up areas. This revised term better represents regions characterized by large surface roughness, including both urban areas and landscapes with tall vegetation.

The detailed responses to each comment raised by the reviewers are presented in the following sections. The responses are highlighted in blue, and the corresponding revisions in the manuscript are marked in red. We sincerely hope that these revisions address all concerns and meet the reviewers’ expectations.

Reviewer #1:

General Comments:

This manuscript provides a new dataset of roughness z_0 values focused on "built up" areas in China, which covers a large percentage of the country. Their method for generating the z_0 values is by first using meteorological stations and ERA5 to derive site specific z_0 values, then using random forests to calculate a gridded z_0 dataset based on six different inputs (slop variation, terrain standard deviation, percent tree cover, leaf area index, normalized difference vegetation index, and urban-rural classification). They then test their z_0 dataset using a nested WRF setup and find improved agreement with observations compared to the default z_0 values and also a recent z_0 dataset from another study. Overall, the paper is well written, and the method is very clear. I think the method is a nice approach that others could use for their own datasets; however, I do feel that a few minor revisions need to be made before the paper should be published.

Response: We sincerely appreciate your positive feedback and constructive comments. Your suggestions have been carefully considered and have significantly contributed to the improvement of our manuscript. Our detailed point-by-point responses are provided below.

Specific comments:

1. The manuscript's main goal is improving z_0 values in "built up" areas. I'm assuming this is anywhere there is infrastructure, which could be urban, suburban, residential, etc. However, the random forest regression analysis in Fig. 3 demonstrates that that the urban rural classification is actually only the 4th most important input parameter in determining z_0. The two most important inputs are actually just the terrain itself. This, to me, indicates that the most likely reason models are underestimating wind speed closer to the surface is just because the terrain is under resolved with coarser grid spacing. Perhaps there are other reasons, regardless, it is not clear in the manuscript that "land use changes such as urbanization", as stated in the abstract, are the reason z_0

data is not accurate. I believe that the feature importance figure requires additional discussion, and these findings are worthy enough of being restated in the conclusions and the abstract.

The feature importance figure is probably the most important finding that I took away after reading the manuscript. The authors certainly constructed a better z_0 database, but the reason other z_0 databases are wrong seems to not necessarily be because of urbanization.

I think this comment is in line with the "(3) Lack of Resolution-Dependent z_0 Consideration" comment from another reviewer. The authors stated in their response "the horizontal resolution of ERA5 does not affect the estimated z_0 values at individual stations." This is true, but then the gridded z_0 dataset is resolution dependent because the high resolution (100 m?) SRTM data is used as an input to the RFR but then the simulations are done at a much coarser resolution. Whether the other reviewer is satisfied with the authors response is obviously up to the other reviewer but, in my opinion, the most straightforward way to look at the resolution dependence would be to add an additional domain at finer resolution and see if there is improvement even with the default z0 values. Or, considering that the authors already ran a multiscale setup, they could compare results between d01 and d02.

Response: Thank you for your valuable comment. We would first like to clarify a terminology revision. Based on the issues raised by you and other reviewers, we realized that our previous use of the term "built-up areas" was not sufficiently accurate. In the original manuscript, this term referred to 11 categories from the urban-rural classification (*URC*) dataset of Li et al. (2023): Urban center, Urban landscape, Densely clustered towns, Sparsely clustered towns, Dense villages, Sparse villages, Isolated villages, Residential croplands, Populated croplands, Residential rangelands, and Residential woodlands. However, we acknowledge that "built-up areas" is generally understood as regions characterized by intensive infrastructure (e.g., urban, suburban, or residential zones), which does not fully align with our definition, particularly because some "residential" categories include croplands and woodlands. To more accurately describe the physical characteristics of the underlying surfaces considered in this study, we have replaced “built-up areas” with “high-roughness surface areas” throughout the manuscript. In the following, we will respond in detail to your comments concerning (1) the feature importance, and (2) the resolution dependence.

(1) Feature importance

We appreciate your insightful comment regarding the relatively low importance of the *URC* variable in determining z_0. In our study, the *URC* variable primarily served to define and constrain the study areas rather than act as a dominant predictor of z_0. In other words, the inclusion of *URC* was intended to ensure that the random forest model was trained only over high-roughness surface areas. It should be noted that aerodynamic roughness characteristics related to surface morphology, such as vegetation density and infrastructure distribution, are already inherently captured in the CMA station wind observations. In other words, z_0 derived from observed wind speeds already reflects the influence of high-roughness surfaces, while *URC* serves mainly to delineate the spatial coverage of z_0 across different *URC* categories.

The relatively low importance of the *URC* variable is due to the relatively small differences in z_0 among different *URC* classes. z_0 is governed primarily by the morphological height and density of surface roughness elements, whereas the *URC* is not defined by these attributes; instead, it integrates global land-cover and population data (Li et al., 2023), making it weakly sensitive to z_0. Moreover, even in categories of Urban center and Urban landscape, there remains non-negligible tree cover, mean tree fractions of approximately 10% and 11%, respectively (Fig. R1). This lowers *URC*’s ranking in feature-importance analyses. To better capture the influence of roughness elements, more detailed surface parameters, such as building height and building density, would be helpful. Once such data are widely accessible, they should be incorporated to further improve the accuracy of z_0 estimates.

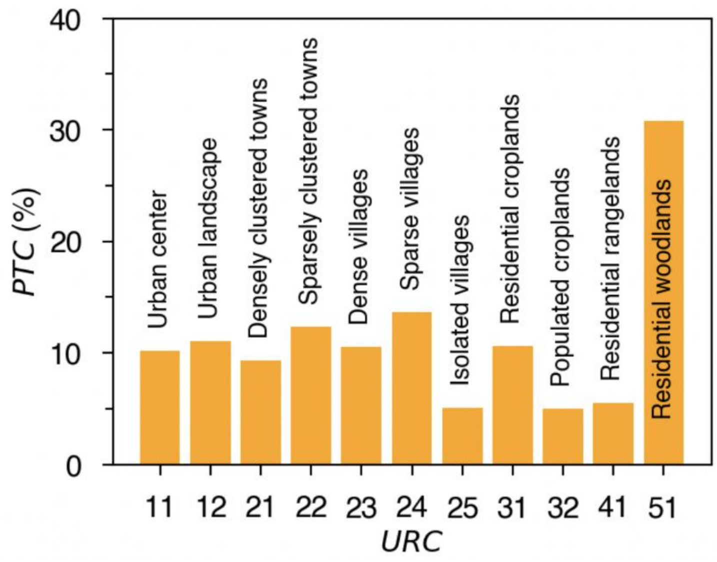

Figure R1. Mean percent tree cover (*PTC*) for each *URC*. The numerical labels on the x-axis represent the *URC* codes, with the specific *URC* types annotated on the bars.

(2) Resolution dependence

The above explanation regarding feature importance also indicates that, even though terrain features rank among the top two important features in the random forest model, it cannot be concluded that the correction of wind speed overestimation in the WRF is due to finer terrain resolution. In fact, the fundamental reason is that the z_0 derived from CMA observations over high-roughness surface can better reflect the influence of these areas. The gridded z_0 data we produced can effectively improve wind speed simulations because the training truth values are more accurate and can better represent the actual surface roughness. This is the central point of our study: we propose a low-cost approach to substantially supplement z_0 truth values. The purpose of z_0 data production and wind simulation validation is primarily to demonstrate the feasibility and effectiveness of this z_0 estimation method. Therefore, the model's overestimation of wind speed still stems from insufficient consideration of the influence of high-roughness surfaces.

We also thank you for the suggestion to demonstrate the resolution dependence by comparing simulations at different resolutions. Following this idea, we compared the simulation results between the d01 and d02 domains. Figure R2 shows the simulated wind speeds using different z_0 datasets with the coarse-resolution (0.09° for d01) and fine-resolution (0.03° for d02) simulations. Overall, the mean wind speeds at both 10 m and 100 m heights are close between the two domains across all z_0 cases. Similar to Figs. 6a/6d and 7a/7d in the manuscript, we further analyzed the performance of the coarse-resolution simulations.

