Review of "Improvement of near-surface wind speed modeling through refined aerodynamic roughness length in built-up regions: implementation and validation in the Weather Research and Forecasting (WRF) model version 4.0"

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 reanalyses 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.

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 $z_{0,\mathrm{ERA5}}$. 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.
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.
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.
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.

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.
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.