

## Summary

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.

## General 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 4<sup>th</sup> 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 inline 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  $z_0$  values. Or, considering that the authors already ran a multiscale setup, they could compare results between d01 and d02.

2. Some figures use  $\ln(z_0)$  as the parameter that's being shown: Fig. 4, Fig. 8, Fig 1b and 1c. 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.
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.
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.
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.

### **Specific comments**

- Line 46: remove "of"
- Line 210: change "confirms the reasonableness of the  $z_0$ \_optimal" to "confirms that  $z_0$ \_optimal is reasonable".
- Fig 3a does not have a fully white or transparent background. Additionally, Fig. 3d doesn't need a grid
- Line 358: I'd suggest rewriting to avoid using representativeness