As shown in Figs. R3-R4, z_{0_RFR} still leads to a significant improvement in the near-surface wind speed simulations, even at coarse resolution. However, the improvement magnitude does not show a consistent dependence on model resolution. For example, when using the $z_{0\_Default}$ or z_{0_RFR}, the finer-resolution simulations generally yield lower *RMSE* values of 10 m wind speed compared with the coarser-resolution simulations. In contrast, when using the z_{0_Peng}, the d02 simulation produces a higher *RMSE* than d01 (Fig. 6a and Fig. R3a). Overall, these results suggest that although the z_0 dataset was developed at a 0.01° resolution, it is well suited for mesoscale simulations at kilometer-level resolutions.

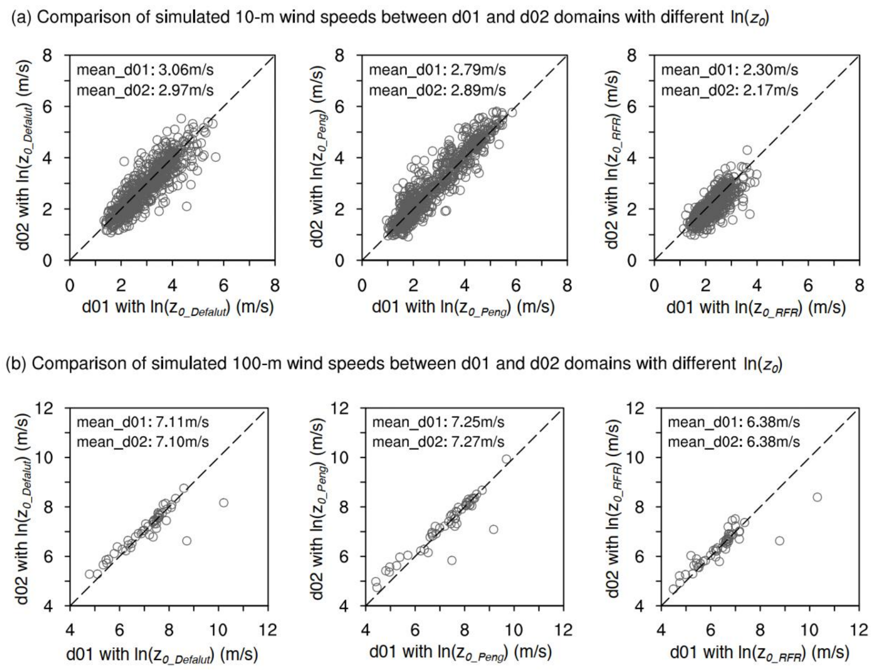

Figure R2. Comparison of mean 10-m (a) and 100-m (b) wind speeds in April between the coarse-resolution (0.09°; d01) and fine-resolution (0.03°; d02) simulations using $z_{0\_Default}$, z_{0_Peng}, and z_{0_RFR}. Each point corresponds to (a) a CMA station or (b) an anemometer tower located within the d02 domain. The overall mean wind speed across all observation sites is also shown.

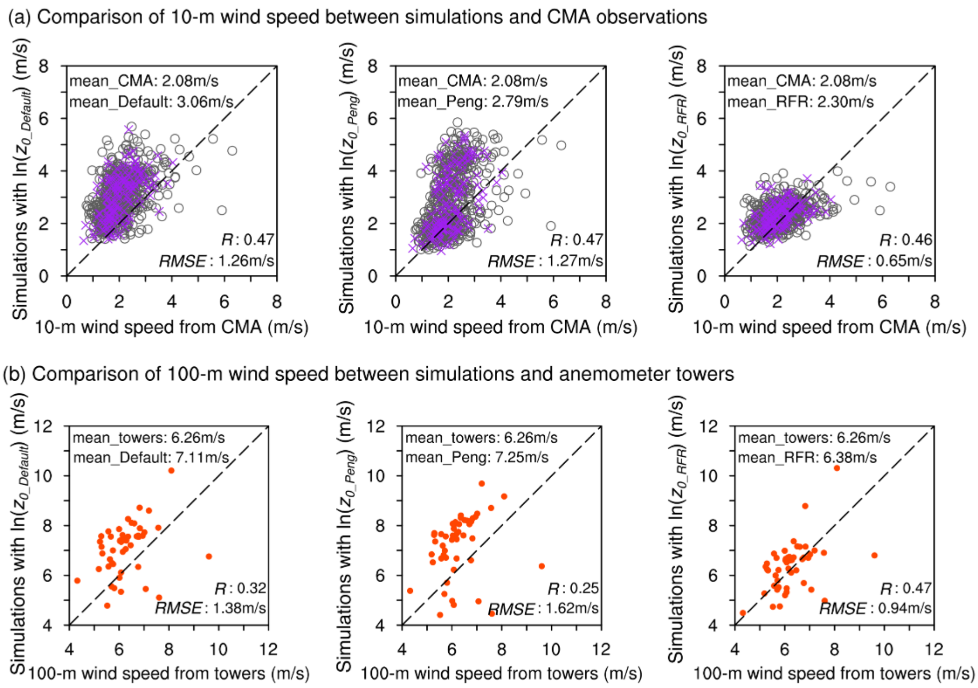

Figure R3. (a) Comparison of mean 10-m wind speeds in April between the coarse-resolution (0.09°; d01) simulations using $z_{0\_Default}$, z_{0_Peng}, and z_{0_RFR} and observations from CMA stations. All points (grey circles and purple crosses) represent the 753 CMA stations within the d02 domain available for comparison, while the purple crosses represent the 155 stations utilized for independent validation, which were not used in training the z_{0_RFR} model. (b) Comparison of mean 100-m wind speeds in April between the coarse-resolution (0.09°; d01) simulations using $z_{0\_Default}$, z_{0_Peng}, and z_{0_RFR} and observations from anemometer towers. The corresponding wind speed means, *R*, and *RMSE* of all stations are also indicated.

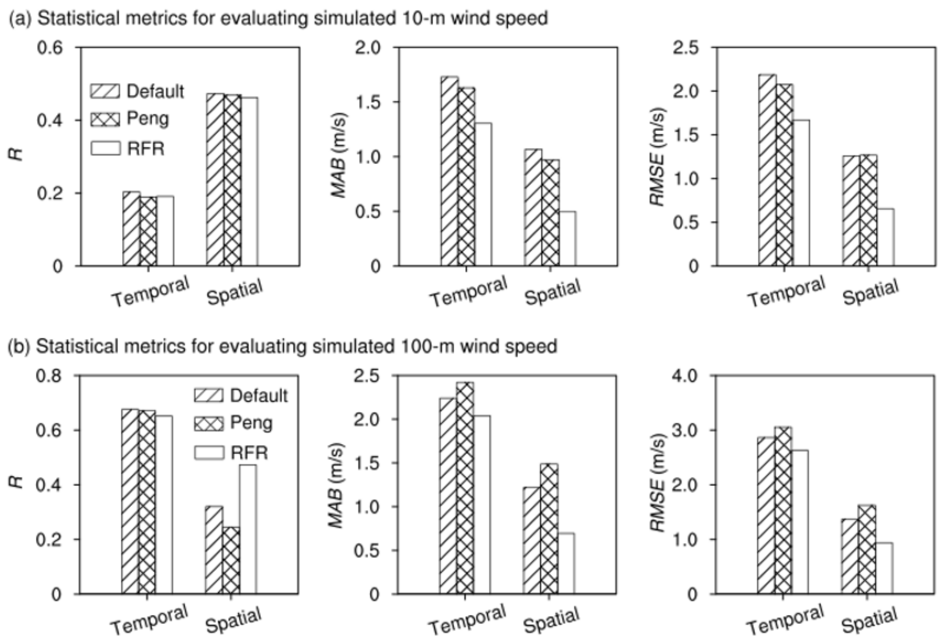

Figure R4. Statistical comparison of the coarse-resolution (0.09°; d01) simulations and observations within the d02 domain. (a) 10-m wind speeds from 753 CMA stations, and (b) 100-m wind speeds from 50 anemometer towers. Temporal and spatial *R*, *MAB*, and *RMSE* are included.

Based on the responses above, and to provide greater clarity for the readers, we have made the following additions to the manuscript:

(1) In the revised manuscript, we have added an explanation of *URC* importance in lines 277-288: "The *URC* ranks only fourth in feature importance. This ranking should not be interpreted as implying that land use or urbanization is insignificant. Rather, in our framework, *URC* is used mainly to delineate the study domain and to ensure that the RFR algorithm is applied only to high-roughness surface areas. The aerodynamic effects of high-roughness elements, such as tall vegetation, buildings, and other infrastructure, are already embedded in the wind observations from CMA stations. As a result, the influence of these roughness elements is directly reflected in the z_0 values themselves, rather than being captured by the *URC*. Essentially, *URC* is not defined in terms of the morphological height and density of roughness elements; instead, it is derived from global land-cover and population data (Li, X. et al., 2023), and is therefore weakly sensitive to z_0. For example, in categories of Urban center and Urban landscape,

there remains non-negligible tree cover, mean tree fractions of approximately 10% and 11%, respectively (Fig. S1b). This lowers *URC*'s ranking in feature-importance analyses. To better capture the influence of roughness elements, more detailed surface parameters, such as building height and building density, would be helpful. Once such data are widely accessible, they should be incorporated to further improve the accuracy of z_0 estimates." Figure R1 has been added to the Supplementary Materials as Fig. S1b.

(2) We have added a discussion on the resolution dependence that "Therefore, z_0 values estimated from 10-m wind observations are reasonably representative at ~100 m-1 km scales, making the generation of 0.01° gridded z_0 datasets for use in mesoscale simulations both appropriate and justified, with no evident resolution dependence observed. We compared simulation results at different resolutions. Leveraging the nested modeling setup used in this study, the d01 domain with a 0.09° resolution was treated as the coarse-resolution simulation, while d02 at 0.03° served as the fine-resolution simulation. The results show that, even at the coarser resolution, our gridded z_0 dataset provides a clear advantage and substantially improves near-surface wind speed simulations (Fig. S8 and S9)." in lines 417-422 of the revised manuscript, and added Figs. R2-R3 to the supplementary material as Figs. S8-S9.

(3) In Abstract, raw expression that "In built-up areas, surface roughness has been substantially altered by land use changes such as urbanization. However, many numerical models assign z_0 values based on vegetation cover types neglecting urban effects. This has resulted in a lack of reliable z_0 data in built-up regions." has been replaced by "In high-roughness surface areas, surface roughness has been substantially altered by land use changes such as urbanization. However, many numerical models still assign long-standing and fixed z_0 based on traditional land cover types, neither accounting for shifts in land cover nor updating class-specific z_0, leaving z_0 values in high-roughness surface regions outdated and unreliable."

2. Some figures use $\ln(z_0)$ as the parameter that's being shown: Fig. 4, Fig. 8, Fig 1b and 3c. This is not very intuitive for the reader since z_0 itself has units of meters and

a physical meaning. These figures would be much clearer if the z_0 value was shown and then the axes or colorbar were logged.

Response: Thank you for your valuable suggestion. In the revised manuscript, we have made the following changes accordingly. The axes in Figs. 3c and 8a-b have been changed to logarithmic scales, and Fig. 4 now presents the distribution of z_0 instead of $\ln(z_0)$, with an adjusted colorbar to better highlight the differences among datasets. However, we have retained $\ln(z_0)$ in Figs. 1b and 8c, because z_0 affects the wind profile in a logarithmic manner based on the Monin-Obukhov similarity theory. Therefore, comparing $\ln(z_0)$ values provides a more direct and physically meaningful interpretation of relative differences. For example, z_0 values of 0.01 m and 0.02 m differ by 0.01, while 0.001 m and 0.002 m differ by 0.001, but both pairs have the same $\ln(z_0)$ difference (−0.69), indicating comparable aerodynamic impacts.

3. Along those same lines, any time $\ln z_0$ is used it should be $\ln(z_0)$, this would improve the clarity of the manuscript significantly.

Response: Thank you for your suggestion. We have carefully checked the entire manuscript and revised all occurrences of "$\ln z_0$" to the correct format "$\ln(z_0)$" to improve clarity and consistency.

4. I had a similar comment as one of the other reviewers regarding the circular logic in using ERA5 data to derive z_0. I believe other readers would question this, as well. The findings in the supplementary confirm that there is improved agreement with the NCEP data. I think those findings should be included more as an appendix in the manuscript rather than supplementary material. I believe the additional discussion section that was added could probably move to the appendix along with the relevant supplementary material, but that is ultimately up to the authors to decide.

Response: We appreciate your comment. We fully agree that the cross-validation using NCEP data is important to demonstrate the robustness of the refined z_0 dataset. However, since these results serve as supporting evidence rather than the main focus of this study, we consider it is more appropriate to keep them in the supplementary material. We have ensured that the main text clearly refers to these supplementary results for readers who wish to examine the validation details.

5. Lastly, I think the error metrics should all be defined with their equations in Section 2.5. For example, when the authors restate MBP in the captions, the equations being inline make them difficult to read.

Response: Thank you for your valuable suggestion. In the revised manuscript, the definitions of all error metrics have been moved to Section 2.5. The definitions that were previously included in figure and table captions have been removed to improve readability and clarity.

6. Line 46: remove "of"

Response: Thank you for pointing this out. The word "of" has been removed as suggested in line 48 of the revised manuscript.

7. Line 210: change "confirms the reasonableness of the $z_{0\_optimal}$" to "confirms that $z_{0\_optimal}$ is reasonable".

Response: Thank you for the suggestion. We have revised the text accordingly to "supports that $z_{0\_optimal}$ is reasonable" in lines 246-247 of the revised manuscript.

8. Fig 3a does not have a fully white or transparent background. Additionally, Fig. 3d doesn't need a grid.

Response: Thank you for the comment. In the revised manuscript, we have updated Fig. 3a to have a fully white background, and the grid in Fig. 3d has been removed as suggested.

9. Line 358: I'd suggest rewriting to avoid using representativeness.

Response: Thank you for the suggestion. We have revised the sentence to avoid using "representativeness." The sentence in lines 413-415 of the revised manuscript now reads: "Moreover, the agreement between ERA5- and WRF-derived z_0 values suggests that the spatial extent represented by the estimated site-level z_0 values is not determined by the resolution of the reanalysis or simulation dataset used, but rather by the measurement height of wind observations at the stations."

Reference

Li, X., Yu, L. and Chen, X.: New insights into urbanization based on global mapping and analysis of human settlements in the rural-urban continuum, Land, 12, 1607, doi:10.3390/land12081607, 2023.

Reviewer #2:

General Comments:

This manuscript introduced an approach to estimate roughness length at CMA weather stations (z_{0_CMA}) that minimizes differences in 100-m wind speed (u_{100}) between ERA5 (u_{100_ERA5}) and CMA (u_{100_CMA}) stations (Equations 2 and 3), using the wind profile described by Monin-Obukhov similarity theory flux-profile relationship in neutral conditions (Equation 1). They assumed differences in near-surface wind speed between ERA5 and CMA stations are mainly due to z_0 and the influence of z_0 diminishes with height (i.e., $u_{100\_ERA5} \sim u_{100\_CMA}$) (assumption 1), and the impact of atmospheric stability on wind speed is identical between ERA5 and CMA (assumption 2) (Lines 122-125). The estimated station-wise z_0 (z_{0_CMA}) was then used to derive a gridded z_0 dataset using a random forest regression algorithm, i.e., z_{0_RFR}, which improved near-surface wind simulations in the WRF model compared to those simulations with other static z_0 values.

This manuscript is well written and organized. This manuscript not only provides a method to estimate roughness length at measurement stations but also suggests a potential way to provide a gridded dataset that can be applied to numerical simulations, which well fits the scope of this journal. I do not 100% agree with the authors on the two assumptions that they used to derive z_{0_CMA} (assumptions 1 and 2 above). I think impacts of the validity of these two assumptions are topics to be discussed and further studied (it would be good if this issue is briefly discussed in the manuscript), but they don't need to be addressed in the current manuscript. Below I have several minor comments and suggestions.

Response: We sincerely appreciate your positive comments regarding the manuscript's clarity, organization, and the proposed methodology. We also thank you for highlighting the two assumptions used to derive z_{0_CMA}. We acknowledge that the two assumptions cannot be fully verified with the available data. However, adopting the z_{0_CMA} values derived under them markedly improves WRF-simulated wind speeds compared with those using default z_0 values. Therefore, from a practical modeling perspective, these assumptions represent reasonable and effective approximation. We also provide further explanation and discussion of their rationality from the following aspects.

For Assumption 1, the original intent is: "(1) the near-surface wind speed difference between ERA5 and CMA is primarily attributed to z_0, and the influence of z_0 diminishes with height. Consequently, at higher levels within the near-surface layer, the wind speed from ERA5 reanalysis is considered comparable to that from observations;" And we selected 100 m as the analysis height for two reasons: (1) ERA5 provides near-surface wind speed only at 100 m, and (2) several anemometer towers we collected also include wind speed observations at 100 m, which can indirectly support the validity of the assumption. Specifically, the estimated z_{0_CMA} values under this assumption are mainly concentrated in eastern China, while those at most western stations are difficult to estimate accurately. This pattern is generally consistent with the observation that the bias between ERA5 and tower-measured 100-m wind speeds is much smaller in eastern China than in the west (Figs. 1c and 2a in the manuscript).

Additionally, we conducted a sensitivity experiment on the choice of reference height, re-estimating z_{0_CMA} using 150 m and 200 m (Fig. R1). As shown in Fig. R1 a and c, the annual-mean z_{0_CMA} derived from 150 m or 200 m is broadly consistent with that obtained at 100 m: for 88.6% (150 m) and 87.3% (200 m) of stations, the absolute difference from the 100-m-based estimate is less than 0.5. Stations that are more sensitive to the reference height may be influenced by local terrain complexity, as suggested in Fig. R1 b and d.

In short, both the model-improvement outcomes and the sensitivity-test results indicate that this assumption is reasonable and practically useful. Nevertheless, some deviations in the simulated wind speeds still exist. If more multi-height observational data become available in the future, it would allow testing alternative reference height assumptions and obtaining more precise z_0 estimates.

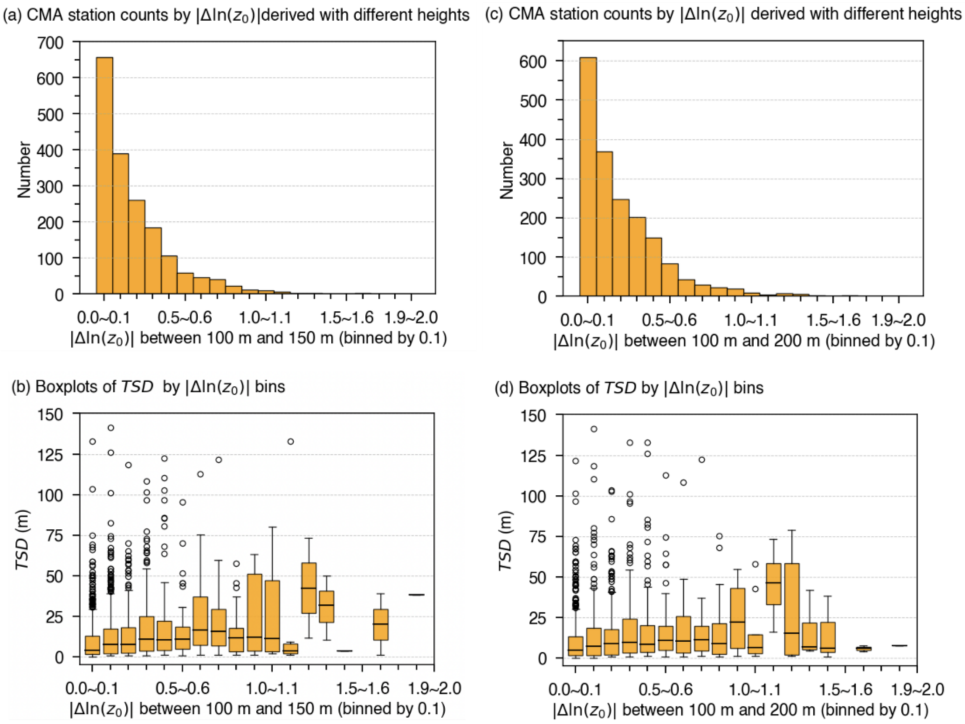

Figure R1. Sensitivity of the reference-height choice in Assumption 1 for z_{0_CMA} estimation. (a, c) Distributions of station counts by the absolute difference in annual-mean z_{0_CMA} values between estimates using 150 m (a) or 200 m (c) and those using 100 m. (b, d) Boxplots of *TSD* across bins of the absolute difference in annual-mean derived $ln(z_0)$. (b, d) Boxplots of *TSD* versus binned absolute differences in annual-mean z_{0_CMA}, computed relative to the 100-m–based estimate for 150 m (b) and 200 m (d).

For Assumption 2, we assume that the impact of atmospheric stability on wind speed is identical for both ERA5 and CMA stations, allowing us to neglect explicit stability corrections when estimating z_{0_CMA}. In the Monin-Obukhov similarity framework, the stability correction term is generally smaller in magnitude than the logarithmic term. Moreover, when estimating z_{0_CMA}, the stability correction term appears in both the numerator and the denominator of the governing equation. Therefore, their effects can be considered to approximately cancel each other out. This simplification is reasonable from both an efficiency and consistency standpoint, as also supported by the validation of simulated wind speeds. Additionally, Duplyakin et al. (2021) have shown that incorporating stability corrections into vertical interpolation of wind profile does not necessarily improve accuracy. They compared multiple interpolation schemes in U.S. wind resource assessments and found that neutral log-law method performed comparably to stability-corrected version. This also supports that such an approximate treatment seems feasible and a widely adopted simplification.

Based on the above, we have added a brief discussion about the two assumptions in Section 4. Discussion of the revised manuscript (lines 448-465): "The two assumptions used in the z_0 estimation are also discussed. Although these assumptions cannot be fully verified with the available data, they are pragmatically motivated and indirectly supported by the improved performance of wind-speed simulations using the resulting z_0 estimates. Assumption 1 posits that the near-surface wind-speed discrepancy between ERA5 reanalysis and CMA observations is dominated by z_0 and that the influence of z_0 weakens with height, making ERA5 winds at higher levels within the surface layer comparable to observations. This is partly supported by the spatial pattern of estimated z_0 (denser over eastern China, where 100-m wind-speed biases between ERA5 reanalysis and anemometer tower observations are smaller (Figs. 1c and 2a)) and by a sensitivity test on the reference height (Figs. S11a and S11c). When re-estimating annual-mean z_{0_CMA} at 150 m and 200 m, 88.6% and 87.3% of stations, respectively, show an absolute difference from the 100-m–based estimate below 0.5, indicating broad consistency across heights. A minority of stations exhibit larger deviation, which may be influenced by local terrain complexity (Figs. S11b and S11d). Assumption 2 treats the effects of atmospheric stability on wind speed as effectively similar in ERA5 and at CMA sites, allowing us to omit explicit stability corrections in estimating z_{0_CMA}. This simplification enhances methodological consistency and computational efficiency, and it is indirectly supported by the validation of simulated winds. Moreover, prior work has shown that neutral log-law method can perform comparably to stability-corrected scheme for vertical interpolation in U.S. wind-resource assessments (Duplyakin et al., 2021), suggesting that such an approximate treatment seems feasible and a widely adopted simplification. Overall, although neither assumption can be fully verified with the presently available data, their practical applicability is evidenced by improved WRF wind-speed simulations. Future work, ideally leveraging multi-height wind profile observations and coincident stability metrics could further test these assumptions, yield more precise z_0 estimates." Figure. R1 has been added to the supplementary material as Fig. S11.

We have also carefully considered your following suggestions, which have substantially improved the clarity and rigor of the manuscript. Detailed, point-by-point responses to each comment are provided below.

Specific comments:

1. Lines 92-93, "variance of the slope": Could you clarify what data you used to derive this variable with its spatial resolution (e.g., 3 arcsec SRTM)?

Response: Thank you for the question. The variance of the slope ($\overline{\theta^2}$) used in our study was sourced from the dataset accompanying the turbulent orographic form drag scheme in WRF (Zhou et al., 2018). This dataset was processed from the global 30″ GMTED2010 digital elevation model (Danielson & Gesch, 2011; ~1 km nominal spacing at the equator). To clarify this in the manuscript, we added the following sentence that "We obtained $\overline{\theta^2}$ from the dataset accompanying the turbulent orographic form drag scheme in WRF (Zhou et al., 2018), which was processed from the global 30″ GMTED2010 digital elevation model (Danielson & Gesch, 2011)." in lines 102-104 of the revised manuscript.

2. Line 100, "z_0 dataset at a spatial resolution of 0.01°×0.01°": Why did you select this spatial resolution?

Response: Thank you for raising this point. We chose a spatial resolution of 0.01°×0.01° for two reasons: (1) Our objective is to develop a z_0 dataset suitable for mesoscale modeling with kilometer-level resolutions; (2) The "rule of thumb" for near-surface wind observations is that the horizontal representativeness is roughly 10-100 times the measurement height. Since our z_0 truth values are inferred from 10-m CMA winds, the representative footprint is on the order of ~100m-1 km, which is well matched by a

0.01° grid. Thus, constructing a 0.01° gridded z_0 dataset is both appropriate and justified, which was explained in lines 89-91 and 417-418.

However, this does not imply that the wind-speed simulations necessarily exhibit any resolution dependence. To address this (also raised by Reviewer 1), we tested the performance at different model resolutions using our nested setup: the outer domain (d01, 0.09°) as a coarse-resolution case and the inner domain (d02, 0.03°) as a fine-resolution case. Across both nests, the z_0 dataset delivers clear and consistent improvements in near-surface wind simulations (Figs. R2-R3), indicating no evident resolution dependence within the mesoscale range considered. These results support that a 0.01° dataset is suitable and effective for kilometer-scale WRF applications. We have added the supporting multi-resolution comparison to the revised manuscript in lines 417-422: "Therefore, z_0 values estimated from 10-m wind observations are reasonably representative at ~100 m-1 km scales, making the generation of 0.01° gridded z_0 datasets for use in mesoscale simulations both appropriate and justified, with no evident resolution dependence observed. We compared simulation results at different resolutions. Leveraging the nested modeling setup used in this study, the d01 domain with a 0.09° resolution was treated as the coarse-resolution simulation, while d02 at 0.03° served as the fine-resolution simulation. The results show that, even at the coarser resolution, our gridded z_0 dataset provides a clear advantage and substantially improves near-surface wind speed simulations (Fig. S8 and S9)."

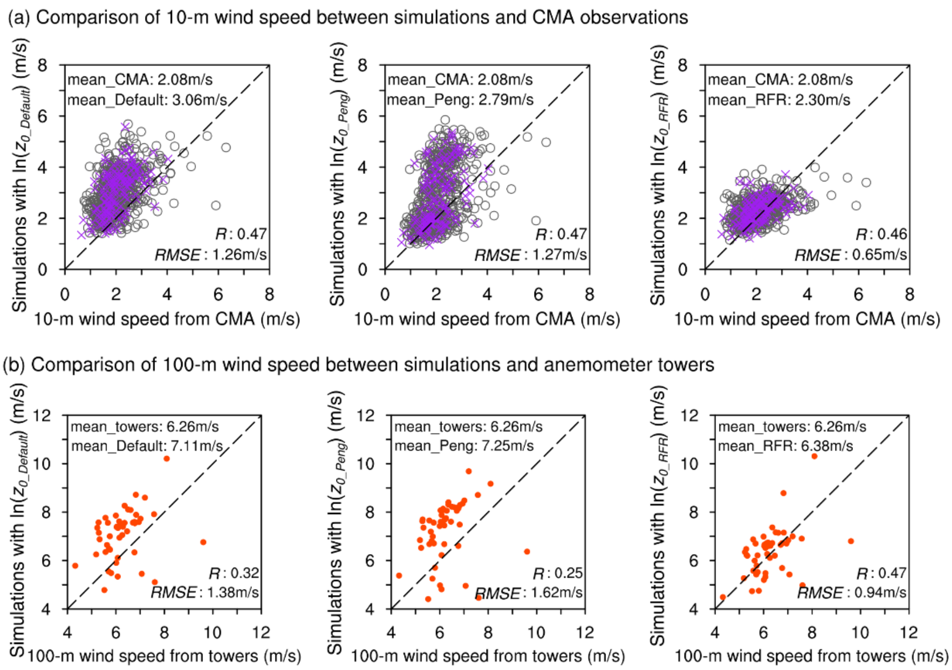

Figure R2. (a) Comparison of mean 10-m wind speeds in April between the coarse-resolution (0.09°; d01) simulations using $z_{0\_Default}$, z_{0_Peng}, and z_{0_RFR} and observations from CMA stations. All points (grey circles and purple crosses) represent the 753 CMA stations within the d02 domain available for comparison, while the purple crosses represent the 155 stations utilized for independent validation, which were not used in training the z_{0_RFR} model. (b) Comparison of mean 100-m wind speeds in April between the coarse-resolution (0.09°; d01) simulations using $z_{0\_Default}$, z_{0_Peng}, and z_{0_RFR} and observations from anemometer towers. The corresponding wind speed means, *R*, and *RMSE* of all stations are also indicated.

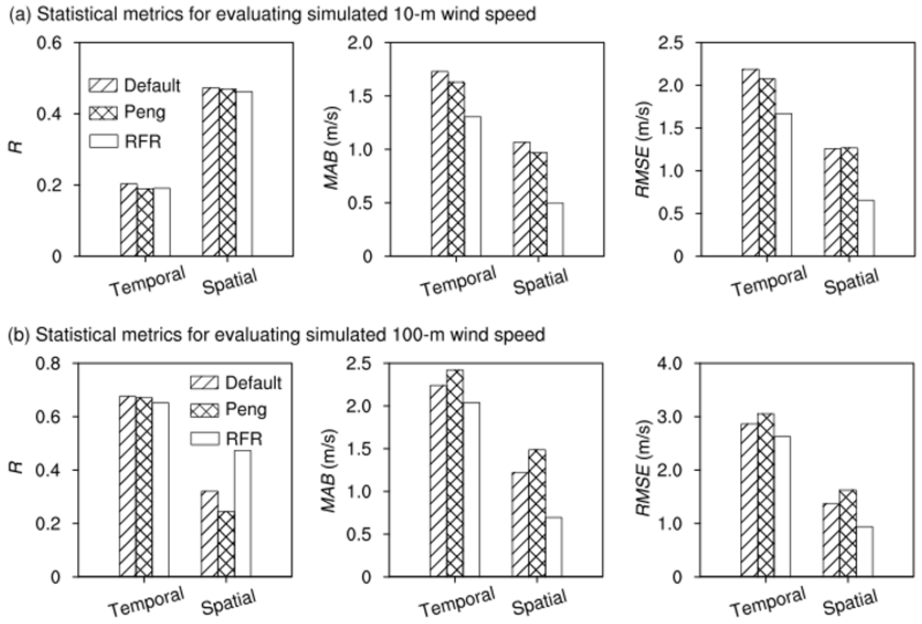

Figure R3. Statistical comparison of the coarse-resolution (0.09°; d01) simulations and observations within the d02 domain. (a) 10-m wind speeds from 753 CMA stations, and (b) 100-m wind speeds from 50 anemometer towers. Temporal and spatial *R*, *MAB*, and *RMSE* are included.

3. Line 206, "all stations are situated in build-up areas": Not "all" stations seem to be situated in build-up areas. Figure 2c shows there are some stations at croplands categories. Am I missing something?

Response: Thank you for pointing this out. You are correct that not all stations lie in what is commonly called "built-up areas." In our original manuscript, we used "built-up areas" as shorthand for the urban-rural classifications of Li et al. (2023) that define our study area, including Urban center, Urban landscape, Densely clustered towns, Sparsely clustered towns, Dense villages, Sparse villages, Isolated villages, Residential croplands, Populated croplands, Residential rangelands, and Residential woodlands. As you pointed, several of these classes encompass croplands and woodlands, even those labeled "Populated" or "Residential", so "built-up" is not an accurate descriptor. To more accurately reflect the surface characteristics relevant to our study, we have replaced "built-up areas" with "high-roughness surface areas" throughout the manuscript. This term is intended to encompass both urban/town environments and landscapes with tall vegetation (e.g., residential croplands and woodlands) that imply relatively large z_0. These wording changes are terminological clarifications only and do not affect our analysis or conclusions.

4. Lines 210-211, "the robust consistency in the relationship between z_0 and wind speed confirmed the reasonableness of $z_{0\_optimal}$": I think the robust consistency in the relationship between $z_{0\_optimal}$ and wind speed is an expected outcome because you used equation 1, which relates z_0 and wind speed, to derive z_{0_CMA}, which was in turn used to derive $z_{0\_optimal}$. In that regard, I'm not sure how the robustness relationship between z_0 and wind speed can be related to the reasonableness of $z_{0\_optimal}$.

Response: Thank you for your careful reading and for raising this point about Fig. 1d. We agree that, if one looks only at Eq. (1) (in the manuscript) at a single time step, a strong z_0-wind-speed relationship is expected and should not be taken as proof of the validity of $z_{0\_optimal}$. Our intention with Fig. 1d was not to use that relationship as an independent validation, but rather as a consistency check: to verify that, under our statistical aggregation procedure, the derived $z_{0\_optimal}$ varies with wind-speed bias in the expected manner.

Specifically, for each station we estimated instantaneous $\ln(z_0)$ at each hour using Eqs. (2)-(3) (in the manuscript), and then defined the station's monthly $\ln(z_{0\_optimal})$ as the median of all hourly values within that month. Because the $\ln(z_{0\_optimal})$ is a cross-time statistic (median) rather than a direct substitution at any single time, the instantaneous implication from Eqs. (2)-(3), namely, "if $u_{10\_ERA5} > u_{10\_CMA}$ then $z_{0\_ERA5} < z_{0\_CMA}$", does not necessarily persist after aggregation. We therefore relate binned difference of $\ln(z_0)$ to the mean bias percentage of 10-m wind speed in Fig. 1d to check whether the monotonic, theory-consistent pattern remains after aggregation, rather than to "prove" $z_{0\_optimal}$. The reasonableness of $z_{0\_optimal}$ is assessed through independent validation by comparing 10- and 100-m wind speeds simulated based on $z_{0\_optimal}$ against observations (Figs. 6-7 and Table. 1 in the manuscript).

To avoid any impression of circular validation, we have revised the relevant passages in the manuscript accordingly to: "Additionally, as a consistency check, we examined how the difference in $\ln(z_0)$ covaries with the 10-m wind-speed bias between ERA5 reanalysis and station observations." in lines 231-232 and "Because $\ln(z_{0\_optimal})$ is defined as a monthly median of hourly $\ln(z_0)$, this cross-time statistic does not trivially inherit the instantaneous relationship implied by Equations (1)-(3). The monotonic, theory-consistent pattern observed in the binned $\ln(z_0)$ difference versus wind-speed *MBP* therefore serves as a post-aggregation consistency check, rather than as proof. Accordingly, the robust consistency in the relationship between z_0 and wind speed preliminarily supports that $z_{0\_optimal}$ is reasonable, and suggests that improving z_0 values over high-roughness surface areas in numerical models could significantly enhance wind speed simulation accuracy. The validity of $z_{0\_optimal}$ will be assessed via independent validation by comparing simulated wind speeds with observations (Section 3.3)." In Lines 243-249.

5. Line 215, Figure 1: It is hard to read Figures 1a-1c. I think using color scales that are more consistent with numerical values (e.g., bluish/reddish colors for negative/positive values) would help.

Response: Thank you for the helpful suggestion. We have updated Figs. 1a-1c in the revised manuscript.

6. Line 230, "categores": "categories".

Response: Thank you for pointing this out. The typo has been corrected from "categores" to "categories".

7. Lines 333-334, "the resulting gridded z_0 dataset significantly reduces uncertainties ~ particularly over relatively flat built-up areas": The WRF model has various urban canopy model (UCM) options, which parameterize effects of urban topography by updating surface drag etc. This includes for example, a single-layer UCM (WRF UCM option 1), a building effect parameterization (BEP; WRF UCM option 2), etc. Considering the impact of the updated roughness length dataset is mainly over urban

areas, could you explain the advantage of using the updated dataset instead of using an UCM? Also, could you compare the impact of $z_{0\_optimal}$ with the impact of using UCMs?

Response: Thank you for the thoughtful question. It likely stems from our potentially misleading use of the term "built-up". We have revised the terminology, replacing "built-up areas" with "high-roughness surface areas". Accordingly, our updated roughness-length dataset applies not only to urban areas. Additionally, our study targets mesoscale wind simulations at kilometer-scale grid spacing (~1-10 km). We are less familiar with UCMs, but our review confirms that UCMs and our z_0 dataset are designed for different purposes and are suited for different simulation scales. UCMs were conceived to operate at ~0.5-1 km grid spacing to bridge mesoscale forecasting (~105 m) with microscale transport/dispersion (~100 m) models (Tewari et al., 2006; Chen et al., 2010), and they are most commonly applied at ~1 km resolution (Lian et al., 2018; Salamanca et al., 2018; Wang et al., 2021). At our target kilometer-scale resolutions, our z_0 dataset offers a more practical and effective path for improving near-surface wind simulations for the following three reasons. First, regarding parameter dependence, our approach bypasses the need for land-use classification, sources of error that directly impact UCMs performance. Second, in terms of computational cost, our method is highly efficient, avoiding the expensive prognostic calculations required by UCMs such as BEP. Third, for model configuration, our solution offers greater flexibility and portability, as it does not impose the specific PBL and LSM combinations that some UCMs demand.

Therefore, a direct comparison of wind speed simulations using our z_0 dataset and UCMs within the scope of this work is not undertaken for two primary reasons. First, as our expertise and research focus lie in mesoscale simulation, we are less familiar with the specific application of UCMs. More importantly, these approaches are designed for fundamentally different purposes. Our z_0 dataset is tailored for efficient mesoscale wind simulation at kilometer-scale resolutions, whereas UCMs are the preferred and more specialized tool for investigating detailed urban canopy effects, such as building-induced drag and urban thermodynamics.

Based on the above, we have added a brief discussion of the applicable spatial resolution of our z_0 dataset in lines 423-429 of the revised manuscript: "However, for simulations at ~1 km resolution and finer, such as urban-scale wind modelling, our z_0 dataset cannot fully capture urban heterogeneity, because it did not incorporate key morphological parameters (e.g., building height and density) to distinguish between different urban forms. Therefore, an urban canopy model (UCM) would be a more appropriate choice. UCMs were conceived to operate at ~0.5-1 km grid spacing to bridge mesoscale forecasting (~10^5 m) with microscale transport/dispersion (~10^0 m) models (Tewari et al., 2006; Chen et al., 2010), and they have been widely applied and validated in subsequent urban studies (Lian et al., 2018; Salamanca et al., 2018; Wang et al., 2021). Therefore, our z_0 data are suitable and effective for mesoscale simulations at kilometer-level resolutions."

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Reviewer #3:

General Comments:

This manuscript addresses an important challenge in meteorology and wind energy applications. The authors propose a cost-effective method to estimate aerodynamic roughness length at CMA weather stations by reconciling discrepancies between ERA5 reanalysis and surface observations, then extend these estimates to a gridded dataset using Random Forest Regression. The refined dataset is implemented in WRF and compared with default and alternative z0 datasets. Results show improvements in 10-m and 100-m wind simulations. The study is timely, practical, and demonstrates methodological robustness across reanalysis and meteorological conditions.

I appreciate the novelty and practicality of the proposed approach. The validation experiments convincingly show that the new dataset reduces biases in WRF simulations. To further strengthen the paper, I encourage the authors to enhance the methods section and provide additional clarifications.

Response: We would like to express our sincere gratitude for your positive feedback and insightful comments and suggestions. These have significantly enhanced the quality of our manuscript. We have carefully considered all your points. In the following sections, we provide a detailed response to each of your comments.

Specific comments:

1. If I understand correctly, the study reconstructs ERA5 100 m wind speeds from the log-law (Eq. 2-3) using 10 m winds and z_{0_ERA5}. However, ERA5 also provides 100 m wind speed as a native output. It would be useful to explicitly state this, clarify why the reconstructed values were preferred. It would also be helpful to evaluate the potential differences between the log-law–derived 100 m winds and native ERA5 100 m winds, and discuss any implications for the derived z_0.

Response: We thank you for raising this important point. Indeed, ERA5 provides native 100-m winds. Our choice to use the log-law-reconstructed 100-m winds from u_{10_ERA5}

and z_{0_ERA5} rather than the native ERA5 100-m winds can be justified from two following perspectives.

First, our $\ln(z_0)$ estimation relies on two assumptions that warrant equating Eqs. (2) and (3) for the optimization. Assumption 2 permits neglecting stability terms solely when Eqs. (2) and (3) are set equal, meaning ERA5 and CMA have identical stability and the effect cancels to some extent. This does not justify we can ignore the stability for wind speed calculations. However, the native ERA5 100-m wind inherently includes stability effects. Therefore, directly pairing native ERA5 100-m winds with our CMA log-law construction would amplify the error in the derived $\ln(z_0)$.

Second, our z_0 estimation method requires only 10-m wind speeds and z_0 from reanalysis, together with 10-m wind speeds from observations. It does not rely on reanalysis winds at other heights, which makes the approach low-cost and easily extensible. For Assumption 1, the original intent is: "(1) the near-surface wind speed difference between ERA5 and CMA is primarily attributed to z_0, and the influence of z_0 diminishes with height. Consequently, at higher levels within the near-surface layer, the wind speed from ERA5 reanalysis is considered comparable to that from observations;" And we selected 100 m as the analysis height for two reasons: (1) ERA5 provides near-surface wind speed only at 100 m, and (2) several anemometer towers we collected also include wind speed observations at 100 m, which can indirectly support the validity of the assumption. Specifically, the estimated z_{0_CMA} values under this assumption are mainly concentrated in eastern China, while those at most western stations are difficult to estimate accurately. This pattern is generally consistent with the observation that the bias between ERA5 and tower-measured 100-m wind speeds is much smaller in eastern China than in the west (Figs. 1c and 2a in the manuscript). Additionally, to illustrate the dependence on height selection, we conducted a sensitivity experiment, re-estimating z_{0_CMA} using 150 m and 200 m (Fig. R1). As shown in Fig. R1 a and c, the annual-mean z_{0_CMA} derived from 150 m or 200 m is broadly consistent with that obtained at 100 m: for 88.6% (150 m) and 87.3% (200 m) of stations, the absolute difference from the 100-m-based estimate is less than 0.5.

Stations that are more sensitive to the reference height may be influenced by local terrain complexity, as suggested in Fig. R1b and d.

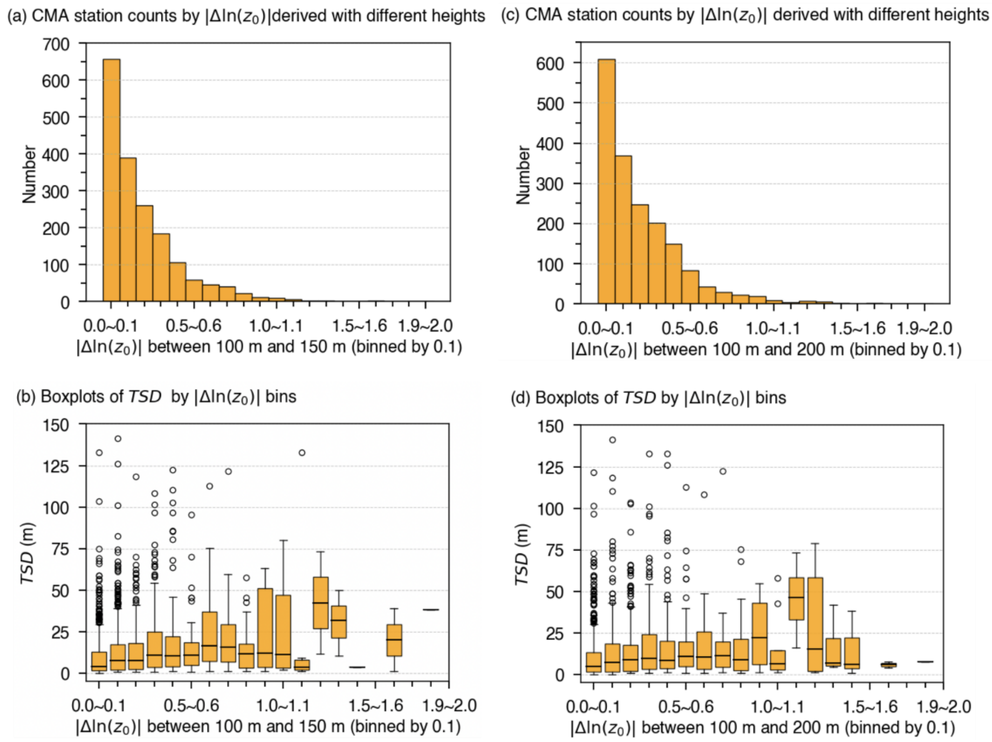

Figure R1. Sensitivity of the reference-height choice in Assumption 1 for z_{0_CMA} estimation. (a, c) Distributions of station counts by the absolute difference in annual-mean derived z_{0_CMA} between estimates using 150 m (a) or 200 m (c) and those using 100 m. (b, d) Boxplots of *TSD* across bins of the absolute difference in annual-mean derived $ln(z_0)$. (b, d) Boxplots of *TSD* versus binned absolute differences in annual-mean z_{0_CMA}, computed relative to the 100-m–based estimate for 150 m (b) and 200 m (d).

In summary, lines 142-150 of the revised manuscript now clearly explain why we prefer the reconstructed values and the advantages of this approach: "Actually, ERA5 provides native 100-m winds, but here we use log-law–reconstructed 100-m winds from u_{10_ERA5} and z_{0_ERA5} instead. The reason is that the z_{0_CMA} is derived under the assumption that stability-correction term is neglected. This means that the 100-m wind speeds in Equations (2) and (3) are both calculated without considering stability effects. However, the native ERA5 100-m wind field inherently embeds model-diagnosed stability influences. Therefore, directly pairing native ERA5 100-m winds with our

CMA log-law construction would amplify the error in the derived $\ln(z_0)$. In addition, the reconstruction offers two practical advantages. First, it requires fewer variables and a more transparent linkage, relying only on 10-m wind speeds and z_0 from reanalysis, together with 10-m wind speeds from observations; Second, our results indicate that the z_0 estimates are not particularly sensitive to the choice of reference height (see Section 4. Discussion), so there is no need to use native reanalysis winds at heights other than 10 m."

Details of the sensitivity to the choice of reference height are reported in lines 448-457: "The two assumptions used in the z_0 estimation are also discussed. Although these assumptions cannot be fully verified with the available data, they are pragmatically motivated and indirectly supported by the improved performance of wind-speed simulations using the resulting z_0 estimates. Assumption 1 posits that the near-surface wind-speed discrepancy between ERA5 reanalysis and CMA observations is dominated by z_0 and that the influence of z_0 weakens with height, making ERA5 winds at higher levels within the surface layer comparable to observations. This is partly supported by the spatial pattern of estimated z_0 (denser over eastern China, where 100-m wind-speed biases between ERA5 reanalysis and anemometer tower observations are smaller (Figs. 1c and 2a)) and by a sensitivity test on the reference height (Figs. S11a and S11c). When re-estimating annual-mean z_{0_CMA} at 150 m and 200 m, 88.6% and 87.3% of stations, respectively, show an absolute difference from the 100-m–based estimate below 0.5, indicating broad consistency across heights. A minority of stations exhibit larger deviation, which may be influenced by local terrain complexity (Figs. S11b and S11d)."

2. The method assumes that stability impacts are identical in ERA5 and CMA observations and thus neglects stability corrections. While this simplification is reasonable for efficiency and consistency, ERA5 simulated stability itself may be biased, potentially introducing additional uncertainty. Previous studies evaluating vertical interpolation methods (e.g., the NREL report by Duplyakin et al., 2021, https://www.nrel.gov/docs/fy21osti/78412.pdf) found that simple neutral log-law

interpolation often performs best in U.S. wind resource assessments. Referencing this evidence would strengthen the justification for the assumption.

Response: We appreciate your helpful suggestion. To strengthen the justification for Assumption 2, we have added a citation to Duplyakin et al. (2021) in lines 457-462: "Assumption 2 treats the effects of atmospheric stability on wind speed as effectively similar in ERA5 and at CMA sites, allowing us to omit explicit stability corrections in estimating z_{0_CMA}. This simplification enhances methodological consistency and computational efficiency, and it is indirectly supported by the validation of simulated winds. Moreover, prior work has shown that neutral log-law method can perform comparably to stability-corrected scheme for vertical interpolation in U.S. wind-resource assessments (Duplyakin et al., 2021), suggesting that such an approximate treatment seems feasible and a widely adopted simplification."

3. ERA5 ingests a wide range of observations, including surface and upper-air data. While 10 m or 100 m winds are not necessarily assimilated directly, the assimilation of pressure, temperature, and upper-level winds improves boundary-layer structure and indirectly benefits surface-layer winds. A short discussion of this point would emphasize the credibility of ERA5 data as the reference in the proposed method.

Response: Thank you for the suggestion. We have added a brief discussion to emphasize the credibility of ERA5 in lines 409-412 of the revised manuscript: "Meanwhile, although 10-m and 100-m winds over lands are not assimilated directly in ERA5, its 4D-Var system ingests a wide range of surface and upper-air observations that constrain boundary-layer structure and indirectly improve near-surface winds; this strengthens the credibility of using ERA5 as the reference field (Hersbach et al., 2020)."

4. The Introduction focuses strongly on the importance of z_0. in dense urban areas. However, Fig. 2d shows that the dataset also covers lower-density built-up regions and natural surfaces such as residential areas and woodlands. Extend the introduction to cover natural vegetation would strengthen the scope.

Response: Thank you for the helpful suggestion. First, we have replaced the term "built-up areas" with "high-roughness surface areas" throughout to reflect that our study spans both built-up regions and vegetated landscapes. Second, we refined the study-area expression and incorporated a concise description of natural-vegetation roughness effects. The specific changes are as follows:

Lines 31-33: "With the rapid advancement of urbanization and industrialization, human activities and energy use are increasingly concentrated along the settlement-landscape continuum (Liu et al., 2014), particularly in high-roughness areas such as built-up zones and inhabited vegetated landscapes."

Lines 48-52: "Specifically, most models, such as the widely used ECMWF Reanalysis v5 (ERA5), determine z_0 with long-standing and fixed values based on traditional land cover types. Such treatment fails to reflect the impact of transitions between surface types and changes in roughness elements within the same type, particularly the complexity of urban structures, thereby posing significant challenges for accurate wind speed simulation and prediction over high-roughness surface areas (Wang et al., 2024)."

Lines 52-54: "Numerous studies have demonstrated that the changes of z_0, caused by land use changes, particularly urbanization and industrialization, as well as deforestation and afforestation, significantly impacted wind speed."

Lines 59-62: "A similar mechanism operated in Canada. At Sudbury Airport (Ontario), 10-m wind speeds declined by ~34% during 1975-1995 mainly due to reforestation-induced increases in surface roughness (Tanentzap et al., 2007). These findings highlight the need to refine z_0 in models by incorporating the effects of high-roughness surface areas across urban-town settings and tall-vegetation landscapes."

Lines 64-65: "Winckler et al. (2019) showed that roughness changes are a primary control on deforestation's biogeophysical effects, notably surface temperature responses."

5. Since natural regions are included, vegetation phenology (e.g., foliage status) could influence z_0. The October case therefore provides a meaningful seasonal contrast to

April and would be better discussed in the main text rather than only in the Supplementary Material.

Response: We appreciate your point that vegetation phenology can modulate z_0 and that October offers a meaningful seasonal contrast to April. Because the main text is already dense and our primary contribution is the method development and wind improvement, we keep the figures about October in the Supplementary Material. However, we have added a comparison of performance across April and October in lines 386-390 of the revised manuscript, as follows: "Station-wise correlations increase and errors decrease to a similar extent in both months, and the daily time series likewise show closer tracking of peaks and lulls. Taken together, these results further reinforce the reliability and applicability of the proposed z_0 estimation under varying meteorological conditions. They also indicate that although phenology-driven changes in canopy structure and seasonal circulation modulate wind speeds, the performance advantage of the proposed z_0 is not diminished."

6. The feature importance results (Fig. 3e) are interesting, especially the dominance of topographic predictors relative to vegetation metrics. It is somewhat surprising that *LAI* appears less important, given that leaf phenology can strongly influence roughness. This may result from collinearity with *NDVI*, which can bias feature importance rankings, or from averaging across all regions, thereby masking deciduous-seasonal effects. An extended discussion of this result would be helpful. Alternatively, if the authors feel the analysis adds little value, it could be streamlined.

Response: Thank you for your insightful question. z_0 is primarily determined by the characteristic height of surface roughness elements, particularly their relief. As a result, topographic features rank among the most important factors. For vegetation characteristics, PTC not only captures the horizontal distribution of vegetation density but also serves as a proxy for the presence of tall roughness elements. However, LAI mainly reflects vegetation density, making it relatively less critical.

To assess the role of collinearity, we first note that *LAI* and *NDVI* are moderately correlated across the 1,805 stations ($R = 0.72$). And We conducted a post-hoc permutation test on the test subset by randomly shuffling *NDVI*, which has reduced R^2 by 3.2% and increased *RMSE* by 6.2%. Taking the RFR model used in this study as the baseline, we conducted an ablation study by retraining two control models with the same data splits and hyperparameters, removing *LAI* in one and *NDVI* in the other. In both cases, test performance was broadly similar to the baseline (Table R1). When *NDVI* was removed, the impurity importance of *LAI* still ranked below topographic predictors and *PTC* (Fig. R2). Therefore, these results indicate that the relatively low impurity-based importance of *LAI* is not primarily driven by collinearity of *LAI* and *NDVI*.

We have added a discussion of *LAI* importance in lines 273-277 of the revised manuscript: "z_0 is primarily controlled by the characteristic height of surface roughness elements, particularly their relief. Consequently, topographic features rank among the most influential factors. For vegetation-related features, *PTC* not only reflects the horizontal distribution of vegetation density but also serves as a proxy for the presence of tall roughness elements. By contrast, *LAI* mainly represents vegetation density, making it relatively less critical. Although *LAI* is strongly correlated with *NDVI* ($R = 0.72$), its low importance is not driven by this collinearity."

Table R1. Test-subset performance of the Random Forest Regression (RFR) under baseline and ablation settings. The baseline refers to the raw RFR model used in our study trained with all features. Two ablation cases are retrained with identical splits and hyperparameters, excluding *LAI* in one case and *NDVI* in the other.

	Baseline	*NDVI*-excluded	*LAI*-excluded
R	0.90	0.90	0.90
RMSE	0.11	0.11	0.12

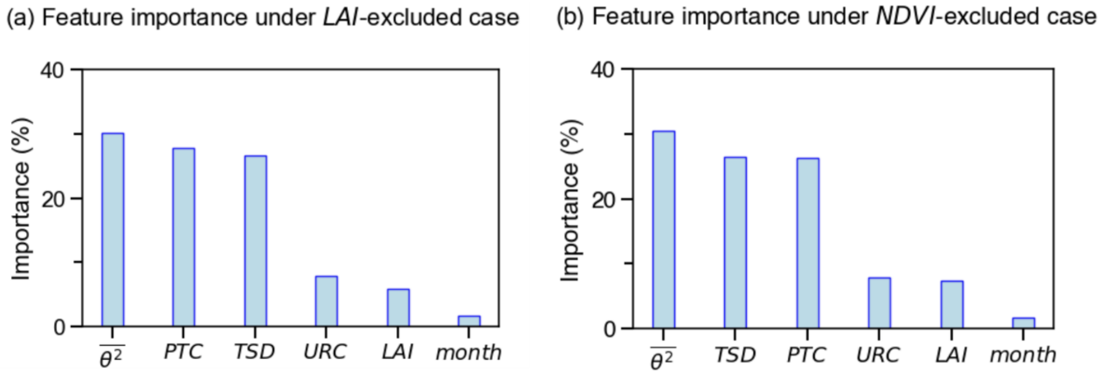

Figure R2. Importance scores of feature variables under *NDVI*-excluded case and *LAI*-excluded case.

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